





S. 455.

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BY WILLIAM NICHOLSON.

V O L. I.

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P R E F A C E.

THERE is scarcely a more difficult task than to convey an adequate notion of a plan of some extent, within the limits of a short discourse. Whatever may be the promises, the hopes, or the intentions of an Author or Editor, the world, as in justice it ought, will suspend its judgment till the actual performance shall afford the knowledge which is indispensable for that purpose. Correspondents will arrange for themselves such materials as they think fit to publish, without committing to memory any of the outlines which the authors of Journals may have drawn up. For these reasons, little more need be said concerning the plan, than is presented in the title-page. Whatever the activity of men of science or of art may bring forward, of invention or improvement, in any country or nation, within the possibility of being procured, by means as respectable as the motives that call for them, shall appear in this Journal; either in the form of short notices, or the full descriptions of their respective authors, or the more ample report deduced from actual visitation and enquiry. The relative magnitude of each object will establish the rule from which either of these modes will be adopted. Arrangements have already been made, channels of communication opened, and other correspondences are in prospect, which must increase, in value and extent, proportionate to the importance and curiosity of the subjects to be displayed in this work, and the impartiality and care with which they shall be treated.

In a former Address to the Public the Author has mentioned the advantages in regard to accuracy and fidelity which he apprehends must result from the conductor of a work of this nature becoming, in a certain degree, responsible by name for its contents: he has alluded to the general tenor of his pursuits, as known to the Public, and in some respect qualifying him to engage in such a task; and he has deprecated the supposition of any vain pretence to superiority, by remarking, that no one could serve the Public in this way if he were to wait with the absurd hope of first bringing his own knowledge to a state of perfection. This is all that need be said relating to himself.

The leading character on which the selection of objects will be grounded is utility; and next to this, novelty and originality. The Author's researches and collections, and those of his friends, will afford a considerable portion of new and curious matter, sufficient to render the work interesting, even to that extreme few who are so fortunate as to have access to all the expanded sources of philosophical intelligence. But in the department of perfectly original matter, much of prudence is required to be exercised, in order that the claim of novelty may not operate to the exclusion of much more valuable and important subjects. It is certain that, if every article in a journal of science were to be professedly original*, it would be a work of comparatively much less value to Philosophers and the Public. Such a plan would in a great measure defeat the attempt to convey the best discoveries of our contemporaries in the most authentic manner, namely, in their own words. And when we reflect

* Lewis's Philosophical Commerce of Arts, and various other publications of inferior note, have failed of public support, chiefly from this circumstance in their plan.

on the very limited circulation of academical Transactions, from their price, their number, their extent, distance of publication, difference of language, labour of perusal, and the efforts of mental abridgment, it is also certain that, from one or other of these causes, even the best memoirs they contain must continue unknown to a very large class of men of science. Under the impression of these truths, while no exertions will be spared to obtain immediate original information, concerning any object presented to the world in this collection, the aim at originality must nevertheless be subordinate to the less easy but more essential requisites of public utility and interesting research. Whenever, in the progress of investigation, discoveries thus buried from the knowledge of the world, shall present themselves, the rational plan of a public journal will require them to be brought forward, though years may have elapsed since their first publication. It would be easy to exhibit a numerous catalogue of errors retained in the works of authors of the first eminence, from the want of such general communication.

After this short account of the materials, it might be expected that something should be said of the manner in which this Journal is to be conducted. On this occasion it would not be difficult to point out imperfections in the works of others, and promise to avoid them; or to enumerate the various requisites which ought to characterise the journalist of ability and integrity, and promise to exert them. Such specific engagements may have their value, and are probably entered into with great sincerity. But it appears more natural and easy to leave every individual of principle and understanding to imagine what ought to be done. As the events present themselves, the proper mode of conduct will itself stand forward and leave no cause for hesitation.

Yet, while the Author himself avows the decided purpose of exhibiting his sources of communication in the most unreserved manner, it is no less proper that he should pay every attention to the rights of others. He will never take upon him to decide for another, how far he shall or shall not come forward as the author of any communications he may receive, and still less will he dare to infringe that first and most sacred property which men hold in the products of their own understanding. It is not, in his opinion, for any man or set of men to decide what are the cases of moral obligation in an inventor to communicate his discoveries, or his private arrangements in business. Philosophers, manufacturers, and others, may therefore rest assured, that he will publish no communications he may acquire either in conversation or otherwise, nor mention the name of any individual in his writings; unless the object be already before the Public, or unless he shall receive permission directly, or most clearly implied, to that effect. But in every case of anonymous communication he will be careful, according to the nature of the object, to mention the degree of credit he himself is disposed to attach to the several facts.

Such papers as have no name or signature are written by himself. This will in general appear also from the manner, chiefly with respect to the use of the first person singular. The first person plural will probably be used in such occasional sentences as refer to the Author and the Reader in the joint consideration of any subject; but never in the manner adopted by anonymous writers, to denote the concealed individual.



ADVERTISEMENT.

LONDON, MARCH 1, 1798.

A YEAR has nearly elapsed since this Journal first appeared. A complete Volume is now before the Public ; on which occasion it seems necessary to mention the nature of its contents. Enough has already been said of the utility of such a Work and the duties of the Editor ; among which fidelity and accuracy are undoubtedly the chief. Whatever defects of ability may appear in the share I have had in this collection, I can with confidence assert my claim to those moral requisites. I have descended to none of the arts of Book-makers. No commendatory letters have flowed from my pen : no imaginary congratulations are echoed : no pretended success forms the subject of my acknowledgements. I have confided in the sincere performance of my engagement with the Public ; and have solicited the approbation of good men by such means only as my heart could thoroughly approve. I trust it will give pleasure to many of my Readers to hear that I have not been disappointed. The friendship and correspondence of men whose talents and virtues I revere, men whose approval constitutes the only estimable part of fame, have amply overpaid my exertions : and in a commercial view, though I have found the sale of my book unequal to what might have been expected in times of less general distress, yet it has been progressively increasing, and sufficient to encourage my perseverance.

The copious Table of Contents will render it unnecessary to recapitulate any of the excellent works I have received or collected. I have adhered to the principles of selection which are expressed in the Preface published last year, and have been more solicitous to offer productions of real merit and utility than such as were chiefly remarkable for their novelty. It will be seen, however, that nearly half the papers in this volume are original and interesting ; that above a third consists of new and important works which have never yet appeared in our language ; and that the remaining part consists either of digested reports and abridgments of excellent, but voluminous papers dispersed in academical collections, or such as from other circumstances deserved to be copied entire.

Men of information will hence perceive that this work is not an indiscriminate compilation of things nearest at hand, nor a loose temporary record of transactions which a few years must render worthy of the oblivion they will experience. It is reasonable to hope that it will every year become of more value, as the Repository of Discoveries in Science and the Arts ; and that it will tend to accelerate the progress of both.

ERRATA.

Page 13. l. 4. *r. other engagements.*

15. l. 27. *r. as in the figure; these lines were parallel to the threads of the cloth.*

17. l. 26. *r. Instrument. And in the plate certain letters are omitted, which however may be easily supplied from the text.*

64. l. 15. *r. 0.0166.*

66. l. 39. *r. one-third more.*

68. l. 10. *r. Figure 11.*

73. l. 15. *r. Market.*

87. l. 18. *r. $31\frac{1}{2}$. $7854 - 31 - 18^2$. $7854 = 522$.*

107. l. 8. of the note, *r. which are of the same nature.*

124. l. 2. *r. be divided the whole.*

139. ——— throughout the mathematical part, for

“*r. **”

144. l. 24. *r. Analysis.*

374. bottom line, *r. eighteenth Volume.*

Page 186. l. 5. of the Mathematical question, *r. Then D's chance.*

207. l. 31. *r. meteorological apparatus.*

266. l. 41. *dele and repelled.*

304. l. 10. *r. or the water.*

313. l. 24. *for communication r. paper.*

381. to the note add 1, page 252.

382. l. 37. *r. Surfaces.*

399. l. 5. *r. as it would have otherwise.*

418. l. 22. *r. place of percussion.*

422. l. 36. *read five feet wide.*

432. l. 24. for *fine H × fine b; or putting radius =*

unity, Cof. a = Cof. a, &c. read fine

H × fine b × radius; or putting radius

= unity, Cof. A = cof. a, &c.

— l. 26. *read Mr. William Cuthieu.*

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- V. Concerning the Properties of the Sulphureous Acid, and its Combinations with Earthy and Alkaline Bases. By Citizens Fourcroy and Vauquelin p. 313

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- VI. A Memoir on the Nature of the Alum of Commerce, on the Existence of Potash in this Salt, and on various simple or triple Combinations of Alumine with the Sulphuric Acid. Read before the National Institute of France. By Citizen Vauquelin — — — — — p. 318

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III. Experiments and Observations made with the View of ascertaining the Nature of the Gaz produced by passing electric Discharges through Water; with

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- VI. Concerning the Steam Engine, as originally invented by the Marquis of Worcester, and the Improvements since made in Steam Engines without the Piston, or Lever. With a Description of an Engine of this Kind constructed by Mr. Peter Keir, of Kentish-Town — — — — — p. 419

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- VI. An Account of some Experiments to determine the Force of fired Gunpowder. By Benjamin Count of Rumford, F.R.S. M.R.I.A. ————— p. 459

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- VIII. On the Irritability of the Pollen of Plants. With an Account of a Composition for closing wide-mouthed Vessels (continued from page 313) p. 471

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- II. Experiments on the Composition and Proportion of Carbon in Bitumens and Mineral Coals. By Richard Kirwan, Esq. F.R.S. L. & E. M.R.I.A. &c. p. 487

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- IV. The Description of a new portable Electrical Machine. Invented by the Rev. W. Pearson of Lincoln — — — — p. 506

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- V. A Memoir concerning a remarkable Phenomenon in Meteorology. Read to the Society of Naturalists of Geneva. By M. de Saussure, October 1797. p. 511

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- VIII. Observations on Strontian. By Citizen Pelletier. Read to the National Institute, April 30, 1796 — — — p. 158

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- IX. Philosophical News, and Accounts of Books — — — p. 523

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- I. Observations on Strontian. By Citizen Pelletier. Read to the National Institute, 30th April 1796 (concluded from p. 522). — p. 529

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- II. Abstract of two Memoirs on a Method of obtaining Barytes pure, and on the Properties of this Earth compared with those of Strontian. By Citizens Fourcroy and Vauquelin. Read to the National Institute, 30th April and 21st September 1796 — p. 535

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- III. Extract of a Letter from Count Muffin Puschkin, Vice President of the Department of Mines at Petersburg. On the Salts, Precipitates and Amalgam of Platina; on Cobalt; on Antimonial Soap; and on the Decomposition of Soap by the acid Extracts of colouring Matters — p. 537

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- IV. Observations on the fundamental Property of the Lever. With a Proof of the Principle assumed by Archimedes in his Demonstration. By the Rev. S. Vince, A.M. F.R.S. — p. 541

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- V. Observations on the Acid of Tin, and the Analysis of its Ores. Read at the Sitting of the Class of Mathematical and Philosophical Sciences of the National Institute of France, the first of Messidor, in the Year 5. By Citizen Guyton — p. 543

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- VII. Experimental Researches to ascertain the Nature of the Process by which the Eye adapts itself to produce distinct Vision — p. 547

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VIII. Experiments and Observations on the Inflexion, Reflexion, and Colours of Light. By Henry Brougham, jun. Esq. — — p. 551

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IX. An Account of the Manner in which Heat is propagated in Fluids, and its general Consequences in the Economy of the Universe. By Benjamin Count of Rumford. (Concluded from p. 348) — — p. 563

Experiments of the fusion of ice by water suffered to repose upon it. Precautions. Results with boiling hot water, exhibiting the quantities of ice melted in a given time. Experiments with cold water at 41 degrees, by which a greater quantity of ice was melted than by boiling water. General consequences applied, at length, to the distribution of heat over the surface of the globe, by virtue of the internal motions of water, and currents which take place in the seas, similar to the trade winds in the atmosphere.

X. Useful Notices respecting various Objects.—Welding of Cast-Steel—Flexure of Compound Metallic Bars by Change of Temperature — p. 575

Sir Thomas Frankland's method of welding cast-steel to iron. Experiments to determine the proportion between the thickness of compound metallic bars and their flexure by heat.

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- I. Observations on Water-spouts seen from Nice. By Mr. Michaud, Correspondent with the Royal Academy of Sciences at Turin — p. 577

State of the weather previous to the appearance of water-spouts. Vaporous foot of a water-spout, unaccompanied by the spout itself. Description of another water-spout of extreme magnitude, the lower part of which threw out large streams of vapour and jets of sea water. Termination of water-spouts on their approach to the shore. Instance of the foot and stem being formed at several miles distance from each other; the former of which remained stationary on the sea until the winds had brought the cloud from which the latter was to issue, immediately over the mass of vapour. Additional facts. Observations; and explanation of the drawings.

- II. Experiments and Observations on the Inflexion, Reflexion, and Colour of Light. By Henry Brougham, jun. Esq. (Concluded from p. 563) p. 585

Explanation of phenomena dependent on the flexibility of light. Interference of the penumbæ of bodies. Colours of light through mediums partially transparent. Colours of a flame. Blue shadows. Coloured fringes. Colours surrounding the sun's image—and the flame of a candle. Sir Isaac Newton's coloured rings by reflexion from a concave glass mirror. Admeasurement of the flexibilities of the several primary rays. Summary of optical science. Explanation of various phenomena dependent on reflexibility. Bright streaks from a candle when the eyes are nearly shut. Other streaks variously coloured formed by reflexion from surfaces of a fibrous structure. Colours from scratches in a piece of metal;—from the minute particles of unpolished bodies;—from hairs, spiders' webs, certain fossils, &c. Whether these principles be sufficient to explain the natural colours of bodies. The rays of light are not variously reflexible in the manner taught by Newton in his celebrated experiment of internal reflexion by a prism. In that experiment the rays were variously reflected, for no other reason than because their incidences were different. Images formed by reflexion of one uniform colour. Experiment and observations to shew that the natural colours of bodies may be produced in this way. Colours of thin plates by reflexion. Summary of propositions.

- III. On certain Points of Nomenclature. By a Correspondent — p. 597

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A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

APRIL 1797.

ARTICLE I.

The Principles and Application of a New Method of constructing Achromatic Telescopes.
By ROBERT BLAIR*, M. D.

AFTER a short historical retrospect to the discoveries of the unequal refrangibility of light by Newton, and the subsequent correction of its effects in optical instruments by Dollond, together with the remedy which is afforded in compound lenses for the aberration arising from sphericity, our author remarks, that it was expected by men of science that an increase of the aperture and power of the refracting telescope would be the necessary consequence of such important steps towards the perfection of its theory. These expectations have not hitherto been fully answered: for it is certain, that no Achromatic Telescopes have yet been made of equal aperture with the single object-glasses of Huyghens and others, nor in this respect comparable to reflectors, though the errors of workmanship are much more noxious in these last than in lenses.

The general answer made by artists to enquiries of this nature is, that the fault lies in the imperfection of glass, and particularly in the dense glass known by the name of flint-glass.

The imperfection of glass, for optical purposes, arises partly from its opacity and colour, but chiefly from irregularities in the refractive densities of its parts. The researches of chemists and manufacturers to remove this defect have been considerable, but hitherto without much success.

From the consideration that it is not impossible to introduce a fluid medium to supply the place of one of the lenses in the compound object-glass, Dr. Blair was led to the experimental enquiry, whether nature afforded fluids possessed of the requisite qualities.

* The original paper, from which I have made the above abridgement or selection of parts, is inserted in the Transactions of the Royal Society of Edinburgh, vol. iii. It occupies 76 pages. N.

To ascertain the mean refractive and disperse qualities of fluids, the Doctor made use of two kinds of apparatus. The first, intended to afford a gross knowledge of their properties, consisted of an apparatus of prisms. In the second method, where the fluids promised to be of practical use in Optics, they were more critically examined by means of lenses, in which the effect, from being magnified, is rendered more conspicuous.

The prismatic apparatus consists of a small, equi-angular, three-sided prism, of brass. Through this prism, and parallel to one of its sides, are bored two holes, at a small distance from each other, equal in size to the pupil of the eye. The sides of the prism are ground flat, and there are two pieces of glass with parallel sides, of the same dimensions as the sides of the prism. There are also prisms of the same size, and with the same angles, of different kinds of glass; and some crown-glass prisms with smaller angles, which, by being applied to the large prism, or to each other, vary the refracting angle at pleasure.

When it is proposed to try the properties of any fluid, one of the small plates of glass is applied over the holes on the side of the brass prism. A few drops of the fluid are then dropped into the hole, and, when it is full, the other plate is laid over the holes upon the opposite side, and the whole is secured by tying a piece of packthread round the ends. One of the glass prisms is now to be applied to the brass prism contiguous with one of the parallel plates, the refracting angles of the two prisms being placed in opposite directions so as to form a small parallelopiped.

Nothing farther is necessary than to apply the eye to the hole which contains the fluid, in such a way as to observe through it any bright well-defined object. The bars of the window answer the purpose very well in the day-time, and the moon or a candle in the night. The intention of the two holes is for the sake of greater expedition. The properties of two fluids may thus be examined and compared at the same time. As the prismatic portion of fluid and the glass prism have equal refracting angles, and refract in opposition to each other, it will easily be understood, that, if the object seen through the two prisms coincides with the same object seen directly, the mean refractive density of both mediums will be the same. When this is the case, if the object seen through these prisms appears free from prismatic colour, the disperse power of the fluid medium is also the same with the disperse power of the glass prism. But otherwise they will be different.

Those mediums, it is to be observed, are said to have the same mean refractive density, which, under equal obliquities of incidence, equally refract the mean refrangible rays; and two mediums are said to have the same disperse power, which produce an equal inclination of rays of the same colour to the mean refrangible ray, when the whole refraction of the mean refrangible ray is equal in both.

When an object seen through the equal wedges of glass and fluid appears coloured, one of the smaller glass wedges is to be applied, and shifted, till the object appears colourless. It is easy to distinguish, by the order in which the prismatic colour lies, whether the small prism is to be applied in such a way as to increase the dispersion of the rays occasioned by the fluid so as to enable it to counterbalance that of the glass, or whether the refracting angle of the glass prism requires to be enlarged, to enable it to counteract the dispersion occasioned by the fluid.

By proceeding in this way to shift the angles of the prisms till, first, the direct and refracted images of an object coincide, without regarding the colour, and, next, till the refracted

fracted image appears colourless, without regarding the coincidence, the ratio of the mean refractive and dispersive powers of that kind of fluid, and that kind of glass, with which the experiments are made, will be obtained from the angles of the prisms being given in both cases.

In order to ascertain the absolute refractive density of glass, or any other medium, that is to say, the general ratio of the sines of the angles of incidence to the sines of the angles of refraction of the mean refrangible ray which obtains in that medium, the Doctor took a direct method, similar in principle to that employed by Sir Isaac Newton, and described by him in the seventh proposition of the first book of his Optics, and likewise in his Optical Lectures, p. 54; but which, as he justly remarks, will be found much easier, and perfectly accurate.

Instead of causing the rays to pass through the sights of a large and accurate quadrant at the distance of ten or twelve feet, as directed by Sir Isaac Newton, the Doctor employed a Hadley's quadrant, in the following manner:

Plate I. fig. 1.—I represents the index-glass, and H the horizon-glass of a Hadley's quadrant. SI represents a solar ray incident on the index-glass, thence reflected to the horizon-glass H, and from it to the eye at E. The line *fg* represents another solar ray, incident on the prism P, and through it refracted to the eye at E. When the prism is turned slowly round its axis till the spectrum G appears at its greatest height, this is its proper position. The angle formed by the direct and refracted ray is then the least possible, and the angles of incidence and emergence are equal. Let the prism be secured in this position. A slight inspection of the figure will shew, that when the reflected and refracted images of the sun are made to coincide, the angle marked by the index of the quadrant is the same which the incident ray *fg* forms with the refracted ray PE produced. For SZH is the angular distance of the sun, and his doubly reflected image marked by the index; and the angle *fg* G, which the ray incident on the prism forms with the refracted ray produced, is equal to it; *fg* and SI being parallel, and PZ and HZ being coincident.

The manner in which the ratio of the sines of the angles of incidence and refraction may be computed, from the above angle and the refracting angle of the prism being given, is fully explained in the celebrated works which have just been quoted.

The Doctor here remarks, that as it is the ratio of refraction of the mean refrangible ray which is wanted, the centre of the reflected image of the sun ought to be made to coincide with the centre of the coloured spectrum, as represented in the figure; and if, instead of this, the coincidence be formed with the most or least refrangible ray, or any of the intermediate rays, it will be the ratio of refraction of these rays, and not of the mean refrangible ray, which will be found from the observation. Hence this method might be practised for determining the dispersive power, as well as the mean refractive density of any transparent substance, whether solid or fluid; but the Doctor has preferred a combination of prisms or lenses, because it is the relative ratios, more than the absolute ratios, which are most immediately wanted.

By this prismatic apparatus, the optical properties of a great variety of fluids were examined. The solutions of metals and semi-metals proved in all cases more dispersive than crown-glass. Some of the salts, as, for example, sal-ammoniac, greatly increased the dispersive power of water. The marine acid disperses very considerably, and this quality increases with its strength. The most dispersive fluids were accordingly found to be those in which this acid and the metals were combined. The chemical preparation called *causium*

antimoniale, or *lutyrum antimonii*, in its most concentrated state, when it has just attracted sufficient humidity to render it fluid, possesses the quality of dispersing the rays in such an astonishing degree, that three wedges of crown-glass are necessary to remove the colour produced by one wedge of this substance, of an equal refracting angle opposed to them. The great quantity of the semi-metal retained in solution, and the highly concentrated state of the marine acid, are considered by the author as the cause of this scarcely credible effect.

Corrosive sublimate mercury, added to a solution of *sal ammoniacum* in water, possesses the next place to the butter of antimony among the dispersive fluids which he examined. It may be made of such a degree of strength as to require a wedge of crown-glass of double the refracting angle, to remove the colour which a prism of it produces. The mercury and marine acid contained in this solution are manifestly the cause of its dispersive power: for neither the water, nor the volatile alkali, which are its other component parts, will be found capable, if tried separately, of contributing towards this effect.

The essential oils were found to hold the next rank to metallic solutions, among fluids which possess the dispersive quality. The most dispersive were found to be those obtained from bituminous minerals, such as the native petrolea, pitcoal, and amber. When the refraction is without colour, the proportion of the refracting angle of a prism of these, to the refracting angle of a prism of crown-glass acting in opposition, is about two to three. The dispersive power of the essential oil of saffraas, is not much inferior to these. The essential oil of lemons, when genuine, requires the refracting angles of the prisms necessary to produce a colourless refraction, to be as three to four. In oil of turpentine, this proportion is as seven to six; and the essential oil of rosemary is still less dispersive.

Some expressed oils, which were examined, were found not to differ sensibly in dispersive power from crown-glass; which was also the case with rectified spirits, and with nitrous and vitriolic ether.

Having been thus successful beyond his hopes in discovering fluids capable of removing the great imperfection of telescopes arising from the different refrangibility of lights, the next object of the author was to select from this variety those that seemed best adapted to optical purposes.

There was no doubt that those mediums which most disperse the rays were *ceteris paribus* to be preferred. It will also be found, when the method of correcting those errors which arise from the spherical figures of lenses comes to be considered, that there is apparently an advantage in using a dispersive medium, whose mean refractive density exceeds the mean refractive density of crown-glass.

As the antimonial caustic possesses both these advantages in a degree far beyond what was to be expected in any fluid, some of it was included between two double convex lenses of crown-glass, whose radii of convexity were as two to one. The least convex sides of these were turned toward each other, and they were kept at a proper distance by means of a glass ring. The cavity was then filled with the strongest butter of antimony. Here it is evident, that there is a concave lens of the dispersive fluid acting in opposition to the two convex lenses of crown-glass, and that the proportion of the radii of these is the same which was found by the prisms to correct the colour, namely, three wedges of crown-glass to one of the butter of antimony.

This compound object-glass being put into a tube, an eye-glass was applied, and, according

ing to expectation, the colour was found to be removed. But the Doctor was surpris'd to find, on directing the instrument to a planet, and using a deep eye-glass, that this fluid, in its highly concentrated state, was subject, like flint-glass, to great irregularities in its density, discoverable by streams of light, like comets' tails, issuing in different directions from the disk of Venus, which was the planet observed. By shaking the object-glass, these might be in a great measure removed, but soon returned; and, after standing all night, broad veins, in different parts of the included fluid, were perceptible to the naked eye.

It was necessary, on this account, to reject very dense fluids. The antimonial preparation was found to be reducible to a sufficient degree of fluidity, by mixing it with spirit of wine or vitriolic ether, into which a small quantity of the marine acid had been previously dropped. This prevents any precipitation of the semi-metal in the form of a calx. In this diluted form, either this preparation, or the solution of corrosive sublimate mercury alone in spirit of wine or in water, with the addition of crude *sal ammoniacum*, may be employed for producing refraction without colour, and without being subject to that irregularity of density to which flint-glass and very dense dispersive fluids are subject.

But as solutions of saline substances in this diluted state do not differ materially in dispersive power from the essential oils, these two kinds of fluids may be used indifferently.

In one case, however, our author remarks, that water, or vitriolic ether, impregnated with antimony or mercury, will have the advantage from being less dense than essential oils, and that is, where it is required to produce a single refraction, in which there shall be no difference of refrangibility of heterogeneous light. As this expression might sound strange in the ears of Opticians, the Doctor has employed a considerable part of his paper to explain what is meant by it. He shews, that there are cases of single refraction in which *the violet rays are the least refrangible*; or in which *all the rays are equally refrangible*; or in which *the red rays are refracted from the perpendicular, and the violet rays towards the perpendicular, while the mean refrangible rays suffer no refraction*.

These positions must, no doubt, at first consideration, appear paradoxical; but their singularity will vanish on attending to two circumstances: 1st, That these refractions, though single, are not effected by the simple agency of one medium on the confine of a vacuum, but by the difference of the contrary actions of two mediums at their common surface; and, 2^{dly}, That Opticians, from the habit of contemplating the crown and flint glasses, have associated the notion of a greater dispersive power with that of a greater power of mean refraction.—But our author, having ascertained various facts in which the greater power of dispersion accompanies the less mean refractive power, has in consequence shewn that an achromatic lens may be constructed, in which all the refractions are made in the same direction*. The cases of single refraction here mentioned are very perspicuously and at some length shewn, with suitable diagrams, as well from the general facts before enumerated, as from assumed powers of attraction of the mediums on the rays of light, after the supposition of Sir Isaac Newton. For the sake of brevity, however, I shall give the substance of his explanation, without particularly attending to the order of arrangement.

* In an object-glass formed of oil of turpentine included between two double convex lenses, the radii of whose convexities are as six to one, and the deep sides inwards, there are four refractions, all towards the axis; and the aberration, from difference of refrangibility, is removed. This compound lens has twenty inches focal length, and one inch and half aperture. Its performance as a telescope is not contemptible. B.

If a pencil of compounded light be imagined to pass obliquely out of the plane surface of any dense medium into a vacuum, it is well known that the refraction will be made from the perpendicular, the violet rays being most refracted and the red least, so that these rays will be inclined to each other. But suppose, again, that instead of the rays being suffered to pass into the vacuum, another medium were applied, having a plane surface in contact with that of the former, and that its power of dispersion were such as precisely to counteract the dispersive power exerted on the rays by the former medium: in this case it is evident that the pencil of light would pass forward colourless. Nevertheless, as the dispersive power may be equal in two mediums which differ in mean refractive power, it is no less clear, that refraction will take place wherever these two powers are not precisely equal; and that either towards the perpendicular, if the power, or, as it is called, refractive density of the second medium, be greater than that of the first, or from it if less.

In the next place, suppose the two mediums not to agree in dispersive power, but that the second medium disperses more than the other. The violet ray will not only be prevented from separating on the one side of the red, but, by the excess of dispersive power, it will be drawn so far as to make an angle with that ray on the other side, and the dispersion will be made the contrary way. If the mean refraction of the second medium exceed that of the first, the whole pencil will be bended towards the perpendicular, and towards the same region as the dispersion carried the violet ray; so that the effect will be similar (though less in power) to that of a common refraction and dispersion by a single dense medium in vacuo. This happens in the transition from crown-glass into flint-glass. But if the mean refractive power of the second medium be less, the pencil will be bended from the perpendicular, though the violet ray will notwithstanding continue nearest the same perpendicular. The red ray will therefore be the most deflected, and the violet least. This effect takes place when light is refracted in the confine of crown-glass and oil of turpentine, and also of many other fluids. If the refractive powers of the two mediums upon the mean ray be equal, the direction of this ray will continue unaltered at its transition from the one to the other. But if the dispersive power differs, the extreme rays will form an angle, of which the red rays will be either at the one or the other limit, according to circumstances. For example, if the dispersive power of the first medium be greatest, the angle will merely be diminished by the action of the second medium; so that the violet ray will be the most, and the red the least, refracted from the perpendicular; as would have happened (though more strongly) in the transition into a vacuum. But if, on the contrary, the dispersive power of the second medium were greatest, the violet ray will be carried more towards the perpendicular than the red by the difference of the two powers. This case of refraction is found to take place in the confine of crown-glass and butter of antimony, when the latter is so far diluted as that both mediums equally refract the mean ray under equal angles of incidence.*

In the case of refraction without colour, as before explained, the power of dispersion in the second medium was assumed to be such as precisely to counteract the effect of the first; that is to say, not only with regard to the extreme red and violet rays, but also with regard to the mean and all the intermediate rays. But Doctor Blair's Experiments shew, that, in mediums which disperse the light but little, the green is the mean refrangible ray; that in by far the greatest number of more dispersive mediums, including flint-glass, metallic solutions, and essential oils, the green light is not the mean refrangible order, but forms one

of the less refrangible orders of light, being found in the prismatic spectrum nearer to the deep red than to the violet; and that in another class of dispersive mediums, which includes the muriatic and nitrous acids, this same green light becomes one of the more refrangible orders, being now found nearer to the extreme violet than to the deep red. Whenever, therefore, the light passes out of one of these three classes of mediums into another of a different class, the dispersive powers will not accurately counteract each other, even though they may be adopted to cause the extreme red and violet rays to become parallel; but the rays which would occupy the interior parts of the spectrum will be dispersed, and that in a greater degree the more remote they are from the extremes.

These several cases of refraction were likewise tried with compound object-glasses, which shew the effect better than prisms. Thus, if a plano-convex lens have its plane side turned toward a distant object, the rays will enter it, as to sense, perpendicularly, and will therefore suffer no refraction. If the convex surface of this lens be brought in contact with a fluid of less mean refractive density than the glass, but exceeding it in dispersive power in that degree which occasions an equal refraction of all the rays, all these rays will then be converged to the same point, which are incident at the same distance from the axis of the lens. The focal distance of this compound lens will be greater or less, in proportion to its radius of convexity, and to the difference of refraction between it and the fluid made use of. While the fluid is confined on one side by the plano-convex lens, let the lens which is brought in contact with it on the opposite side have one of its sides ground convex, and the other concave; the radii of their sphericities being equal to the focal distance at which the rays are made to converge by the refraction which takes place when light passes from the plano-convex lens into the fluid. It is manifest that the light will now both enter into this compound lens, and emerge from it, perpendicularly, and will therefore suffer no refraction, except in the confine of the convex side of the plano-convex, and the dispersive fluid where all the rays are equally refrangible. A compound lens of this kind is represented in the second figure, Pl. I. which requires no farther explanation; excepting only, that, instead of being spherical, it is represented with that curvature which converges homogeneous rays incident at all distances from the axis to the same point. If the required curvature could be given to lenses with sufficient accuracy, this figure seems to represent as perfect a construction of the object-glass of a telescope as can be desired. But there is reason to think that a spherical figure may be communicated, not only much easier, but with greater accuracy than a spheroidal or hyperboloidal, which would then be required; and even if this difficulty could be got over, there would still remain a fundamental fault in the theory. Before relating the observations by which this was detected, Dr. Blair explains the method of removing the spherical aberration by a combination of convex and concave lenses. For, next to the indistinctness arising from the unequal refrangibility of light, this aberration occasioned by the spherical figures of lenses is the great obstacle to the advancement of the powers of vision. The aberration from the spherical figure has been treated of, in all the variety of cases which can occur in single glass lenses by the great Hugenius in his *Dioptrics*, a posthumous work. He there demonstrates, that the quantity of this aberration is very different in different lenses of the same focal distance, according to the convexities or concavities of their two sides, and the manner in which they are exposed to parallel rays.

In

In convex lenses, those rays which pass at a distance from the axis are converged to a point nearer to the lens than its geometrical focus. The distance between the point at which the external ray of a pencil incident on a lens intersects its axis, and the geometrical focus, is called the linear aberration of that lens.

Hugenius demonstrates, that when a plano-convex lens is exposed to parallel rays, with its plane side towards them, this aberration will amount to four times and a half the thickness of the glass. By the thickness of a convex lens, is meant its greatest thickness in the middle, after subtracting its thickness, if it has any, at the outer edge; and by the thickness of a concave lens, is meant its thickness at the external edge, after deducting its thickness in the middle.

On turning the convex side of the lens towards the light, the linear aberration will only exceed the thickness of the lens by one sixth part.

When both sides of a lens are convex, and the proportion of their convexities is as one to six; if the most convex side be exposed to parallel rays, the aberration will exceed the thickness of the lens one fourteenth, which is the smallest possible aberration of any convex lens.

If it is required to increase the aberration, this may be done by grinding one side of the lens convex, and the other side concave, to a longer radius. Such a lens, with its concave side turned towards parallel rays, will have more aberration than any plano-convex or double convex lens of the same focal distance.

Hugenius proceeds to shew, that the same aberration is produced by concave lenses as by similar convex ones. When a plano-convex lens is exposed to parallel rays with its plane side outward, the external ray of the pencil being produced backward, after refraction, will intersect the axis of the lens nearer to it than its focus by four times and a half the thickness of the lens. But if its concave side be exposed to the parallel rays, the aberration will only exceed the thickness of the lens one fourteenth part. A double concave, whose radii are as one to six, with the most concave side turned outward, disperses the rays with the least aberration; and a concave meniscus, with its convex side outward, produces more aberration than any plano-concave or double concave lens of an equal focal distance.

These are sufficient data for correcting the aberration from the spherical figure, in cases where both a convex and concave lens are required in the construction of the compound object-glasses.

Fig. 3. Pl. I. Let AB represent a convex lens receiving a pencil of rays from the object S, and converging rays incident near the axis at ST to the point F, and external rays as SB to the point D, so that DF represents the greatest linear aberration in this case.

Again, let GH (Fig. 4.) represent a concave lens receiving the parallel rays SH, RK, which it refracts in the lines HX and KV. This ray KV being produced backward, will intersect the axis of the lens nearly at the point N, which is called the virtual focus of the concave; and the external ray HX produced backward, will intersect the axis in some point P nearer to the lens than its focus PN, being the linear aberration.

It may here be observed, that the convex is in that position which produces the least aberration, and the concave in the position which produces most aberration. Hence, to render the aberrations DF (Fig. 3.) and PN (Fig. 4.) equal, the focal distance of the convex must be much shorter than that of the concave; and if the distances of the points F

and N, from the convex and concave lenses, be required to be the same as represented in the figures, then must the object be placed much nearer to the convex. Hence the image of the near object S is represented at the same distance from the convex lens in Fig. 3, as the virtual focus of the concave in Fig. 4, where it is represented as receiving parallel rays which are supposed to come from an infinitely distant object.

Now when the distance between K and N, which is the point from which parallel rays are made to diverge by the concave lens, is equal to the distance between T and F, which is the point to which rays issuing from S are made to converge by the convex, and when the aberrations DF and PN are also equal; it will follow in this case, that if the two lenses be placed contiguous, in the manner represented in the twelfth figure, parallel rays incident on these lenses will be converged to the point S, without any aberration of the external ray.

For it is an axiom in optics, that if a ray of light after refraction be returned directly back to the point of incidence, it will be refracted in the line which was before described by the incident ray.

If, therefore, we conceive the whole of the light emitted from the point S (Fig. 3.) and converged by the convex lens towards the points D and F, to be returned directly back from these points, it will be accurately converged to the point S, whence it issued. Now the parallel rays SH, RK (Fig. 4.) after their emergence from the concave lens in the lines HX, KV, are precisely in the same relative situation as the rays supposed to be returned directly back from F and D are in at their incidence on the convex; and therefore, when these lenses are placed contiguous in the manner represented in the twelfth figure, parallel rays incident on the concave lens, and immediately after their emergence from it, entering the convex lens, will be accurately converged to the point S without any aberration.

This, which is the most simple case, will suffice to explain the nature of that aberration which arises from the spherical figures of lenses, and a method of obviating it, by combining a convex and concave.

The demonstration is perfect as far as regards the external ray, which is here represented passing from the external part of the concave into the external part of the convex, in immediate contact with it; and if the surfaces of the two lenses, which respect each other, were either in contact or parallel, it would be true with regard to all the rays. But as this is not the case, there arises a small secondary aberration, the effect of which only becomes sensible in large apertures.

Hence may be understood the reason why the indistinctness arising from the spherical figures of lenses, may, in the common Achromatic Telescope, be more nearly removed in those constructions of object-glasses in which three lenses are employed, than in those composed only of two; and also the advantages in this respect which may be derived from introducing fluid mediums which differ from glass in their mean refractive density, and in the quantity of aberration produced by their refractions. For it will be found upon computation, that when the fluid medium is rarer than glass, the aberration from the spherical figure is increased, and becomes greater in proportion as its density diminishes. Now, by making the density of the fluid medium approach nearer and nearer to the density of the glass with which it is in contact, we may increase the rarity of our refracting medium, or,

which amounts precisely to the same thing, diminish the difference of density of the two mediums at pleasure.

It will appear from what has been explained, that the aberration from the figure cannot be corrected by interposing a dispersive fluid between two convex lenses of a greater refractive density than the interposed fluid. For all the refractions, being made the same way, tend to converge the external rays to points nearer the lens than its geometrical focus. Hence, when rare fluids are made use of to remove the aberration from the difference of refrangibility, some farther contrivance becomes necessary to correct the spherical aberration.

The most obvious way, and which on trial was found successful, is to include the rare dispersive fluid between two glasses, ground concave on one side and convex on the other, and thus form such a concave as shall be required. An Achromatic Object-glass may be formed by combining this with a convex. The objection to this construction is, that one of the advantages arising from the use of fluids is given up, namely, the prevention of that loss of light, by reflection, which is a consequence of the fluid being in immediate contact with the glass, whereas in the present case the space between the convex and concave is occupied by air.

On this account, Dr. Blair attempted to introduce a third medium, by filling this vacancy with a fluid of the least dispersive kind, and of less mean refractive density than the dispersive fluid. For this purpose he employed sometimes rectified spirit of wine, and sometimes vitriolic ether; and by giving to the lenses the proper degree of curvature, in which great variety may be introduced, he succeeded in forming object-glasses in which both aberrations are removed, and hardly any more light lost than in a simple object-glass.

Having gained this point, he now determined to try how far the aperture of the object-glass might be increased, without increasing its focal length, expecting, at least, to equal reflectors in this respect. But the first trials to execute object-glasses on this principle, though they left no reason to complain of want of success when compared with such instruments as are now in use, exhibited new phenomena and new obstacles to the perfection of the theory of Telescopes more unaccountable and perplexing than any he had before encountered. The history of these interesting facts, and the regular progress of discovery by which they were remedied, constitute a large part of the paper, which well deserves to be consulted by those who wish effectually to prosecute this subject. Brevity, however, demands a less historical narration in this place.

These new difficulties arose from the effects of contrary powers of dispersion, which though equal upon the extreme rays, were not found to be the same upon the rays that occupy intermediate spaces in the coloured spectrum. From what has been already stated on this subject, the attentive reader may understand, that such intermediate rays from a compound lens of the kind here described, would not be assembled at the common focus. The particular nature of this dispersion is much more accurately seen by applying a deep eye-glass to the focal image, than by experiments with prisms. When the image of a lucid point is formed in the focus of a simple lens, the violet or most refrangible rays are converged to a focus nearest to the lens, and the deep red rays are converged to a focus at the greatest distance from it. The consequence of this is, that if the image be examined by an eye-glass nearer to the lens than is required for distinct vision, it will be surrounded with a

red

red fringe, which is the prevailing colour of the least refrangible rays; and if the eye-glass be placed at a distance beyond that which is required for distinct vision, it will be surrounded with a blue fringe, which is the prevailing colour of the most refrangible rays.

The reason of this will appear more clearly from inspecting the sixth figure, where the red rays appear outermost within the focus at A, and the violet rays appear outermost beyond the focus at B. These colours are also visible when an image of any luminous object, as the sun, is formed by a lens on a white ground; and they will be so much the more conspicuous, the greater the diameter of the lens in proportion to its focal distance. The reverse of this happens in compound lenses, when the medium employed to correct the colour disperses more than it ought to do.

In this way the correction of the colour may be examined, and the qualities of refracting mediums investigated, to an extreme degree of accuracy. Yet the effect will be rendered still more sensible by covering half the object-glass. For, when this is done, the colour produced by the uncovered half of the object-glass appears, without being mixed with that of the opposite side, even when the eye-glass is adjusted to distinct vision. Thus, in Fig. 6, the colours produced by both sides of the lens are mixed at the general focus F. But if the rays coming from one side be intercepted, those which are refracted by the other side will appear in their proper colours. By these means, and by employing a very luminous object surrounded by a dark ground, and a high magnifying power, the least uncorrected colour may be rendered sensible.

The first conjecture which presented itself on the observation of irregularities of this kind, in compound lenses adjusted so as to correct the aberration of the extreme rays, was, that the colour might somehow proceed from the compensation at different distances from the axis of lenses not corresponding as the plane surfaces of prisms everywhere do. Trials on the images formed by the central parts, and afterwards by the parts near the circumference, proved, however, that this conjecture did not reach the cause. Subsequent experiments and examinations of the coloured fringes led the author to the true cause, that is to say, the want of proportionality in the dispersive powers of the different mediums, as has been already stated. It remained, therefore, to discover, by a new process of experimental enquiry, the adequate remedy for such an extreme source of imperfection. This led to the valuable doctrine, that one class of dispersive mediums throw the green nearer to the violet, while another class throw the same colour nearer to the red, than is seen in the spectrum formed by crown-glass and other mediums, which disperse but in a small degree. These mediums were therefore applied in opposition to each other, to correct this secondary aberration in lenses more compounded than might have been required if such a difficulty had not presented itself. And lastly, the greater degree of composition in these perfect lenses was got rid of by the happy expedient of mixing the two different kinds of dispersive mediums, which, as it fortunately turned out, did produce a composition of an intermediate nature, with regard to this proportionality in the arrangement of the prismatic colours.

It has already been remarked, in the order of this abridgement, that, in metallic solutions and essential oils, the green light is among the less refrangible rays; but that, in the marine and nitrous acids, the same green light becomes one of the more refrangible. It was not probable that the essential oils could be united with marine acid so as to form a colourless

fluid fit for optical uses; but nothing could be better adapted for this purpose than metallic solutions.

The first trial was made with butter of antimony. On increasing the proportion of marine acid, the fringes of green and purple, which, being the intermediate rays, were irregularly refracted by the metallic solution, grew narrower and narrower, till they entirely disappeared; and if more was then added, they re-appeared in an inverted order. The same thing was tried with a solution of crude sal ammoniac and corrosive sublimate. With a certain proportion of these two substances, the rays of all colours emerge from the compound object-glass equally refracted. If the proportion of the ammoniacal salt, and consequently of the marine acid it contains, be increased, the green rays which were the mean refrangible, in the dispersive fluid as well as in crown-glass, draw nearer to the violet, making a part of the more refrangible half of the spectrum, and consequently emerge less refracted than the united red and violet rays, and are converged to a focus at a greater distance from the object-glass; so that the green fringe now appears within the focus, and the purple fringe beyond it. But, on increasing the proportion of mercurial particles, these same green rays shift their situation to the less refrangible half of the spectrum, which appears from their now emerging most refracted, and being converged to a point nearer to the object-glass than the united red and violet, whose refrangibility does not appear to be affected by these admixtures, which occasion such remarkable fluctuations in the refrangibility of the green rays and other intermediate orders. It may possibly seem strange at first view, that the green rays should emerge most refracted from the compound object-glass, when their refrangibility in the dispersive medium is diminished, and least refracted under the contrary circumstances. The cause of this is, that the principal refraction of the compound object-glass is performed by the indispersive convex lens, which is opposite to the refraction produced by the dispersive concave.

Fig. 7. represents an object-glass of this kind in Dr. Blair's possession, in which the metallic particles are so far diminished, and the particles of marine acid so far increased, as to render the refraction of the several orders of rays proportional in both mediums. There are two refractions in the confine of glass and the fluid, but not the least colour whatever. Hence it is inferred that, notwithstanding the considerable difference of density, and the refraction which, on account of the anterior surface being plain, and the posterior surface spherical, having the focus of parallel rays for its centre, is performed only at the two surfaces of the fluid, there is no unequal refrangibility of light. The rays of different colours are, as the Doctor observes, bent from their rectilinear course with the same equality and regularity as in refraction.

As custom has already appropriated the word *Achromatic* to that kind of refraction in which there is only a partial correction of colour, the author proposes to distinguish this entire removal of aberration by the term *Aplanatic*.

Thus far I have endeavoured, partly by abridgement, partly by extracts, and partly by taking greater liberties of narration and arrangement, to convey the substance of Dr. Blair's important discoveries to the public. It is now some years since the attention of the world was first directed to his improvements. I have therefore been induced to make enquiry, among the artists of this metropolis, into the state of the undertaking

undertaking considered as a manufactory. These enquiries led me to the Doctor himself; from whom I understand, that all the practical difficulties are removed, and that the delay which has prevented the philosophical world from being yet supplied with these instruments has arisen merely from the interruption of engagements. And as I have reason to expect some communications from him on this subject in future, it becomes unnecessary to enter into any of those remarks, in this place, which the immediate consideration of the subject might suggest.

II.

A Remarkable Effect of the Inflection of Light passing through Wire Cloth, not yet clearly explained.

IN the second volume of the Transactions of the American Philosophical Society, p. 201, an optical problem is proposed in a letter from Mr. Hopkinson to Mr. David Rittenhouse, who has given a solution in his answer. Mr. Hopkinson, upon looking through the threads of a silk handkerchief, held close before his face, at a distant lamp, was surprised to observe what he thought to be the threads magnified; and still more, that these supposed threads remained stationary, notwithstanding any motion he gave the handkerchief to the right or left.

Mr. Rittenhouse, in his remarks on this phenomenon, observes very justly, that the appearance, which is that of certain luminous points regularly arranged in right lines crossing each other, cannot consist of any image of the threads of the handkerchief, but must be certain images of the lamp formed by the inflection of light passing near the threads. He appears to have considered the handkerchief as a very imperfect instrument, and therefore too hastily threw it aside for an instrument consisting of parallel hairs not crossed by others. With this he observed that the line of light or portion of the sky seen through a window-shutter very nearly closed, appeared multiplied, so as to consist of three parallel lines almost equal in brightness, and on each side four or five others, much fainter, and growing more faint, coloured and indistinct the farther they were from the middle line. When the hairs were thicker, but at the same distance apart, or number in the inch, so as to diminish the opening between them, the middle lines were less bright, but the others stronger and more distinct; and he could count six on each side of the middle line seeming to be equally distant from each other, estimating the distance from the centre of the one to the centre of the next. In this experiment, the hairs were the 190th of an inch in diameter, and were regulated by passing them over the threads of a screw of 106 turns in the inch: so that the opening or interval between hair and hair was $\frac{1}{210}$ th of an inch. The middle line was well defined and colourless; the next two were likewise pretty well defined, but something broader, having their inner edges tinged with blue and their outer with red. The others were more indistinct, and consisted each of the prismatic colours in the same order; which, by spreading more and more, seemed to touch each other at the fifth or sixth line; but those nearest the middle were separated from each other by very dark lines much broader than the bright lines.

From experiments made by applying this frame of parallel hairs before the object-glass of a small telescope furnished with a micrometer, he found that the position of the two nearest

nearest side-lines indicated an angle of deflection equal to $7^{\circ} 45''$; and, from estimate founded on the appearance of the lines, he judged that the other streaks were deflected in angles of double and triple the same original angle.

After this ingenious philosopher had proceeded so far, and found a ready solution for the facts he had observed, by referring them to the Newtonian doctrine of inflection, he went on, with more haste than precision, to account for the appearance exhibited by the handkerchief. He remarks, that, if the handkerchief be held before the eye, so that its threads may be disposed horizontally and perpendicularly, and a luminous spot be viewed, the perpendicular threads will, by inflection, on the principle explained in his frame of hairs, cause it to appear as a row or horizontal line of spots, for example, five; and that the horizontal threads will, on the same principle, convert every one of these into five perpendicular rows; by which means, a square figure, composed of twenty-five luminous points, will be seen, of which he gives the figure.

On these last inductions it may be sufficient to remark, that though his frame of parallel hairs might have converted a real row of luminous points into such a square, yet it is inconceivable how the same effect could have followed, with regard to rays of light which must have already passed through the handkerchief in order to receive the first modification. But as Mr. Rittenhouse, from his figure of 25 spots, which are in no case produced by the handkerchief, shews that he did not strictly attend to the phenomena, but wrote in this instance from recollection and reasoning, I shall dismiss these particulars of the original observers, and mention the facts I have myself remarked.

When this problem was first pointed out to me a few months ago by a very active promoter of the sciences, I took the earliest opportunity of looking at a street lamp through a piece of muslin containing one hundred threads in the inch. Instead of one spot of light, there appeared nine; four of which occupied the corners of a square, one the centre, and four others the middle points, in a right line between the corners. Upon examining them with an achromatic perspective magnifying fourteen times, I saw that the spots were true images of the flame of the lamp, the central one being perfect, all the others coloured, with the red outermost, and the corner lights least luminous and distinct. It made no difference in the appearance, whether the cloth was applied close to the object-glass, or at any distance to which the arm could reach before it; or moved sideways in its own plane; but the apparent dimensions of the square were less when the cloth was applied between the object-lens and its focal image, in proportion (by estimate) as the distance of the cloth from the focus was less. When coarser muslins were used, the square was smaller according to the coarseness, but in what proportion was not examined, because it was supposed to follow no very simple ratio. No effect of this kind took place with the object-glass of a microscope.

From these facts I inferred as follows:—The middle flame is formed of all the pencils of light which pass through the central parts of the holes in the cloth without any deflection. The four flames in the middle of the sides of the square, are formed by the pencils of light inflected towards the short threads bounding each square hole in the cloth, assisted by the deflective power of the opposite threads respectively. The corner flames are formed by the combined action of the two threads contiguous to the angles of every hole; the sum of which force, acting in the diagonal with a power varying from the distance of the respective

parts of the lines about the angle, cannot produce an image so clear and definite as the sides. The images are farther asunder the finer the cloth, because the nearness of the threads increases the inflection, inasmuch that when the interval is about $\frac{1}{100}$ th of an inch there will be no central image*. As the images are produced by an equal number of classes of pencils parallel to each other, any motion of the cloth either parallel or perpendicular to its own plane before the object glass will not affect them, because they come to the object-lens under the same circumstances of parallelism. But when the deflections were made by the cloth between the object-lens and its focus, the positions or distance of the images were affected more or less upon the principle of the prismatic micrometer. And, lastly, the microscope does not shew this effect, because the rays from the object, not being parallel, are not uniformly deflected, and have no virtual foci.

I was well satisfied at first with this theory, and still think the greatest part will, upon further examination, prove to be well founded. But upon repeating the experiment with a piece of new cloth made of brass wire about fifty-five threads in the inch, the wire itself being by estimate about the $\frac{1}{100}$ th of an inch thick, other lights became visible, the cause of which does not appear so obvious. Besides the square, consisting of nine lights, there were others much fainter, more coloured, and elongated, as in Fig. 11. In order to discern these to the greatest advantage, the small focal image of the sun was viewed in a concave metallic reflector of two inches focus, by means of the achromatic perspective before mentioned, at the distance of nine feet, with a power of only eight†. All the images, except that in the centre, were coloured spectra, extremely brilliant and beautiful; the original nine, however, still retaining their pre-eminence. They were fainter and more elongated, the more remote from the centre to which they all pointed; that is to say, an imaginary line drawn through the middle of all the colours of any one of the spectra, would have intersected the central colourless sun. Nevertheless their arrangement with regard to each other was not radial, but in lines forming prolongations of the sides of the middle square, as in the figure of the cloth: these lines were parallel to the threads. The interval between each range of images, measured from the centre, was greater the farther off, but not regularly increasing; those near the middle square being almost equal. Seven ranges were counted beyond the square on each side, and a very long triple line of whitish faint light extended still farther. There were also a great number of much fainter images irregularly scattered in the angular spaces between the crosses, all pointing to the centre, and produced, as I conjectured, by irregularities in the cloth.

As the distance of the cloth before the object-glass did not affect the pencils of light, it readily occurred, that by inclining its plane to the axis of vision in such a manner as that one set of the wires might still remain perpendicular to that axis, the other set would apparently crowd together, and diminish the openings through which the rays were to pass. In this manner the cloth might be made, in effect, of any required fineness. When the cross wire or web of the cloth was inclined to the axis, that part of the cross in Fig. 11 which corresponded with the direction of the inclined wires, had the intervals of its spectra enlarged, at the same time that they became broader and more coloured, but fainter, and in particular the central colourless image nearly vanished. I have no doubt

* Newton's Optics, III. obs. 6.

† This was for the sake of light, as the chamber was not darkened. The object-glass of 9 inches focus and 1 inch aperture bears a magnifying power of fifty, without producing any colour.

but it would have disappeared totally, if the want of perfect flatness in the cloth had not prevented its being inclined beyond a certain angle with regularity. The branches of the crofs at right angles to the inclined wires also nearly vanished.

But when the warp or longitudinal wires were inclined to the axis of vision, the images were altered in their relative brightness and position, in a manner which it would be too prolix to relate. These changes terminated in an hexagon with six radii, as in Fig. 12. when the inclination was about 45 degrees. In this, as in all the changes, the spectra were coloured with the red farthest from the centre, and they all pointed to the centre. This unexpected appearance led to an examination of the cloth by a simple magnifier in both positions of inclination, when it was found, that in the first described position the interstices of the wires appeared to be nearly long squares, or parallelograms; but in this last described position the threads of the warp being in reality bended by the operation of weaving over the weft, in angles of about 35° out of the plane, did apparently meet, so as to leave apertures of the form of equiangular triangles, and no doubt caused the hexagonal combination in Fig. 12.

After this imperfect description of a phenomenon of some curiosity, I must decline entering into any discussion of the consequences to which it seems to lead. Sir Isaac Newton's Experiments on Inflection, with the edges of two knives, are simple, and at first consideration do not appear difficult to be understood. Yet, simple as they are, there are certainly many variations yet to be made, and admeasurements to be taken, before the popular explanations of the mutual agency of bodies and light upon each other can be admitted without hesitation. This process of examination affords some peculiar facilities, on account of the great quantity of light which is similarly affected, and the possibility of applying a magnifying power and micrometer. Subsequent meditation may perhaps suggest the cause why the radii in these figures are triple, and parallel to each other, while all the spectra individually diverge from a centre: but I have rather chosen to refer them as they are to the examination of philosophers. And I am the more induced to hope that this examination will be made, when it is considered, that the appearance presents itself through any fine muslin or cambric to the naked eye, though still better with the opera-glass or periscopes, which are in the hands of every one, and that the wire-cloth of fine sieves or wine-strainers is a material easy to be procured.

III.

Description of an Instrument which renders the Electricity of the Atmosphere and other weak Charges very perceptible, without the Possibility of an equivocal Result.

IN the year 1787, when the electrical doubler engaged the attention of philosophers, for its astonishing power of magnifying the minutest quantity of simple electricity, and the subsequent discovery of its spontaneous electricity had greatly reduced its apparent utility, I had the pleasure of a conversation with the Reverend Abraham Bennet, of Wirksworth, the inventor, who shewed me his method of depriving this instrument of much of its adherent electricity, by working it for a time with all its parts in communication with the earth. But at the same time he remarked, that if he were to make an instrument in which this

electricity

electricity should be totally removed, he would have recourse to a simple multiplier, and not a doubler. I did not apprehend the contrivance from this slight description, which at my request, however, he readily extended, and convinced me of the full value of his invention. I then mentioned my intention to construct an instrument on that principle, which soon afterwards I did. It was shewn to Sir Joseph Banks and his friends, at his house, nearly at the same time, and in the same year transferred to the celebrated Mr. van Marum of Harlem, who now possesses it. From various other avocations, I was prevented from causing any others to be made. It is not therefore wonderful that the same thought should since have occurred to so great a master of the subject as M. Cavallo, who in the third volume of his *Electricity*, published in 1795, gives a description and engravings of an instrument very different in form, but the same in principle. The form I have given it, which is all the share I had in the contrivance, is more calculated for a speedy repetition of the process than M. Cavallo's: but his instrument is much easier to be made by one who is not professionally a workman. Mr. Bennet's notion with regard to form was very different from either. Both instruments have the property, that a simple or small aggregate of electricity will not be multiplied unless its intensity exceed that of the adherent electricity of the plates, supposed to be contrary; and both are capable of destroying their own electricity, and exhibiting unequivocally what they receive by communication, however weak it may naturally be, provided the supply be kept up. They are both also limited as to the extent of their multiplication; that is to say, the effect will be denoted by a fixed number or multiplier, so long as the nearest distance of the plates continues unaltered. For example, if this number were 100, the instrument would shew no electricity which was less intense than the $\frac{1}{100}$ th part of what the electrometer demanded to act upon it. This coefficient is nevertheless capable of being enlarged at pleasure, by adjusting the plates to a less distance asunder.

Fig. 8. represents a vertical section of the instruments. A is a metallic vase, having a long steel axis which passes through an hole in the stand H at K, and rests on its pointed end in an adjustable socket at C. The use of the vase is, by its weight, to preserve, for a considerable time, the motion of spinning which is given by the finger and thumb applied to the nob at the top of the instrument. The shaded parts D and E represent two circular plates of glass nearly $1\frac{1}{2}$ inch in diameter. The upper plate is fixed to the vase, and revolves with it; the lower is fixed to the stand. In the lower plate are inserted two metallic hooks, diametrically opposite each other, at F and G. They are cemented into holes drilled in the edge of the glass, which is near two-tenths of an inch thick. In the upper plate are inserted in the same manner two small tails of the fine flattened wire used in making silver lace. These tails are bended down so as to strike the hooks in the revolution, but in all other positions they remain freely in the air without touching any part of the apparatus. At C is a screw, which by raising or lowering the vase keeps the faces of the glass plates from each other at whatever distance may be required. The faces of the glass plates which are opposed to each other are coated with segments of tin foil, as represented Fig. 9 and 10, the latter of which represents the upper plate. Each of the tails communicates with the tin foil coating to which it is contiguous, as does also the hook F with that coating of the lower plate nearest to it. But the hook G is entirely insulated from the whole apparatus, and is intended to communicate only with the electrified body or atmospherical conductor L.

The lower coating nearest to G is made to communicate permanently with the stand H, and consequently with the earth.

In this situation, suppose the motion of spinning to be given to the apparatus, and the effects will be these :—One of the tails will strike the hook G, by which means the upper coating annexed to that tail will assume the electric state of L by communication. But this state, on account of the proximity of the lower uninsulated plate to which it is, at that instant, directly opposed, will be as much stronger than that of L, as a charge exceeds simple electrization. The tail G with its plate or coating proceeds onward, and after half a revolution arrives at the situation to touch the hook F. The upper coating, the lower on the side of F, the hook F itself, and the tail V, must then constitute one jointly insulated metallic mass, in which no charge subsists, but which is simply electrified by the whole charge received at G. And of this mass the surfaces of the plates themselves, constituting the electric well of Franklin, will throw out all their electricity to the hook and tail. But the coating and its tail instantly pass round, leaving F electrified, and proceed to bring another charge from G and deposit it as before. The balls at F are therefore very speedily made to diverge. It is scarcely necessary to remark, that the two upper coatings do nothing more than double the speed of the operation; one of the tails being employed in collecting, while the other is depositing: and that the gold leaf electrometer may be advantageously substituted for the cork-balls.

The instrument I caused to be made was five inches high. The receiving side G was connected with a coated jar of four square feet coating, and the giving side F was connected with Benner's gold leaf electrometer. The electrometer was rendered as strongly positive as it was capable of being, and the jar was rendered negative, by giving it as much of that power as was produced by drawing a common stick of sealing-wax once through the hand. In this state the jar was incapable of attracting the finest thread. The vase was then made to spin; and the effect was, that the leaves of the electrometer first gradually collapsed, and then in the same manner gradually opened, and struck the sides of the glass of the electrometer with negative electricity. The experiment was renewed and repeated with every requisite variation.

IV.

Observations on the Art of Printing Books and Piece Goods by the Action of Cylinders.

— *Experto credite.*

WE may conceive three ways of delineating figures, or writing. The first and most ancient consists in making the traces successively by a brush, a pen, or other instrument. This is design, painting, or writing. In the latter methods, either the whole or the greater part of the figures are made by the action or pressure of an original pattern against the material intended to be written or painted upon. It is the art of printing. The colouring matter is either deposited from the face of prominent parts of the original form, which is usually called a block or type; or else it is pressed from cavities cut in the face of the original, which in this case is called an engraved plate. Most books are
printed

printed from original patterns, in relief; and most of the imitations of paintings are performed by means of engravings. These arts are most frequently distinguished by the names of letter-press and copper-plate printing.

It can scarcely be matter of new information to those who are but moderately acquainted with the state of the Arts, to be told that letter-press or book-printing is performed by an assemblage of single metallic letters, called types, made of lead hardened by an addition of antimony in the metallic state; that these letters are composed in the form of book pages, and wedged together in iron frames called *chases*; that the ink is a composition of linseed oil and lamp black, of so singular a nature, that it will adhere to a ball covered with a pelt or sheep's skin soaked in water, and kneaded to extreme softness under the feet, but quits this skin with great readiness to apply to the face of the letter when dabbed with the ball; and still more, that it almost totally quits the letter to adhere to paper rendered semi-transparent by soaking in water; or lastly, that the paper is applied and pressed against the form of composed letter by means of a flat piece of wood urged downwards by a screw. These and numerous early discovered principles of this most useful art are generally known, and require no more than mere recapitulation in this place.

The genius of the Chinese language not permitting that people to analyse its sounds into an alphabet, as has been done by most other nations, has induced them to retain those signs of things, and of their correspondent words, which probably constituted the first picture or hieroglyphic writings of every rude society. Changed and complicated as these may have become by the rapidity of transcription, the corruption of ignorance, or whatever other causes may have operated through a long succession of ages, they still for the most part use words that properly denote things, and not sounds. Such words cannot, therefore, be subdivided; and it has accordingly been found most convenient, by these first possessors of the art, to print from entire blocks, as was also done by the first printers in Europe. But our artists soon discovered that a few of the simplest characters, namely, the letters of the alphabet, would be in many respects more useful, as the elements for composing blocks for printers, than a number of blocks originally cut for every page of every individual book.

Book-printing, therefore, though in fact of the same nature as block-printing, has been carried into effect by very different machinery from that made use of in the arts which still retain the latter method. In book-printing, the heavy metallic form lies on a kind of table, and the colour and the paper are successively applied to its face: but in block-printing, the block is carried and applied to the colour, and afterwards to the work intended to be printed. Thus, for example, in the printing of paper-hangings, the colour is spread with a brush upon a woollen cloth stretched over a surface of parchment or skin evenly supported by a half fluid mass of water and mashed paper. To this the block is carefully applied by a slight perpendicular stroke or two; after which it is applied to the dry paper on a table, and pressed against it either by one or more blows with a mallet, or by the regular action of a lever. The mechanical part of calico-printing is effected nearly in the same manner; but with smaller blocks, because of the greater difficulty of making the successive fittings on so flexible a material. And in both these arts, as well as in book-printing, in red and black, the variety of colours are produced by repeated applications of forms or blocks, of which the prominent parts are made to fit each other according to the nature of the design.

In the art of printing from copper-plates, a colour somewhat more fluid than for book-printing is made use of. It is pressed into the cavities of the plate by smearing it over the surface; and by subsequent careful wiping the redundant colour is cleared away. In this state, if soaked paper, for which purpose the most spongy texture is the best, be strongly pressed against the plate, by passing both together between two cylinders of metal or hard wood, properly defended by woollen cloth, the greatest part of the colour adheres to the paper, and forms what is called a print.

In all these processes, it is easily seen, that in the successive applications of colour, the accurate filling of the form or original with the material intended to receive the impression, and in various other parts of the manipulation, there is much room for the display of skill, or for injury from the want of it. It may moreover be collected, that the motions attendant on the various steps of manufacture are in many instances difficult to be performed with rapidity and ease, until by long continued habit the workman himself is converted as it were into a machine. A very slight degree of attention to this subject must also shew that, if the originals were of a cylindric form, with a contrivance for regularly applying the colour and performing the subsequent operations, it would be easy to print books and piece-goods with a degree of rapidity and uniformity, of which the usual method of successive applications seems scarcely capable without uncommon care and skill. This obvious conclusion has no doubt led to numerous experiments; none of which, as far as I can gather, whatever may have been their particular utility, have given much promise to supersede the ordinary methods. But as the increased demand for the manufacture of printed goods has rendered such an improvement an interesting object to manufacturers, as well as to those indefatigable artists who have directed their efforts towards improvements; and as the latter generally take up a new object under a strong persuasion that it has not before been pursued by others, it will certainly be of advantage to these deserving classes of men, to relate a few of the difficulties of this new art.

The difficulties attendant on any improvement in the arts may be considered either as moral or physical. Under the moral, I would class every thing that relates to the prejudices of men in favour of the old methods, and their fears of risk, together with the æconomical and commercial inconveniencies attending the new processes. The physical difficulties are such as attend the actual performance of any project after the same has been carefully arranged in the mind of the inventor. It happens unfortunately here also that the inventor is seldom aware of the moral impediments, but almost always concludes, that if he can succeed in accomplishing the purpose he has in view, his cares and labour will then be at an end; and that the manufacturer, in particular, instead of pointing out new impediments discernible only from long continued experience, will more readily embrace and approve of the new processes, in consequence of his superior knowledge of their intrinsic value.

Every good invention appears simple in the prospect, but it scarcely ever happens in the execution that the most direct road is taken; and in every case there will infallibly be many things unknown or unforeseen, which practice only can point out as necessary to be done for the complete accomplishment of the object in view. Hence, and likewise because few men possessed of independent fortune are likely to engage or persevere in a labour of this kind, it almost invariably happens that the expences exceed the ability of the inventor himself. For these and other reasons, new undertakings are generally brought forward by the inventor,

inventor, a man strongly prejudiced in favour of his leading pursuit, together with a moneyed friend, who hopes speedily to increase his capital from the abilities of the other. It is not necessary in this place to describe the usual consequences of a partnership, where the minds, the views, and the circumstances of both individuals are so very different, and which may be modified still more essentially if either of the parties be deficient in the common principles required to bind men to each other. It is certainly of the highest importance to both, that the circumstances of such connections should be very maturely weighed before they are entered into.

The commercial difficulties or facilities attending any invention are also of great consequence. Every inventor ought to enquire, not only what has been done before, but likewise into the present state of the manufacture he means to improve. In this way it is ascertained how small a part the mere press-work constitutes in the price of a book. He will find that twelve yards of paper-hangings are printed for one penny, in a single colour, by hand, which afterwards by the accumulation of price, in paper, colour, duty, and ordinary profit, are sold for three shillings; none of which the inventor can pretend to diminish: and if he could annihilate the whole labour, his advantage would therefore be less than three per cent. without reckoning the cost and operation of his machinery. In the callico-printing, with a more expensive material, dyeing and field processes, duty and profits of manufacturer and vender, the price of laying the block will turn out to be an object still less considerable. Again: it will be seen that small flat blocks cost but little money, in comparison with cylinders of sufficient diameter to retain their figure, and long enough to apply to the whole breadth of the cloth.

Under these and other similar points of view, the inventor, who may consider the subject in a superficial manner, would be ready to abandon his undertaking. But this again ought not to be rashly done. It is true, that where the great force of capital is employed on objects not comprehended within his project, the saving, however large in its absolute amount, or desirable to a manufacturer, will scarcely come within the reach of the inventor by any bargain he can make short of an actual partnership. But it may be possible to separate the respective departments of a manufactory. A spinner is not necessarily a weaver, nor a printer a linen-draper or a dealer in paper-hangings. The several departments of manufacture and commerce are, generally speaking, in the hands of acute men, who seldom reason ill with regard to the advancement of their peculiar interests; and these departments are continually fluctuating in their arrangement, as convenience, profit, or the accumulation of capital may lead. Experiments are for ever on foot, from day-work to piece-work, and from piece-work to the employ of master-workmen with others under them, all supported by the capital of the large manufacturer, who himself in many instances is the mere instrument maintained by the advances or acceptances of the warehouselman, the factor, or the merchant. An inventor who has not capital may seek for employ on the goods or the capital of others; and if he has skill to maintain his ground against the numerous enterprises which the activity of opposite interests will raise against him, he will find that the old order of things will readily alter, as soon as an evident interest in favour of the new is shewn by actual and continued proofs in the market.

Most of the physical difficulties attendant on any new process are such as experience only can shew. Thus, in the forging of iron by the pressure of rollers instead of ham-

mers,

mers, a scheme upon which many thousands of pounds have been expended in this country, it was apprehended that the more impure parts, which are also the most fluid, might be pressed out by the action of cylinders, with equal or perhaps more advantage than by that of hammers; at the same time that the determinate figure of bars of any required size might be given without skill in the operator. Experience nevertheless has shewn, that the more fluid part is driven out much more effectually by the sudden action of a blow, than by the slower compression of a cylinder, which allows time for much of the fluid matter to extend itself within the mass. Various similar effects present themselves when cylinders for printing are substituted instead of planes. Instead of the action of dabbing, the colour is usually applied by simple and gradual contact, to much less effect; and the impression, though not essentially different from that of the block, is performed by a gradual action, which affords time for the cloth or paper to fold itself in a minute degree into the cavities of the sculpture. Hence it is found that the length of paper or cloth printed from a cylinder by a definite number of revolutions, will be greater or less than another piece manufactured precisely in the same way, but with a less or greater degree of pressure. In a block this defect is much less, not only from the considerable hold it takes upon the surface of the material, but also because the error is rectified at every successive application. One of the chief difficulties of cylinder printing consists, therefore, in the difficulty of laying one colour after another; and this would continue to be so even if the materials were not susceptible of change, the contrary to which is the fact. There are two projects for obviating this. The one consists in confining the whole piece to a long table, or to the circumference of a large cylinder, and causing the printing cylinder to move, not by the successive apposition of its carved surface, but of a bearing face regulated by a toothed wheel. The other method consists in the use of a frame to confine two or more cylinders, each provided with its own toothed wheel, and revolving against a large clothed cylinder provided with a suitable wheel to drive the others. The piece is caused to pass between the large cylinder and the others, in order to receive the impression. With regard to the first of these methods, it does not appear easy to confine paper, and still less cloth, in such a manner that its parts may continue without shift or wrinkle during the action of a cylinder, which not being allowed to roll without the check of a wheel, must draw the surface either the one way or the other. The difficulty of confinement will be very much increased, by the indispensable requisite that the paper should be afterwards hung up to dry, and the callico be carried to the dye-house and the bleach-field, between the successive impressions, by which means the dimensions of both will be greatly altered. In the second method, it is observable that no colours can be printed but such as fall clear of each other. In this way, moreover, the gathering action of the cylinders may prove very mischievous. For, if we suppose the paper or cloth to pass between the great cylinder and the first printing roller by an action of the latter which tends to make it slip forward on the face of the great cylinder, and that when it arrives at the second printing roller it there experiences an action of a contrary nature, the consequence will be, that the material will become slack between the two rollers, and the fittings will be false. Not to dwell on that experience which brings forward this obstacle among others, its great probability may be deduced from the allowable supposition that the circumference of the first printing cylinder should be one thousandth part of an inch too large, and that of the second the same quantity

too small. For in this case the material will be shifted one-twentieth of an inch in fifty turns by the first cylinder, and the same quantity in the contrary direction by the second; a quantity upon the whole quite sufficient to destroy the effect of the colours in the progress of one single piece. Such minute differences can hardly be avoided in the first instance; in addition to which we may place the varying dimensions of the printing cylinder, if not made of metal; and of the great clothed cylinder, which in effect has a larger or smaller diameter in proportion to the pressure which operates to render its elastic covering either thicker or thinner. The only method of diminishing these evils seems to be, that all the printing cylinders should by dimension or pressure, or both, be made to draw the same way, the outer cylinder most, and the others gradually less and less, so that the material should have a tendency to apply itself more tightly during its passage through the apparatus.

The application of the colour to the surface of a cylinder block is attended with some difficulty. An ingenious mechanic may contrive various means to produce the action of dabbing, if required. When a stuffed cylinder covered with cloth is made to revolve in the colour, and thence after passing a scraper to apply itself to the block cylinder, it is found to be no inconsiderable difficulty that its dimensions change, and its covering becomes wrinkled by the action of the scraper as well as that of the block. A better method, therefore, consists in a revolving web of woollen cloth, like a jack-towel, stretched over three horizontal cylinders parallel to each other, two of which support the elastic surface of the web, which in its revolution accompanies the block cylinder, and the other serves to guide the same web to the colour, or a cylinder revolving in it. This method would be very easy and pleasant in its operation, if it were not for a property common to all straps which revolve on the surface of two or more wheels. These are observed always to seek the highest place; so that if a cutler's wheel were made with a groove to carry a strap, instead of a round edge, the strap would infallibly mount the ledge, instead of remaining in the groove*. On this principle, the web would very speedily shift itself to one end of the cylinders, if it were not confined sideways, or the lower roller were not made considerably thickest in the middle, and gradually tapering towards its extremities. This last simple expedient is not without its difficulties; but as I have not actually tried it, I shall defer entering into any discussion on that head.

The running of the paper or piece-goods towards one end of the leading cylinder is also one of the greatest difficulties attending this method of printing. It is not perfectly removed by tapering the leading cylinders.

The nature of the trade of paper-staining in this country, which requires a large sum to be immediately vested in the payment of the excise duty, and consequently prevents any considerable stock from being manufactured until orders are actually received, and the varying fashions in printed calicoes, which render the expence of cutting the block by far the heaviest part of the disbursement for printing, are probably the chief reasons why manufacturers in this country have been less solicitous for the construction of machines calculated to afford profit only in the case of very numerous impressions. The physical difficulties of this art have likewise conspired, in no small degree, to prevent its having been applied in the large way to any but a few simple designs of the sort called running patterns in one colour.

* This curious effect arises from a flexure produced in the edge of the strap, by the elevated part of the wheel, which throws the advancing part of the strap more and more towards that elevation. It cannot be explained in a few words; but may easily be seen by wrapping a straight slip of paper round an extinguisher or any other cone.

An Account of the Diamonds of Brazil. By M. D'ANDRADE.

AS the Society * is desirous of an account of the Diamonds of Brazil, I shall endeavour to satisfy them to the utmost of my power; but previous to a description of their form, the place where they are found, and the manner of searching for them, I think it will be useful to convey an idea of the country in which these diamonds are found.

The province of Brazil which produces diamonds is situated inland between $22\frac{1}{2}$ and 16 degrees of south latitude. Its circumference is near 670 leagues. On the east it is limited by the captainry or province of Rio Janeiro; on the south, by that of St. Paul; on the north, by the *Sertoens*, or interior part of the maritime province of the bay of All Saints, and part of that of the mines of Goyarel; on the west, lastly, by another part of the last-mentioned province, and by those deserts and forests which are inhabited by the savages, and extend to the frontiers of Paraguay. On the side nearest St. Paul there are vast uncultivated plains; the interior is divided by chains of mountains and hills, with superb valleys and luxuriant fertile plains. It abounds with wood, and is watered by a great number of rivers and brooks, that facilitate the working of the mines of gold, which is obtained by washing in spangles from the river sands, or in veins open to the day. This province is divided into four *comarcas* or districts, which, reckoning from north to south, are *Santo Jeao del Rei*, *Villa Rica*, *Sabara* and *Sero Dofrio*, or cold mountain, called in the language of the savages *Yritauray*. The diamonds are found in this last district. The whole province is very rich in the ores of iron, antimony, zinc, tin, silver and gold.

The Paulists and inhabitants of the ancient captainry of St. Vincent were the first who discovered these mines, and peopled in great part the whole of this rich province, as well as those of *Mato Grosso*, *Cuiaba*, *Goyares*, and *Rio grande de San Pedro*. In a word, almost the whole of the interior of Brazil, with its immense riches, would have been still unknown but for them. The metropolis at present enjoys the fruit of their eccentric activity and hazardous discoveries. Constantly with their arms in their hands to defend themselves against the savages, in the midst of impenetrable forests and solitary wastes, exposed for twelve years to famine and the inclemency of the seasons, they overcame every obstacle: nothing could check their unconquerable spirit. There is not a single mountain, brook, or mine, which has not been traversed, discovered, and visited by them. Antonio Soary, a Paulist, who gave his name to one of these mountains, was the first who discovered and visited the *Sero Dofrio*. Gold only was sought for, but at last diamonds were discovered in the *Riacho Fumbó*, whence they were first obtained, and afterwards in the *Rio de Peire*; a great number were likewise obtained from the *Giguitignogna*, a very rich stream; and lastly, at the end of 1780 and beginning of 1781, a gang of near three thousand interlopers, called *Grimpeiros*, discovered diamonds, and obtained an immense quantity from the *Terra de Santo Antonio*: but they were forced to abandon this spot to the Royal Farm, who took possession of it. Then it was that the suspicion was confirmed, that the mountains are the true matrix of diamonds; but as the work in the beds of rivers and on their banks is less tedious, can be conducted on a larger scale, and affords larger diamonds, the

* The Society of Natural History of Paris. This account is inserted in the *Annales de Chimie*, XV. 82, from which this translation is made.

Farm abandoned the mountains, and formed great establishments in the river of *Toucan-birnen*, which flows through the valleys of this chain, and is near ninety leagues in length. It was found by examination and digging, that the whole surface of the ground, immediately beneath the vegetable stratum, contained more or less of diamonds, disseminated and attached to a matrix ferruginous and compact in various degrees, but never in veins, or in the divisions of geodes.

Attempts were made at first to prohibit the working; but the activity of individuals who infringed the order of Government, and sent home diamonds by the shipping from Brazil, under the denomination of oriental diamonds, induced Government to establish a farm. The first farmer was Riberito Caldera, a Paulist, with the condition that no more than 600 negroes should be employed in this work. This condition has always been evaded; for the number of slaves employed are from six to eight thousand: and this number was scarcely diminished when the Portuguese Government, to put an end to this fraud and the depreciation in price of diamonds proportional to the quantity brought to market, caused the undertaking to be carried on for its own account. But at present, from other considerations, it is farmed again to individuals. Notwithstanding the great profits which enter the royal treasury, it is certain that the inhabitants of the province are greatly injured by it, because the District of Diamonds being continually enlarged, has condemned to destructive repose immense tracts very rich in gold.

Let us now proceed to the diamonds.

The figure of the diamonds of Brazil varies. Some are octahedral, formed by the union of two tetrahedral pyramids. This is the *adamas octædrus turbinatus* of Wallerius, or the octahedral diamond of Romé de l'Isle. These are almost always found in the crust of the mountains; others are nearly round, whether by a peculiar crystallization or by rolling. They resemble those oriental stones which the Portuguese and the natives of India call *reboludos*, which signifies rolled. And lastly, others are oblong, and appear to me to be the *adamas hexædrus tabellatus* of Wallerius. The two last are usually found in the beds of rivers and broken places of their banks.

Diamonds are also found, as I have remarked, in the crust or external covering of mountains. These masses are formed of a bed of ferruginous sand, with rolled flints, forming an ochreous pudding-stone from the decomposition of emery and muddy iron-ore; it is called *cascalho*, and the beds or strata *tabeleiros*. These *tabeleiros* have different names, according to their situation or their nature. When the stratum is horizontal, and in the plane of the bed of the river, it is properly a *tableiro*; but if it rises in banks, it is called *gopiara*; lastly, if the pudding-stone contains much emery, it is then denominated *taban-bua cauga* in Brazilian, that is to say, black-stone, or iron-stone.

In some places the *cascalho* is uncovered; in others, it lies beneath a kind of vegetable muddy earth, *humus damascena* Linn. or beneath a reddish fat sand, which sometimes contains rounded flints. This happens in the returns of the mountains, or upon the banks of great torrents. This sand is called *pisarra*. The bank or stratum beneath the *cascalho* is either shistus rather sandy, or the solid bog-ore of iron. It is likewise in the *cascalho* that gold in spangles and in pyrites is found; the former of which is in my opinion afforded by the decomposition of the auriferous pyrites. For the gold in veins has another form,

and its matrix is either fat quartz or fine-grained tender *cos*, micaceous gneis, or the quartzose ore of iron, *tephus ferreus* Linn.

The exploring of diamonds is performed by changing the beds of streams, in order that the sand or gravel may be washed, and the diamonds selected; or by breaking the *capitals* with large hammers, and afterwards washing it in troughs. This washing differs from that of gold, because it requires a small quantity of very clear water, and very little of the *calambis* at a time; proportions which are precisely contrary to those required in washing gold. Black slaves are employed in this business, entirely naked excepting a cloth round their middle, in order that they may not embezzle any of the diamonds; but in spite of every precaution, and the vigilance of numerous inspectors, they nevertheless find means of concealing them, which they sell at a very low price, to the interlopers, for tobacco and rum.

This is all the information I can with certainty state respecting diamonds. I have only to remark, that other provinces likewise afford them; as Cuiaba, and the districts of Guara-puara, in the province of St. Paul; but these parts are not explored.

VI.

Abstract of the Specification of MR. WILLIAM DESMOND'S new Method of Tanning; with Observations relative to that Subject.

EARLY last year * a patent was taken out by Mr. William Desmond for the method or process of tanning practised, as it is said, with great success in France by Seguin. The substance of Mr. Desmond's specification is as follows:

The art of tanning consists in impregnating skins with a principle obtained from tan, with which they form a compound, insoluble in water, and possessing other qualities well known in the substance called leather. He obtains the tanning principle by digesting oak-bark, or other proper material, in cold water, in an apparatus nearly similar to that used in the saltpetre works. That is to say, the water which has remained upon the powdered bark for a certain time, in one vessel, is drawn off by a cock, and poured upon fresh tan. This is again to be drawn off, and poured upon other fresh tan; and in this way the process is to be continued to the fifth vessel. The liquor is then highly coloured, and marks, as Mr. Desmond says, from six to eight degrees on the hydrometer for salts †. He calls this the tanning lixivium. The criterion to distinguish its presence is, that it precipitates glue from its aqueous solution, and is also useful to examine how far other vegetable substances, as well as oak-bark, may be suitable to the purpose of tanning. The strong tanning liquor is to be kept by itself. It is found by trials with the glue, that the tanning principle of the first digester which receives the clear water, is, of course, first exhausted. But the same tan will still give a certain portion of the astringent principle, or gallic lixivium, to

Jan. 15, 1795.

† Probably that of Baumé, described in his *Elémens de Pharmacie*. For the corresponding specific gravity in the usual form, see p. 39 of this Journal.

water.

water. The presence of this principle is ascertained by its striking a black colour when added to a small quantity of the solution of vitriol of iron or green copperas. As soon as the water from the digester ceases to exhibit this sign, the tan is exhausted, and must be replaced with new. The gallic lixivium is reserved for the purpose of taking the hair off from hides.

Strong hides, after washing, cleaning, and fleshing, in the usual way, are to be immersed, for two or three days in a mixture of gallic lixivium and one thousandth part by measure of dense vitriolic acid*. By this means the hair is detached from the hides, so that it may be scraped off with a round knife. When swelling or raising is required, the hides are to be immersed for ten or twelve hours in another vat filled with water and one five-hundredth part of the same vitriolic acid. The hides being then repeatedly washed and dressed, are ready for tanning; for which purpose they are to be immersed for some hours in a weak tanning lixivium of only one or two degrees; to obtain which, the latter portions of the infusions are set apart; or else some of that which has been partly exhausted by use in tanning. The hides are then to be put into a stronger lixivium, where in a few days they will be brought to the same degree of saturation with the liquor in which they are immersed. The strength of the liquor will by this means be considerably diminished, and must therefore be renewed. When the hides are by this means completely saturated, that is to say, perfectly tanned, they are to be removed, and slowly dried in the shade.

Calf-skins, goat-skins, and the like, are to be steeped in lime-water after the usual fleshing and washing. These are to remain in the lime-water, which contains more lime than it can dissolve, and requires to be stirred several times a-day. After two or three days, the skins are to be removed, and perfectly cleared of their lime by washing and pressing in water. The tanning process is then to be accomplished in the same manner as for the strong hides, but the lixivium must be considerably weaker. Mr. Desmoud remarks, that lime is used instead of the gallic lixivium for such hides as are required to have a close grain; because the acid mixed with that lixivium always swells the skins more or less; but that it cannot with the same convenience be used with thick skins, on account of the considerable labour required to clear them of the lime, any part of which, if left, would render them harsh and liable to crack. He recommends, likewise, as the best method to bring the whole surface of the hides in contact with the lixivium, that they should be suspended vertically in the fluid by means of transverse rods or bars, at such a distance as not to touch each other. By this practice much of the labour of turning and handling may be saved.

Mr. Desmoud concludes his specification by observing that in some cases it will be expedient to mix fresh tan with the lixivium; and that various modifications of strength and other circumstances will present themselves to the operator. He affirms that, in addition to the great saving of time and labour in this method, the leather, being more completely tanned, will weigh heavier, wear better, and be less susceptible of moisture than leather tanned in the usual way; that cords, ropes, and cables, made of hemp or speartery, impregnated with the tanning principle will support much greater weights without breaking, be less liable to be worn out by friction, and will run more smoothly on pulleys; inasmuch that, in his opinion, it will render the use of tar in many cases, particularly in the rigging of ships, unnecessary; and, lastly, that it may be substituted for the preservation of animal food instead of salt.

* Marking 66 degrees on the hydrometer for acids. Sp. gr. 1.848 of Baumé.

The intelligent manufacturer will readily perceive that this new method is grounded on two particular circumstances, besides a more scientific management of the general process than has been usual. The first consists in the method of determining the presence and quantity of the tanning principle, by the hydrometer and the precipitation of glue: the second, in applying this principle in a concentrated state, more early in point of time than has, perhaps, been hitherto done. Our tanners, after the common previous processes and unhairing by acids, by lime, or by piling the hides that they may heat and begin to putrefy, apply the solution of tan, which they call ouze, in a great number of pits in the tannery. They begin with the weakest solution, which has been used, and is of a lighter colour than the other. And they pass the hides, according to their judgment and experience, into ouzes which are stronger and stronger, until at last, in certain cases, the hides come to be buried for a certain time in a solid mass of tan or oak-bark. The oak-bark itself, in the pits, is not only the source from which the water extracts the tanning principle; but seems, likewise, in some measure, during the last stages of the process, to operate mechanically by keeping the surfaces of the hides from touching each other.

On the occasion of this apparently important discovery, I applied to one of the first manufacturing houses in the Borough of Southwark, to make enquiries concerning its value. One of the partners, who appeared to have paid considerable attention to the processes of his manufactory, informed me, that the strong solution obtained from tan had been well known to them for some years under the name of essence of tan; and that it had been proposed to use it as the means of bringing out the complete leather in a short time. I asked whether the objection to its use might consist in the outer part of the skin becoming more perfectly tanned in a short time than the inner part; so as to defend this last from the subsequent change that might have taken place by a slower operation. He answered, that this was partly the reason why it was necessary that the ouzes should be gradually applied from the lowest to the highest strength. But the chief reason he urged in favour of the slow process was, that the hides were found to feed and improve in their quality by remaining in the pit. I could not gain satisfactory information what the nature of this feeding and improvement might be. The fact, as loosely stated, appeared to be, that the skin became much thicker, denser, and heavier in the first ouzes during a certain time; and that these advantages of thickness, density, and weight continued to exist in a certain degree to the very end of the entire process. For, as he remarked, they know very well that, by bringing the skins more hastily into the stronger ouzes, they will be sooner converted into leather; and that a skin which ordinarily requires fifteen months, might be tanned in nine. But then, added he, the skin which comes forward in nine months, from its thinness, and other inferior qualities, will afford ninepenny soles for shoes; whereas, if it had undergone the longer process, the soles would have been worth a shilling:—a difference which largely repays the common interest of capital for the excess of time.

On the other hand it may be remarked, that results of this nature require to be grounded on experiments carefully undertaken, under circumstances differing only with regard to the object of investigation, and often repeated. It does not appear from my enquiries that this has actually been the case; and I have been informed that excellent leather has been produced in the new method. But I have heard nothing of its appearance in the market, though upwards of a year has elapsed since this patent was sealed.

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A very full and circumstantial account of this process, which Mr. Desmond affirms he received from a certain learned foreigner, was inserted in the *Moniteur*, and thence translated and published in the English *Courier* some time in the month of August 1795.

VII.

Description and Account of a New Press operating by the Action of Water, on the Principle of the Hydrostatic Paradox. Invented by JOSEPH BRAMAH, Engineer.

FIG. 1, Plate II, is a front section of a common press for books and papers, and the like. Fig. 2 exhibits a perspective view of the same instrument on that side to which the pump is fixed. The letters distinguish the same parts in both. ABCD is the frame. I is a strong metallic cylinder, in which the rammer or piston EF moves. To the upper part of this piston is applied an iron table, by the motion of which upwards the pressure is communicated to the articles H. QR represents a small cistern, containing water. Within this cistern is fixed a small forcing pump, of which K is the barrel, L the piston, M a slide valve formed of metal, and opening inwards beneath the piston. The nature and effect of this valve may be easily understood from the figure. It consists of a metallic rod, at one end of which there is a nob turned conical next the stem, so as accurately to fit the conical face of the hole into which it is put. The tail is filed on one side, so that it does not entirely fill the cylindrical hole which it occupies; by which means a passage is afforded for water when the head of the valve is raised. In the state of inaction, the valve is kept shut by the operation of a spiral spring at the other end of the tail. N is another valve of the same kind opening downwards. O represents the rod of the piston, with a contrivance for keeping a vertical position during the working. The effect of this contrivance will be understood without difficulty by comparing the two drawings. S is the lever or handle for working the engine.

Its action is as follows: When the handle S is raised, it brings up the piston L, which would leave a vacuum beneath if the pressure of the atmosphere did not force the water in through the slide valve M. The lever is then to be pressed down, which causes the slide valve to shut, and forces the water through the bottom valve N, whence it passes through the pipe P into the cavity F of the great cylinder I, and raises the piston or pressing rammer. A repetition of the same process forces more water in, and the pressure may in this manner be carried to a great extent. When it is proposed to relieve the action, the lever S must be pressed down, which, by the mechanical contact of the lower extremity of the piston L against the tail of the lower valve N, keeps that valve open. In this situation, the lever TU is to be pressed towards R, and will open the valve M. Both valves being in this manner opened at once, the passage between the internal part of the great cylinder and the cistern QR becomes free, and consequently the table G and the rammer EF descend by their own weight, and restore the engine to its original situation.

There is no difficulty in computing the force of this instrument. If the diameter of the barrel K be one quarter of an inch, and that of I one inch, that is to say, four quarters of an inch; one pound lodged upon the piston-rod W will be in equilibrio with sixteen pounds lodged upon the table G; the weights of the parts of the engine attached to, and moving with, each piston, being respectively included. And if the length of the lever SY be fifteen inches, and the distance XY between the centres of motion and of action be

two inches, one pound at the end of S will gain an advantage of $7\frac{1}{2}$ times when compared with that at W. Instead, therefore, of sixteen pounds upon the table G being equal in effect to counterpoise this last action, there will be required upwards of 120 pounds. But a man, in this action of pumping by a downward pressure, can without difficulty apply his whole weight, and with great ease one third or one fourth of his weight, suppose 50 pounds. In this case the pressure will be equivalent to fifty times 120 pounds, or 6000 pounds, that is to say, nearly three tons.

To compare this engine with a screw, in theory, we must enquire what fineness of thread and length of lever would afford a purchase of 120 to one. Let us suppose the thread of a screw, substituted in the place of the barrel I, to be one tenth of an inch thick; the distance from the top of one thread to the top of the next will in this case be one fifth of an inch. This is the space through which the weight must rise in one revolution. The power must therefore move through 120 times that space, namely twenty-five inches; but a lever or radius four inches long will describe a circle somewhat larger than this, and consequently such an engine would in theory be equal in power to the hydraulic engine we have been contemplating.

But when the subject is viewed practically, the difference between the two engines appears to be very remarkable. All practical men know how very large a part of the force operating by means of engines is employed in overcoming frictions. Every one is aware of the extreme friction between solids, and the very slight friction which takes place between the parts of fluids. This is seen in the common expedient of oiling the pivots of wheels, and in the very gradual decay of motion in fluid bodies; while solids moving on each other stop at once, as soon as the force is diminished to a certain degree. The screw is an organ peculiarly liable to friction, and this friction is always much greater than the whole of the reacting force; for there are few instances where a screw will return from extreme pressure, when the agency upon the lever is withdrawn. It is also to be considered, that the whole force of the weight or resistance acts directly upon the face of the screw, at which the motion is required to take place. It has not been appreciated in what degree this resistance or friction increases with the weight. In lighter actions the simple ratio has been inferred; but under more severe pressures the two metallic faces extrude the greater part of the half-fluid matter between them, and appear by the magnitude of their resistance to be attached to each other by a process of the nature of cohesive attraction. For these and other reasons, it appears nearly impracticable to form any comparison between two engines so different in principle, but such as shall be deduced from immediate experiment of their effects. I am not in possession of numerical data to indicate the actual power of screw-engines or presses; which are perhaps the less necessary, because those who are the most interested in the success of an improvement like the present, are for the most part able to come at these without difficulty. I intend, besides, to make these the subject of a future communication. The effects which I observed were these.

A machine of the new construction here described, was employed to press some papers. The force applied to the lever was so slight, that the instrument required no fastening to the table on which it stood: but the effect on the upper bar, A B, which was $\frac{3}{4}$ inches thick, was such as bended it out of a straight line upwards of a quarter of an inch, and I apprehend that it might have easily been broken by continuing the pressure. With a screw-

press, the screw of which was iron, and nearly of the dimensions before mentioned, excepting that the lever was twelve inches long instead of four inches, and the action on the lever upwards of two hundred weight, applied with a jerk, the effect was nearly the same. Here I should estimate the advantage to be very much in favour of the hydraulic engine.

In another engine of this kind, the diameter of the great piston was four inches, and of the smaller three-eighths of an inch; and the advantage given by the lever or handle was twelve to one. Above the piston of the great cylinder was applied a long lever, at one end of which was an axis, and at the other end a large scale to hold weights: it contained twenty hundred weight. The distance between the axis of motion of this lever and the part where it acted on the piston was six inches; and the distance from the same axis to the extremity where the scale was hung, was 126 inches. Every hundred weight in the scale consequently pressed upon the piston with a force equal to twenty-one hundred weight; whence the whole pressure was twenty-one tons. It was easy to work the lever briskly with one hand, and each stroke raised the scale near one-third of an inch. Forty-seven pounds hung at the end of the lever, carried it down with a moderate swiftness of working; but a weight of only forty-three pounds remained in equilibrio, and did not descend. Now, as the true weight in theory was thirty-two pounds, as deduced from the action of the parts in the manner already done with regard to the small machine, it follows that less than one-third of the actual power was employed to give velocity and overcome all friction.

It may be remarked, that the principal frictions in these machines must be at the circumferences of the pistons, and that these do not increase in the simple, but in less than the sub-duplicate ratio of the power. For if the diameter of the great cylinder were double, every thing else remaining unchanged, the surface of its piston, and consequently the power, would be quadrupled. But the friction would be only doubled, and that merely at the leathering of the greater piston.

As the pressure in the experiment last mentioned amounted to 47040 pounds upon the great piston of four inches diameter, or sixteen circular inches surface, it amounted to 2940 pounds upon each round inch. But the medium pressure of the atmosphere on a round inch is near twelve pounds, consequently the action was equal to 245 atmospheres: and as each of these corresponds with a column of 34 feet of fresh water at a medium, the water in the cylinder was pressed in the same manner as if the whole column had been 8330 feet, or $1\frac{2}{3}$ mile, long.

Large presses of this construction are made with two pumps of $1\frac{1}{4}$ inch bore, and a cylinder of seven inches. These have been used in pressing hay and cotton for package; and, as I am informed, are effective in producing a greater condensation on the material with a much less application of moving power and consumption of time. But of this and other particulars, as I have not yet had an opportunity of examining the facts myself, I shall forbear to speak at present*.

* Mr. Bramah, who constructs these engines, has obtained a patent for the invention.

VIII.

*The Process for giving a beautiful White Colour to Raw Silk, without Scouring.**By M. BAUMÉ.**

BERTHOLLET, in his *Elémens de l'Art de la Teinture*, published in the year 1791, after describing the usual methods of depriving silk of its resinous or gummy matter†, proceeds to remark, that, in the manufacture of blonds and gauzes, the natural elasticity and stiffness of this article, are required to be preserved; whence it has become a desideratum to render the yellow silk of Europe white like that of China, without depriving it of its gum. He adds, that Mr. Baumé has solved this interesting problem, but had kept his process a secret; but from the facts he had possessed the means of obtaining, it appeared liable to accidents, and that the chief difficulty consisted in giving an uniform white colour when large quantities were operated upon. He also mentions a difficulty in dressing the whitened silk so as to prevent its curling, and observes that it ought certainly to be kept constantly stretched during the drying. It is besides requisite that the spirit of wine should be recovered after the process, which would else be rendered too expensive. This author does not say whether the white Chinese silk is subject to the same inconvenience of curling when dyed, which, it may be remarked, is a property of no consequence where the material is to be applied in the manufacture of white goods. The motives which led Mr. Baumé to communicate his process to the world, originally retained by him as a lucrative secret, do not appear. Whether the mistakes of those who carried it into effect in the large way might have led him to vindicate the reality of his discovery by publication; or whether the commercial advantages derived from superiority of quality and cheapness in his article over the Chinese silk in the market of France, might in the end have proved of less value than the scientific reputation to be derived from its disclosure; are circumstances which will, no doubt, have their proper weight with such manufacturers as may be induced gradually to adopt this process.

The silk of Nankin is perfectly white, silvery, brilliant, and possesses all the elasticity of raw silk. Our author affirms, that the value of this article imported into Europe amounts to upwards of twenty millions of livres‡, of which France consumes about four or five millions in gauzes, blonds, ribbons, &c. This was formerly supposed to be produced of a white colour from the worm. The late Mr. Trudaine, intendant of commerce, procured the eggs of the silk-worm from China, and cultivated them. The produce consisted of yellow cocoons, and others of the most perfect whiteness. The latter afforded silk equal in this respect to that of Nankin. But Mr. Baumé affirms, that most of the Nankin silk is bleached by art, and, as he thinks, by a process similar to his own.

As it is impossible to wind off a large quantity of silk in the short time previous to that of the insects eating their way through the mass, it is usual, in the first place, to deprive them of life. This is commonly done by exposing the cocoons, properly wrapped up, for two hours to the heat of about 158 degrees of Fahrenheit in an oven; after which they are

* The original Memoire from which the facts here related are taken, is inserted in the *Journal de Physique*, xlii. 375—399.

† Tom. i. p. 146; or p. 141 of Dr. Hamilton's translation.

‡ About eight hundred and thirty thousand pounds sterling.

kept for a certain time in a mafs to preserve their heat, and effectually destroy fuch of the insects as might have efaped the power of the oven. The effect of this process is, that the filk is hardened, and is more difficult to wind off than before. Hence the product of filk is lefs by one ninth part in quantity, and inferior in quality to what might have been obtained by winding off without this previous baking. Mr. Baumé, not only from these views, but likewise because the filk which has not been baked proves fufceptible of a greater lustre, was induced to destroy the chrysalis by spirit of wine. For this purpose he difposes them in a wooden box in a stratum fix inches deep: upon each square foot half a chopin, or fomewhat more, of spirit of wine is to be sprinkled with a small watering-pot made for that purpose. This quantity answers fufficiently near to our half-pint. The liquid is to be equally diftributed, but it is not neceffary that all the cocons fhould be wetted. They are then to be mixed by hand. In the next place another stratum is to be formed over the first, nearly of the fame depth, which is to be sprinkled and treated as before. By this method of proceeding, the box becomes filled, and muft then be covered, and left for twenty-four hours, during which time they become fpontaneously heated to about 100 degrees, and the vapour of the spirit of wine exerts itfelf with wonderful activity. Five hundred French pounds* of the cocons require ten French pints, which is nearly the fame number of Englifh quarts. After this treatment they muft be fpread out to dry, which happens in a fhort time, and is abfolutely neceffary previous to winding off.

When the operator propofes in this manner to extinguish various parcels of cocons belonging to different individuals, each parcel may be tied up loofely in a canvas bag, and wetted on the outside previous to clofing the box.

The spirit of wine to be ufed in this operation, ought to be of the ftrength of 34 degrees of Baumé's hydrometer at the temperature of 55 degrees. It is of the greateft importance to ufe that spirit only which has been kept in veffels of glafs, of tinned copper, or of pure tin. Leaden veffels are abfolutely to be rejected; wooden veffels tinge the spirit, which gives the filk a degree of colour of confiderable folidity, and very inimical to the bleaching process.

With regard to the advantages of this method of extinction, in-preference to that of the oven, the author remarks, that the coft of labour and fuel added to the lofs of filk, and the probability of injury from too much or too little heat, conftitute a fum of difadvantage much greater than the coft of the spirit of wine. It is befides a confiderable advantage, that the spirit of wine renders more diftinguifhable fuch cocons as have perifhed previous to the application of the spirit. These afford a much worfe filk, and muft be picked out.

The filk is wound off upon a reel, while the cocons are kept immerfed in water almoft boiling. Upon this part of the process Mr. Baumé remarks, 1st, That the dead cocons muft be feparated. These are known by the brown or black fpoats on their furface. 2. That well-water, which on account of its clearnefs is almoft univerfally ufed in the filk manufactories, moftly contains nitre, and is extremely prejudicial to the bleaching process. The prefence of nitrous acid gives a yellow colour, which refifts bleaching and even fcouring; he therefore recommends river-water. 3. In fome countries a fmall quantity of

* The Paris pound is to the Englifh avoirdupois pound as 756 to 700. I have not reduced these quantities, becaufe the operation requires no great precision. N.

alum is used. Neither this nor any other saline substance is of the least advantage to the colour, beauty, or quality of the silk.

At the four places of contact of the silk upon the reel, all the threads stick together. It is absolutely necessary that this should be remedied. The method consists in soaking the silk in a sufficient quantity of warm water, at about 90 degrees, for about two hours; after which the threads are to be separated by opening the hanks upon a pin, and lightly rubbing the parts which cohere. When the silk is dry, it is to be loosely folded in its original form, and is ready for bleaching.

The silk while wet is soft, and part of its gummy matter is in such a state, that its threads would readily adhere, if wrung while warm for the purpose of clearing it of the water. After such improper treatment there would be no other remedy than to soak it again in warm water.

The apparatus for bleaching the silk consists of a stone-ware vessel, nearly of a conical form, capable of holding about twelve gallons, having a large opening at the one end, and a smaller of about an inch diameter at the other end. Common pottery cannot be used in this operation, because it is soon rendered unserviceable by the action of the marine acid, and the stone-ware itself is not very durable. This vessel must be carefully examined, to ascertain that it does not leak in the slightest degree; after which the inside is to be rubbed with a pumice-stone, to clear it of asperities which might break the threads. A cover of the same material is to be fitted on by grinding; and the smaller aperture, which in the use is the lowest, is to be closed with a good cork, in the middle of which is thrust a small glass tube about a quarter of an inch in diameter; this is likewise stopped with a cork, excepting at the time when it is required to draw off the liquid contents of the jar. A small perforated false bottom is placed within the vessel, to prevent this tube from being obstructed.

This jar, or as many of them as the purposes of the manufactory may require, is supported by a wooden frame or table, at such a height that a cask may be conveniently placed beneath to receive what may flow from the glass tube in the several periods of the operation.

Six pounds of yellow raw silk are to be disposed in the earthen pot; upon this is to be poured a mixture, previously made, of forty-eight pounds* of spirit of wine at 30 degrees, with twelve ounces of very pure marine acid, absolutely exempt from all presence of nitrous acid, and of the strength of 14 or 15 degrees of Baumé's hydrometer. The pot is then to be covered, and the whole left in digestion till the following day, or until the liquor, which at first assumes a fine green colour, shall begin to assume that of a dusky brown (*feuille morte*).

The acidulated spirit is then to be drawn off. To prevent evaporation, M. Baumé thrusts a cork in the bung-hole of the receiving cask, in which is a sliding glass tube. The use of this tube is completely to surround the small tube proceeding from the earthen vessel. When the whole of the fluid is thus almost entirely drawn off, clean spirit of wine is poured upon the silk, and drawn off repeatedly until it passes colourless. The silk is then suffered to drain without stirring it. In this state it is ready for a second infusion.

Forty-eight pounds of spirit of wine acidulated with twelve ounces of marine acid is now to be poured on the silk, and the whole suffered to remain for twenty-four hours or longer, until the silk becomes perfectly white. The time required for this second infusion

* The pound is nearly a pint, and is divided into sixteen ounces. With regard to the strengths, see Article IX. p. 37.

is commonly longer than for the first : it sometimes amounts to two, three, or even six days, according to circumstances, particularly the temperature and the nature of the silk. Silk which has been in the oven is in general more difficult to bleach.

When the silk has thus obtained its utmost degree of whiteness, the acidulated spirit is to be drawn off into a separate vessel. This fluid is but slightly coloured, and may be used again in the first infusion of other yellow silk, with the addition of six ounces more of marine acid. The receiving vessel is to be removed, and another clean vessel substituted in its place. The silk is then sprinkled with clean spirit, and occasionally pressed down with the hand. As soon as the spirit of wine comes off absolutely colourless, a third infusion is to be made by pouring upon the silk forty-eight pounds of the pure spirit without acid, which is to remain till the following day : it is then to be drawn off, and reserved for washing other silk after the first infusion.

After the silk has been left to drain, and affords no more spirit, it still retains its own weight of that fluid. This is recovered by the very simple process of sprinkling the silk with a small quantity of very clear river-water at a time. While the water applies itself and subsides along the silk, it drives the spirit of wine before it, so that the first portions which flow from the tube are scarcely diminished in strength. The addition of water is to be continued until nothing but mere water comes off below.

In this situation the silk is found to be well bleached, but still retains a portion of marine acid sufficient to render it harsh to the touch, and after a time brittle. It must be washed off with water. The best method is to put the silk loosely into a coarse woollen bag, which is to be secured loosely in another cloth like a small bed or pillow, then placed in a basket and left in a running stream for five or six hours ; but where the convenience of a stream is wanting, the earthen pot containing the silk is to be covered with a cloth, and water pumped through it for five or six hours, or until that which issues from the lower aperture gives no red colour to the tincture of tournsol. At this period the lower opening is to be closed and the vessel filled with water, which must be changed once or twice in twenty-four hours.

The time required for washing was occasionally abridged by passing spirit of wine, or river-water impregnated with a small portion of alkali, through the silk. The neutral salt thus produced is in fact less adherent to the silk than the acid itself, but nevertheless requires to be washed off with a very large quantity of water.

In these as in every other process relating to the silk, great care must be taken to ascertain that the water made use of contains no nitrous acid, which would infallibly occasion imperfection of colour, or spots in the article. After this treatment the silk is ready for drying and lustering; previous to the description of which, the author makes several remarks to the following purport :

Though the mineral acids are the most powerful and destructive of all saline substances, yet they may be applied to silk when diluted with spirit of wine in very considerable doses. In trials made to ascertain the maximum, two ounces of marine acid were added to one pound of spirit of wine, without altering the silk. Two drams of marine acid cause a very perceptible alteration in one pound of silk. I suppose he means pure acid, or perhaps diluted with water ; for the passage as it stands is obscure. Numerous experiments have

shewn that the marine acid is preferable to any other. The proportions admit of much latitude, though he prefers the dose hereinbefore described.

Spirit of wine which has been mixed with nitrous acid, cannot be used in bleaching, even though afterwards rectified upon an alkali, because it still retains a portion of nitrous gas.

Pure spirit of wine without acid extracts a fine yellow colour from silk, which does not separate for years, even though exposed to the sun's light. Yellow silk exposed to the sun loses its colour in a short time. The acidulated spirit which has been used in the infusion of silk, is changed by exposure to the sun, but not in such a manner as to be rendered fit for use a second time.

In order to obtain a beautiful white colour, it is essential that the silk should be immersed in a large quantity of the fluid, especially at the first infusion. Without this management it would become necessary to make three infusions in the acidulated spirit. When the first infusion is well managed, the silk will have lost all its yellow colour, and become considerably white, at the same time that the liquor will have begun to change colour a little. As long as it continues of a fine green, it is certain that it has not exhausted its whole action upon the silk.

The duration of this first infusion may be longer or shorter, without inconvenience, according to the temperature. When the temperature is at 20 degrees of Reaumur, which answers to 77 of Fahrenheit, the first infusion is often made in ten or twelve hours. In small experiments the heat of the atmosphere may be supplied by the water-bath; in which case, all the infusions are easily made in the course of a day.

When the first infusion is finished and the liquor drawn off, the silk appears greenish: the subsequent washings in spirit of wine clear it of the liquor it retained. This sprinkling should be made with the watering-pot, otherwise the quantity poured will be greater, and the management more wasteful.

The cocoons may be bleached in this way, but the inconveniences are too great to render this process desirable.

Pieces of gauze and entire garments of silk have been successfully bleached in this way.

The finest natural white silks are rendered infinitely whiter by this process. Spirit of wine alone has the property of depriving yellow silk of its colour, which it brings to the state of the naturally white silk. In this state the silk is disposed to acquire a greater degree of brightness by a single infusion in the acidulated spirit. This process has its advantages over the other, to which it is also inferior in certain respects; concerning neither of which the author has entered into any detail.

The colouring matter was found to be a resin perfectly animalized, affording by distillation the same products as other animal matters, and the concrete volatile alkali.

Silk whitened by scouring may be dried freely in the air without affecting its lustre. This is not the case with the silk bleached in the gum: if it be left at liberty to dry in the air, it resembles white flax without any lustre. The beauty of this silk consists in its shining brilliancy; to secure which, it must be dried in a state of tension. Mr. Baumé has contrived a simple machine for this purpose. It consists of a strong square frame of wood standing upright upon feet: the upper horizontal bar is six feet long, and has six iron pins driven through

through it at equal distances, so as to project on each side for the purpose of receiving twelve bobbins. The lower horizontal bar is moveable up and down in a mortice by means of a screw at each end: it is furnished with six holes, adapted to receive as many pins to correspond with those above. The skains of silk are to be dressed and arranged upon wooden pins, as they are taken out of the sack from washing. As soon as there are twelve together, they are to be wrung with a staff; after which the skains are to be hung one by one upon as many bobbins put upon the upper pins of the square frame. Another bobbin with tails is to be inserted in the lower loop of the skain, and fastened to the corresponding pin of the lower bar, by means of a strap and hook, which need not be described to such as are slightly acquainted with mechanical objects. When the machine is thus supplied with skains on both sides, the lower bar of the frame is to be pressed down by the screws until the silk is moderately stretched. When it is dry, the screws are to be equally slackened, the skains taken off, and folded with a slight twist, that they may not become entangled.

After this description of the whole of his process, the author proceeds to make certain general remarks on the white China silk. He observes, that in his process the silks acquire the perfect whiteness without much handling, and consequently that there is little cause for them to become entangled. Accordingly the loss in unwinding is found to be no greater than when they are unwound in the yellow state: that is to say, from a dram to a dram and a half in the pound. This saving is of the greatest importance in the price of the silk.

The silk of Nankin, which he supposes to be bleached by some process of the same nature, is probably handled much more. The loss is nearly twelve per cent. when it comes to be opened, and not unfrequently even twenty-five per cent.; a loss which cannot in any respect arise from the package. The quality of the Nankin silk differs much in the package; the external part being always of the best quality, and that which is packed within is of such an inferior quality as sometimes not to exceed half the value. On examining this silk, it not only exhibited unequivocal marks of alkali, but its imperfections were also of the same kind as those which had occurred to Mr. Baumé during the progressive improvement of his own manipulations. The best China silk was neither improved nor injured by the process of Baumé; whence he concludes that they are not naturally white, but have undergone a process similar to his.

The result of the whole is, that the yellow silks of Europe may be bleached to equal or greater perfection than those of Nankin; and that these may be even greatly exceeded by winding the naturally white silk apart from the other, and bleaching it by itself.

The methods of recovering the spirit so as to be used again, and of obtaining the marine acid in the requisite state of purity, will be described in our next.

IX.

On the Hydrometer of BAUME.

AS many French chemists refer to the pèse-liqueur of Baumé, which has never been used in this country, it will be of advantage to describe the method by which it is constructed, and shew the specific gravities indicated by the graduations upon the stem. In-

stead of adopting the simpler method of immediate numerical reference to the density of water expressed by unity, as is done in all modern tables of specific gravity, he had recourse to a process similar to that of graduating the stems of thermometers from two fixed points. The first of these points was obtained by immersing his instrument, which is the common areometer, consisting of a ball, stem, and counterpoise, in pure water. At that point of the stem which was intersected by the surface of the fluid, he marked *zero*, or the commencement of his graduations. In the next place he provided a number of solutions of pure dry common salt in water: these solutions contained respectively one, two, three, four, &c. pounds of the salt; and in each solution the quantity of water was such, as to make up the weight equal to one hundred pounds in the whole; so that in the solution containing one pound of salt, there were ninety-nine pounds of water; in the solution containing two pounds of salt, there were ninety-eight pounds of water, and so of the rest. The instrument was then plunged in the first solution, in which of course it floated with a larger portion of the stem above the fluid, than when pure water was used. The fluid, by the intersection of its surface upon the stem, indicated the place for marking his first degree; the same operation repeated, with the fluid containing two pounds of salt, indicated the mark for the second degree; the solution of three pounds afforded the third degree; and in this manner his enumeration was carried as far as fifteen degrees. The first fifteen degrees afterwards, applied with the compasses repeatedly along the stem, served to extend the graduation as far as eighty degrees, if required.

This instrument, which is applicable to the admeasurement of densities exceeding that of pure water, is commonly distinguished by the name of the Hydrometer for salts.

The Hydrometer for spirits is constructed upon the same principle; but in this the counterpoise is so adjusted, that most part of the stem rises above the fluid when immersed in pure water, and the graduations to express inferior densities are continued upwards. A solution of ten parts by weight of salt in ninety parts of pure water, affords the first point, or zero, upon the stem; and the mark indicated by pure water is called the tenth degree; whence, by equal divisions, the remaining degrees are continued upwards upon the stem as far as the fiftieth degree.

These experiments, in both cases, are made at the tenth degree of Reaumur, which answers very nearly to fifty-five of Fahrenheit.

M. Baumé asserts, that all his instruments, constructed in these methods, agreed together with the utmost precision. From a few experiments, which however require to be carefully repeated, I am disposed to apprehend that the solutions of common salt do not give a sufficiently accurate original point, and that they may differ not only from the comparative dryness of the salt in different experiments, but likewise the state of its crystals, whether hastily or slowly separated in their original fabrication, the purity being supposed the same. Such differences must considerably affect the remote terms formed by repetition of the experimental interval in either instrument. I suppose all M. Baumé's instruments were constructed from solutions made once for all, and reserved for this purpose; and that the French chemists who used them were supplied under his direction. For these reasons I am more inclined to deduce the specific gravities from the experiments of himself and another accurate possessor of these instruments, than to recur to the original method of construction.

M. Baumé, in his *Elémens de Pharmacie* *, from which the whole of this account is deduced, has given a table (p. 410.) of the degrees of his Hydrometer, indicated by different mixtures of ardent spirit and pure water; where, he says, the spirit made use of gave thirty-seven degrees at the freezing point of water; and in a column of the table he states the bulk of this spirit, compared with that of an equal weight of water, as $35\frac{1}{2}$ to 30. The last proportion answers to a specific gravity of 0.842, very nearly. A mixture of two parts, by weight, of this spirit, with thirty of pure water gave twelve degrees of the Hydrometer at the freezing point. This mixture, therefore, contained $6\frac{1}{2}$ parts of Blagden's standard to 100 water, and, by Gilpin's most excellent tables †, its specific gravity must have been 0.9915. By the same tables, these specific gravities of 0.842 and 0.9915 would, at 10° Reaumur, or 55° Fahrenheit, have fallen to 0.832 and 0.9905. Here then are two specific gravities of spirit corresponding with the degrees 12 and 37, whence the following table is constructed.

BAUME'S Hydrometer for Spirits.

Temperature 55° Fahrenheit, or 10° Reaumur.

Degrees		Sp. Gravity	Degrees		Sp. Gravity.
10	=	1.000	26	=	.892
11	=	.990	27	=	.886
12	=	.985	28	=	.880
13	=	.977	29	=	.874
14	=	.970	30	=	.867
15	=	.963	31	=	.871
16	=	.955	32	=	.856
17	=	.949	33	=	.852
18	=	.942	34	=	.847
19	=	.935	35	=	.842
20	=	.928	36	=	.837
21	=	.922	37	=	.832
22	=	.915	38	=	.827
23	=	.909	39	=	.822
24	=	.903	40	=	.817
25	=	.897			

With regard to the Hydrometer for salts, the learned author of the first part of the *Encyclopédie* ‡, M. de Morveau, who by no means considers this an accurate instrument §, affirms, that the sixty-sixth degree corresponds nearly with a specific gravity of 1.848; and as this number lies near the extreme of the scale, I shall use it to deduce the rest.

BAUME'S Hydrometer for Salts.

Temperature 55° Fahrenheit, or 10° Reaumur.

Deg.	Sp. Gr.	Deg.	Sp. Gr.	Deg.	Sp. Gr.	Deg.	Sp. Gr.	Deg.	Sp. Gr.
0	- 1.000	15	- 1.114	30	- 1.261	45	- 1.455	60	- 1.717
3	- 1.020	18	- 1.140	33	- 1.295	48	- 1.500	63	- 1.779
6	- 1.040	21	- 1.170	36	- 1.333	51	- 1.547	66	- 1.848
9	- 1.064	24	- 1.200	39	- 1.373	54	- 1.594	69	- 1.920
12	- 1.089	27	- 1.230	42	- 1.414	57	- 1.659	72	- 2.000

* Fifth edition. Paris 1784. † In *Philosoph. Trans.* 1794. ‡ Tome I. p. 360. à Paris 1796. § *Ibid.* p. 361.

X.

Observations on the Soap of Wool, and its Uses in the Arts. By J. A. CHAPTAL, Instructor of the Polytechnic School.*

I HAVE shewn the method of forming, at any time or place, and at a small charge, a saponaceous liquid adapted to supply the place of soap for domestic purposes†. On the present occasion I shall offer a supplement to my former work, by exhibiting, as a substitute for the soft soap used in fulling almost every kind of woollen stuff; a soap of little expence, which may be easily made in every manufactory.

In every manufactory of broad-cloth, and other fabrics of wool, it is usual to full the stuff immediately after it has passed the loom. This operation is performed not only for the purpose of clearing them of the oil, but to give them the requisite density. For this purpose about thirty pounds of soap are used for every eighty pounds of the stuff. This soap in the south (of France) cost twenty livres the hundred weight before the Revolution. It consumed a large part of our oils, as well as those of Italy, and all the wood-ashes of the domestic fires in the respective countries in which it was made.

Hence it is obvious how greatly beneficial it must prove to the manufacturer, as well as to commerce in general, to be able to substitute without difficulty, instead of the soft soap, another compound of materials, easy to be procured, and of moderate cost. In addition to the saving in the fabrication of the stuff, very great advantage would be derived from the wood-ashes of our fires being left for domestic use, or for the salt-works, or manufactories of green glass; at the same time that the oil formerly consumed would remain to be totally applied to such works as cannot be conducted without it.

In all ages this problem has offered itself for solution to the Manufacturer and the Government. Fullers'-earth, pure alkalis, and other agents, have been successively employed. The first of these is of inferior quality, either for bleaching or fulling; the second dissolves the stuff. The manufacturers of Lodève still recollect with terror a charlatan sent to them by Government a few years ago, who pretended to substitute the mineral alkali in the place of soap.

To these inconveniencies we must add that of not rendering the cloth supple, but leaving it in possession of that harshness which soap alone removes. It is requisite, therefore, that whatever substance may be offered as a substitute for this article should possess the qualities of cleaning, fulling, and softening, the stuff. The composition I am about to describe possesses all these advantages. Experiments have been made at my request, at Lodève, by Citizen Michel Fabriguette, who is intimately acquainted with natural philosophy, and a skilful manufacturer of drapery.

The whole operation consists in making an alkaline lixivium of wood-ashes or pot-ash, and dissolving therein, at the boiling heat, old rags‡, or clippings of wool, to the point of

* Nearly a literal translation from the *Annales de Chimie*, XXI. 27.

† See the Report of Citizens Pelletier, D'Arcer, and Le Lievre, on the fabrication of Soap.

‡ Old woollen rags are a very cheap article in this country. But, as every other kind of hair must certainly answer, and horns and hoofs probably will, there must be an immense and probably cheaper source in the refuse of the tanners, hog-butchers, horners, and comb-cutters. All these, at present, are used only as manure. N.

saturation. The product is a soft soap, very soluble in water, of a green greyish colour, well blended (*bien lié*), and possessing an animal smell, which the cloths lose by washing and exposure to the air.

The various experiments I have made on this subject have presented the following results:

1. As soon as the wool is plunged in the boiling liquid, the filaments adhere together, and a slight agitation is sufficient to effect the complete solution.
2. The lye becomes coloured, and gradually thickens, in proportion as more wool is added.
3. The soap is more or less coloured according as the wool is less or more clean and white.
4. The pile or hairs which are mixed with the wool are more difficult of solution.
5. The quantity of wool the alkali is capable of dissolving, depends upon the strength of the lixivium, its causticity, and the degree of heat. Two pounds three ounces and six drams of caustic alkali, at twelve degrees * of concentration, and at the boiling heat, dissolved ten ounces four drams of wool. The soap, when cooled, weighed one pound four ounces.

An equal quantity of alkali, at the same degree of causticity, heat and concentration, in which I dissolved four ounces of wool, did not acquire consistence sufficient to answer several of the purposes required.

An equal quantity of alkali, marking four degrees, dissolved only two ounces seven drams of wool. The soap, when cooled, weighed fourteen ounces. It was of a good consistence.

6. In proportion as the wool is dissolved in the lixivium, the solvent power of the alkali decreases, and at last it takes up no more. It is at this period, namely, when the wool being agitated in the fluid is no longer dissolved, that the operation must be terminated.

I. The Choice and Preparation of Materials.

The materials required to form this soap are two, alkaline matters and wool.

The alkaline substances may be obtained from the ashes of common culinary fires †, and the lye made by the well-known processes. Lime is to be flaked with a small quantity of water; the paste is to be mixed with sifted wood-ashes, in the proportion of one-tenth part of quicklime compared with the weight of the ashes. The mixture is to be put into a small stone trough (for wooden vessels colour the lye, and become speedily uselefs); water is to be poured on to the depth of several inches. After a certain time, the solution may be drawn off at an aperture formed in the bottom of the vessel for that purpose. It must not be drawn off but at the moment previous to its use, and may have the strength from four to fifteen degrees. But, indeed, it is of little consequence what the strength may be, because the only difference resulting from the use of a weak or a strong lye is, that the quantities of wool which are dissolved, will differ accordingly.

The potash of commerce may be employed in the same manner, by mixing one-third of its weight of quicklime.

As to the choice of the wool, everyone knows, that in the manufactories of woollen cloths of

* Qu. By what measure?—It is greatly to be wished that all measures derived from the density of fluids were reduced to the common expression of the tables wherein water is taken as unity or 1,000. N.

† Wood being much more usually burned in France than in England, their common ashes are what in London are obtained only from the bakers. The uninitiated workman should be aware that coal-ashes are unfit for this purpose.

every kind, there are a number of operations performed, from the first washing of the material to the last package of the finished article, which occasion more or less of loss. The water in which the wool is agitated to cleanse it, the floor on which it is spread out, the warehouse where it is deposited, all afford waste wool; as do the operations of beating, carding, spinning, weaving, fulling, napping and folding. In all these several manipulations we every where see a residue of wool, which, it is true, is collected with some care; but many of these operations are of such a nature, that the remains of wool they afford are soiled and mixed with foreign matters, or else cut and rendered too short to enter into some fabrics; so that they are mostly thrown on the dunghill. This manufacture of soap affords the means of converting them all to use. Nothing more is required, but to collect them all in those baskets in which the wool is washed, and to wash them with care, for the purpose of separating impurities and foreign substances; after which, they are to be reserved for this use.

The cuttings of all the woollen stuffs, afforded by the shops of manufacturers, dealers, tailors, and the like, may be advantageously collected for this purpose; and the same advantage may be derived from the remains of garments after they are worn out.

II. Method of Making the Soap.

When the lye and the wool are both ready, it remains only to cause the lye to boil in a vessel of the common form. When it has arrived at this point, the wool is to be added by small quantities at a time, and agitated to cause a more speedy solution. Care must be taken not to add more wool, until the first portions are dissolved. The operation must be stopped the moment the liquor refuses to dissolve more.

From the operations in the large way, made by Michel Fabriguette, with soaps of his own fabrication, after the method I communicated to him, it is certain that this soap cleans, felts, and supple the cloths perfectly well. But its use requires a few important observations to be made.

1. When the soap is not made with the requisite care, or when dirty or coloured wool has been employed, the fabric receives from the soap a grey tinge, which it is very difficult to eradicate. This tinge is of no consequence when the stuff is intended to be dyed; but it would injure the beauty of that white colour which in certain goods is intended to be preserved. The remedy consists in employing the most select materials to form the soap intended for such delicate applications.

2. Stuffs fulling with this soap contract an animal odour, which, though not very strong, is nevertheless disagreeable; but water and the air completely remove it.

After having succeeded in the employ of this soap in fulling cloths made of wool, I attempted to substitute soda for potash, and to form, according to the process here described, a solid soap, proper for the operations of dyeing cottons. My experiments have succeeded beyond my hopes.

Forty-six pounds of soda at eight degrees dissolved at the temperature of ebullition five pounds of wool *, and afforded, by cooling, sixteen pounds fourteen ounces of soap sufficiently solid not to be spread (couler).

The

* It is affirmed, that when common sea salt is thrown into the combination of oil and vegetable alkali in the process of soap-making, the effect consists not merely in the separation of the soap from the water, now rendered salt, but that the alkalis change place; so that the soap obtains the mineral alkali, and the fluid, instead of containing

The first wool which is thrown into the soda dissolves readily; but it is afterwards seen that the fluid gradually becomes thicker, and that the dissolution becomes more difficult and slow.

The first solutions render the liquor green; after which it becomes black, and the soap, when cooled, preserves a blackish green colour.

This soap has been employed in every manner, and under every form, in my manufactory for dyeing cottons; and I am at present convinced that it may be substituted, instead of the saponaceous liquid we make from the lixivium of soda and oil, to prepare (*apprêter*) the cottons. I have constantly observed, that by dissolving a sufficient quantity of this soap in cold water to render the fluid milky, and by working (*foulant*) the cotton with the apparatus which is well known, it is sufficient to pass the cotton three times through, drying it each time, in order that it may be as well disposed to receive the dye as that which has been passed seven times through the ordinary solution of soap. This will not appear surprising when it is considered that animal matters are very proper to dispose thread and cotton to receive the dye, and that some of the operations of our dye-works consist simply in impregnating them with these substances.

It is to be observed, that cotton which has passed through a solution of this soap acquires a grey tinge nearly similar to what it gains by aluming, while the common soap liquors give it the most beautiful white colour. But this grey colour is not at all prejudicial to the dyeing processes, as we have remarked in speaking of woollens.

I must remark, in confirmation of this last use, which I attribute to the soap of wool, that after having impregnated cotton with it by the ordinary process, I caused it to pass through all the operations to which wool is subjected to produce the scarlet dye. The cotton acquired a deep and very agreeable flesh colour; whereas the cotton which had not received this preparation came out of the bath with its natural colour. This first essay promises advantageous results, which I mean to pursue.

It may be of some utility to observe, that the soap of wool may be beneficially substituted instead of common soap. In domestic operations I have profitably applied it to wash linen, and particularly woollen garments and other articles. I have no doubt but the facility and economy which it presents in its fabrication * will serve to extend its use still further; but in the mean time I have thought it proper to shew the various objects to which I have applied it.

Observation.

As the soap of wool gives a grey tinge to piece-goods, which it is difficult to eradicate, it follows, that it cannot be used for bleaching linen, unless it be made of white wool selected and carefully washed.

ing common salt, will be found to contain the combination of marine acid and vegetable alkali. I do not know if this has been shown to be really the case, nor whether this indirect process be of much value. If it be fully as here stated (which I doubt), our soaps must owe their inferiority to those of Spain to the animal oil they contain, and not to their alkali. N.

* The effect of the Excise Laws in Britain confines the manufacture of soap to premises registered in form, and regularly visited. What the general effects of this arrangement may prove on our great national manufacture of woollens, more especially if the present invention should amply come up to the expectations here excited, is a question that well deserves to be investigated. N.

XI.

Extract of a Memoir concerning three different Species of Carbonated Hydrogenous Gas, obtained from Ether and Alcohol by different Processes, forwarded to the National Institute of France by the Society of Dutch Chemists; being Part of a Report read to the First Class of the Institute, by Citizen FOURCROT, at the Sitting of the 26th Frimaire, 16th December 1796.*

CITIZENS Bondt, Deiman, Van Troostwyk and Lauwerenberg, chemists, of Amsterdam, who for several years past have made experiments together, and have already rendered great services to the sciences, sent on the first Fructidor of the fourth year to the Institute, a Memoir addressed to that body by C. Van Mons of Brussels, its associate. As the subject it treats of relates intimately to the progress and state of modern chemistry, it becomes necessary to give an account as ample as the novelty and importance of the subject demand.

Several months before the arrival of this Memoir, the Institute had received an account of the discovery it announces. The contents of the letters of C. Van Mons, on this discovery made in Holland, forwarded to the Institute in Ventose the fourth year (March 1796), were as follows:

The olefiant gas, which is so called from its characteristic property of forming oil in a circumstance which shall be described, is formed of a mixture of seventy-five parts of concentrated sulphuric acid with twenty-five parts of alcohol, even without the assistance of foreign heat. It is likewise formed by passing alcohol or ether in vapour over siliceous or aluminous in a tube of glass, or simply in a tube of pipe earth ignited without addition. It is not formed by the passage of the alcoholic or ethereal vapour in a tube of glass ignited without siliceous or aluminous, nor in the same tube containing lime or magnesia. The inflammable gas, which is obtained in this last case, is no longer susceptible of becoming olefiant, by a second transition through siliceous or aluminous. The olefiant gas is not absorbed nor altered by remaining over water; with a small quantity of oxygenated muriatic acid gas it forms an ethereal oil. Mixed with this gas in equal proportions, and set on fire, it lets fall a great quantity of carbone; when 0.25, or 0.20, or 0.15 of oxygenated muriatic acid gas is added to 0.75, or 0.80, or 0.85, of olefiant gas, and the mixture set on fire, the carbone appears immediately in the form of very fine lamp-black. The greater the proportion of the olefiant gas, the more perceptible is the appearance of carbone during the inflammation. Too large a portion of oxygenated muriatic acid converts it into carbonic acid. This experiment proves that the hydrogenous attracts oxygen more strongly than the carbone does. C. Van Mons thinks, in his letters on this subject, that the olefiant gas is a true carbonated hydrogenous gas.

Five months after this first account, on the first of Fructidor, in the fourth year of the Republic of France (18th August 1796), C. Van Mons forwarded to the Institute the Memoir of Citizens Bondt, Deiman, Van Troostwyk and Lauwerenberg, in which these chemists have with great care described the properties of this gas, which they had discovered, and which was already known by the name of the olefiant gas. This Memoir, which is very well drawn up, consists of twenty-four paragraphs; the substance of which we shall here relate.

In the first paragraph the authors observe, that the gas which is disengaged during the mutual action of concentrated sulphuric acid and alcohol, which was known to occasion frequent ruptures of the vessels, to burn with an oily flame, which had caused it to be preferred in

* Annales de Chimie, XXI. 48.

lamps supported by inflammable air, appeared to them deserving of a particular examination in consequence of the curious properties it presented, particularly when compared with the gasses afforded by alcohol and ether treated by other methods.

Secondly, From having remarked that the gas is disengaged towards the end of the process for making ether, they took the proportions of the mixture which exists at this period of the etherification; that is to say, four parts of concentrated sulphuric acid and one of alcohol, and treated this mixture in a common glass body appropriated to the production of aeriform fluids.

The third paragraph describes the series of phenomena which take place during the production of this gas. The mixture heats and becomes brown; gas is extricated without the application of external heat; but when such heat is applied, the effervescence greatly increases, the colour of the mixture becomes black, the gas passes abundantly; it is even necessary, to prevent the whole of the liquor from quitting the vessel, that the heat should be withdrawn. The residue, after the extraction of the gas, consists of sulphureous acid mixed with coal, which renders it black. Citizen Fourcroy remarks, that the description makes no mention of ether being produced.

[To be concluded in the next Number.]

MATHEMATICAL CORRESPONDENCE.

AS the present Journal is designed to embrace every branch of useful knowledge, both in science and the arts, a certain portion of it will be devoted to the insertion of such mathematical dissertations and questions, as by their novelty or importance appear to deserve the attention of the public. Every communication, therefore, of this kind, directed to the Editor, post paid, will be thankfully received, and published as early, in some of the succeeding numbers, as the plan of the work will admit; but it is particularly requested that the contributors of questions will direct them, as much as possible, to practical or theoretical improvement; as none that are forced, or framed upon obscure enigmatical principles, which when discovered are of no value, will be inserted.

The following QUESTIONS are proposed for Solution.

QUESTION I. By J. B.

IT is required to divide the half of a given right line into a given number of parts, so that each part, and the sum of that part and the remainder of the whole line may be in geometrical progression; this being a question of practical utility in the division of the monochord or musical string.

QUESTION II. By Capt. W. MUDGE.

IT is required to determine the centrifugal force of a body moving in the circumference of a circle, by the pure principles of fluxions, instead of deriving it from the doctrine of indivisibles, as is done by Newton in the Principia.

SCIENTIFIC NEWS.

THE Public will hear with much pleasure, that the *Journal des Sçavans* and the *Annales de Chimie* are both revived. The former work, as usual, is anonymous, though the papers are marked by initials or characters. The latter presents the respectable names of its authors, Guyton, Monge, Berthollet, Fourcroy, Adet, Seguin, Vauquelin, Pelletier, C. A. Prieur, Chaptal, and Van Mons.

Of the *Journal des Sçavans*, four numbers have reached England, commencing 16 Nivose, an. 5 (Jan. 5, 1797), continued on the 30th of the same trimestre, and afterwards published on the same days of each trimestre respectively. The last, 30 Pluviose, therefore answers to Feb. 18. Regular extracts will in future be given in our Journal.

The first number contains an introduction and a general sketch of the state of letters, sciences, and arts in Europe, at the commencement of the fifth year of the French Republic (23 Sept. 1796). The latter is drawn up with the hand of a master.

Scientific matters contained in the second number are: 1. The public sitting of the National Institute of the 15th Nivose. Citizen Prony announced three astronomical memoirs of Citizen Flaugergue, associate; and the continuation of the great work of Citizens Delambre and Mechain on the Meridian from Bayonne to Dunkirk, which will be finished in the year 6. Citizen Lapeche announced the following memoirs:—On the sulphureous acid, by C. Fourcroy and Vauquelin—Various works of C. Lamarque on the general principles of Chemistry, from which a chromometric scale is obtained—Defence of the new chemical theory against a German, by C. Van Mons, associate—On the substance of gold, by C. Chaptal, associate—On vegetable juices, by the same—On the gluten of wheat, by C. Texier—On the teeth of animals, particularly the horse, by C. Tenon—On the nature and causes of vertigo in horses, by C. Huzard—Researches on the epidemical disorders of cattle, by the same—An elementary table of the history of animals, by C. Cuvier—On the use of mercury in the small-pox, by C. Desessarts.

The travels continued with success by various members of the Institute, afforded Citizen Lapeche an opportunity for a well-timed digression in form of an invocation to Peace, which was greatly applauded.

The Citizen Talleyrand, one of the secretaries of the second class, gave an account of his labours. They consist of two memoirs on Ideology, by C. Tracy, associate—Two memoirs of C. Laromiguiere, associate; one on the operations of the human understanding; the other on the signification of the word Idea—On the conversion of the territorial impost into a duty on successions, by C. Duvillard, associate—On public credit, by C. Dyanniere, associate—Inquiries respecting the Arabian Gulf, by C. Gosselin—Concerning the manners of the Greeks in the time of Homer, by C. Levesque—Three historical memoirs; one respecting the Egyptians, another on the Swifs, and the third concerning Peru, by C. Anquetil—Notice respecting Sylvain Bailly, by C. Delisle de Sales.

Citizen Seguin, associate, read a memoir on the tanning of skins. His process, of which some account has already been given*, is said to produce the same effect in one month, as by the ancient process required fifteen or eighteen.

* Page 26 of this Journal.

Citizen Desfontaines read a memoir on the cultivation of spices in Guiana. Three hundred exotic trees were carried thither by C. Martin; and it appeared that the clove, among others, afforded a very considerable product.

Citizen Dupont de Namours read a memoir on the sociability and morality of the dog, the fox, and the wolf. The title excited the public attention; and the effects of the discourse, which was original, philosophical, and lively, was such that it gained much applause.

ACCOUNTS OF FOREIGN PUBLICATIONS.

Beschreibung einer Reise, &c. or Travels in Germany and Switzerland in the Year 1781, with Observations on the Sciences, Industry, Religion and Manners. By Frederick Nicolai. Vol. XI. and XII. Berlin and Stettin, 1796, in octavo.

Nicolai published the first volumes of his Travels in 1783, at that time offered for subscription, and he has since brought them out two volumes at a time. The 9th and 10th appeared in 1795. This work is greatly esteemed in Germany. The title of Travels, though the author really passed through the countries he describes, is, properly speaking, no more than a vehicle for numerous observations on manners, population, industry, and science. Particular dissertations on various objects are inserted. Thus the sixth volume contains dissertations on the Celts, the Suabians, the Celtic language, &c.; and the eighth, observations of much importance on certain geographical denominations, which Pliny, Ptolemy, and others, give to places situated at the Black Forest and in Helvetia. It must not be concluded that this account contains little novelty because the Travels published by the author were made in 1781. To the recital of his actual travels Nicolai has added a work of the closet, perhaps much too extended, which consists in a numerous collection of notes and tables relative to statistics. Some are dated in the year 1796.

The works of Nicolai are more particularly esteemed in Germany, in respect to the objects of political œconomy they contain.

Ueber die Kultur-verhältnisse, &c. or An Essay on the comparative Culture of Land in the various States of Europe; in which an Attempt is made to determine, by the Magnitude and Population of the several States, the Degree of Cultivation of the European Dominions. With sixteen large Tables, exhibiting the Surface and Population of the European States. By Aug. Fred. Guil. Crome. Leipzick, 1792; 1 vol. octavo; 398 pages of text, and 112 of additions.

Statistische Uebersicht, &c. A Statistical View of the Provinces of the Russian Empire, with regard to the comparative Cultivation of the Land. By H. Storch. Riga, 1795; Hartknock; 1 vol. small folio, 131 pages.

Geographisch-Statistische Tabellen; Tables of the Statistical Geography of Switzerland. By H. K. Zurich, 1795; Ziegler and Sons; 1 vol. small folio; seven tables.

Annalen der Staatskräfte von Europa. Annals of the Powers of the several States of Europe considered, with regard to the present State of Natural Philosophy, Commerce, Science, and Political Relation. The whole drawn up in the Form of Tables. By Ad. Frid. Randel. Berlin, 1792; Frid. Vieweg; 1 vol. small folio.

Der Polynomische Lehrsatz, &c. Principles of the Doctrine of Polynomials, the most interesting Problem of Analytical Science, with some other Theorems relating thereto, explained and developed, by Tetens, Kluegel, Kramp, Pfaff, and Hindenburg. The
smaller

latter has added an abridged Summary of the Method of Combinations, and their Use in analytical Processes. Leipfick, Fleischer the younger, 1796, 8vo.

Ueber die Methode des Hrn. La Grange, &c. A Dissertation on the Method of Mr. La Grange, for resolving all Equations by means of Series. By F. W. A. Murhard.—Gottin-gen, Rosenburg, 1796, 4to. —16 pages.

Die neuesten Entdeckungen, &c. The new Discoveries in Electricity considered with regard to Natural Philosophy and Medicine, collected by Charles Gottlieb Kuenh. Part I. Leipfick, 1796, 8vo.

Dr. Lorenz Deerell has translated into German the first volume which has appeared of Kirwan's improved edition of Mineralogy, with Observations. It is printed for Nicolai at Berlin.

Handbuch der neuesten Erdbeschreibung; or, A Manual of the most modern Geography. By Gasparis, Professor at Jena. Vol. I. 8vo. Weimar, December 1796.

Acta Elec. Mogunt. Scientiarum utilium, quæ Erfurti est, ad Annum 1794 & 1795, cum Figuris in 4to maj. Erfurt, G. A. Reyser.

Traité d'Harmonie & de Modulation. Par H. F. M. Langlé. A Treatise of Harmony and Modulation. By H. F. M. Langlé, some time First Master of the Conservatory of Piety at Naples, and Professor at the Conservatory at Paris.—This treatise is divided into two parts. The first exhibits all the practicable concords in harmony, and the second all the possible modulations. "The authors have treated this second part," say the Journalists, "in such a manner as to facilitate to beginners on the piano-forte and the harp the difficult accomplishment of preludes." Boyer, Paris.

Two Translations of Spalanzani's Travels in the Two Sicilies have appeared; the one at Berne, by Senebier; and the other at Paris by Toscan and Duval, with Notes by Faujas. Neither of these can yet be complete, as Spalanzani's Travels are to form six volumes in 8vo.

The Encyclopédie Methodique par ordre de Matières, which was interrupted in its progress by the French Revolution, is again continued. The sixtieth and sixty-first livraisons are published for Agasse, Rue des Poitevins, No. 18. The sixtieth livraison contains three half-volumes:

1. The seventeenth Part of the Plates of Natural History, forming the seventh Century of Botany; by Citizen Lamarck, of the National Institute, Professor and Administrator of the Museum of Natural History.

2. The second Part of the third and last Volume of Ancient Geography; by Citizen Mentelle, of the National Institute.

3. The Dictionary of Fishery; by Citizen Lacombe, Author of several Dictionaries of the Encyclopédie, particularly that of Arts and Trades. The price of this is thirty-one livres in sheets, or thirty-three stitched. A table is given of the parts already published, by which it is seen that the work is already far advanced, and that there is no probability of its not being completed.

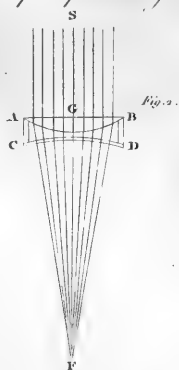
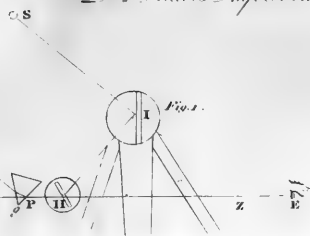
The price of the sixty-first livraison is the same as the foregoing. It contains the eighteenth part of the plates of Natural History, consisting of one hundred plates of Insects.

Vol. IV. Part 1, Dictionary of Botany, by Lamarck.

Vol. VII. Part 2, of the History of Insects.

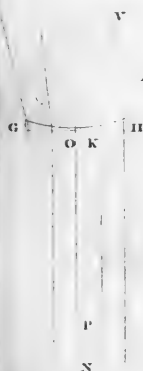
[Accounts of English Publications in the next Number.]

D. P. Plair's Improvement of compound object Lenses.



V X

Fig. 4.



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Fig. 5.



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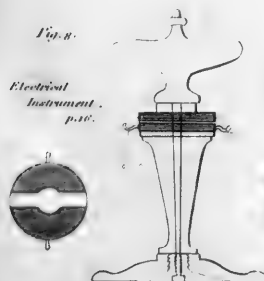
Fig. 6.



Fig. 7.



Fig. 8.



Electrical
Instrument.
p. 16.

Fig. 10.



Uncommon
Solar Spectrum.
p. 13.

Fig. 11.

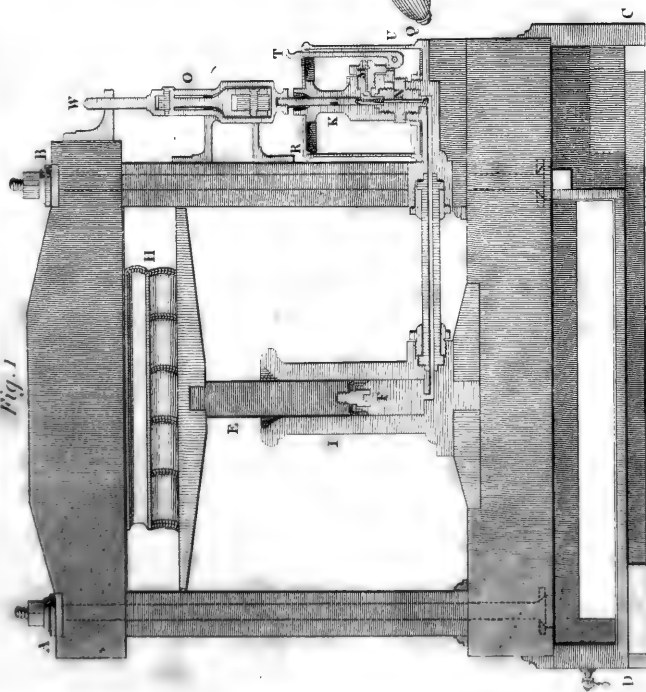


Fig. 12.





Fig. 1



Scale of Inches.

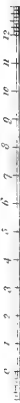
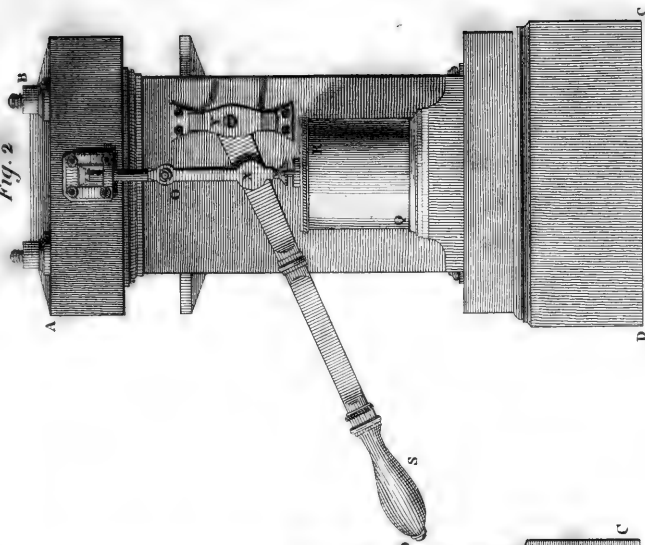


Fig. 2





A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

MAY 1797.

ARTICLE I.

Extract of a Memoir concerning three different Species of Carbonated Hydrogenous Gas, obtained from Ether and Alcohol by different Processes, forwarded to the National Institute of France by the Society of Dutch Chemists; being Part of a Report read to the First Class of the Institute, by Citizen FOURCROY, at the Sitting of the 26th Frimaire, 16th December 1796.

[Concluded from the last Number.]

THE fourth paragraph treats of the choice and purification of the gas. At the commencement and towards the end of the operation, it is mixed with sulphureous acid gas; towards the middle it is purer, and contains only one sixth of sulphureous acid; when washed with water and ammoniac, it is rendered very pure. It does not contain carbonic acid gas.

The fifth paragraph exhibits the physical properties of this gas. Its weight compared with that of common air, is as 909 to 1000; its odour is fetid, when well purged from ether and sulphureous acid. It burns with a strong compact flame similar to that of a resinous oil.

The sixth paragraph enumerates several properties in some measure negative and characteristic of this gas. When left for several months over water, it suffers no alteration. The sulphuric, sulphureous, nitric and muriatic acids do not act upon it; neither does the nitrous gas, nor alkalis. Ammoniac in the elastic state adds merely to its volume, without occasioning any change. Phosphorus heated even to fusion did not affect it.

In the seventh paragraph the authors describe the action of oxygenated muriatic acid gas upon their newly discovered gas. As this is the only substance which acts in any remarkable manner upon it, their description is very accurate and full. They announce with reason this effect as no less curious than new and hitherto unknown. They at first applied the oxygenated muriatic acid, with the intention of proving the presence of carbone in this gas; because

the process had before succeeded in similar cases. Having mixed in a tube over water equal parts of their inflammable gas and oxygenated muriatic gas, an absorption took place which was more rapid than what obtains between the latter gas and water; a thick oil of a pearl-grey colour more ponderous than water was deposited. The tube was filled with a white vapour; much caloric was disengaged, and there remained one eighth part of the gas made use of, which was still inflammable. A second mixture of four parts of oxygenated muriatic acid gas, and one part of the inflammable gas produced by alcohol and the sulphuric acid, and kept over water for eight days, presented the same phenomena as the foregoing. After the separation of the oxygenated muriatic acid gas, there remained only $\frac{1}{20}$ th part, which was azotic gas afforded by the oxide of manganese.

The eighth paragraph is employed in describing the properties of the oil obtained in the foregoing experiment. When collected in an apparatus which the authors do not describe, it presented the following characters: Its semi-transparency resembled the colour of pearls; it sunk in water, and became yellow by exposure to air; its smell was agreeable and penetrating, its taste rather sweet; both these properties were very different from that of ether. It was dissoluble in water, which acquired its odour; liquid vegetable alkali rendered it sweeter, by depriving it of the smell of oxygenated muriatic acid.

In their enquiries, in the ninth paragraph, concerning the composition of this gas from the known properties of sulphuric acid and alcohol, of which it is formed, the Dutch chemists assume in the first place as a principle that it can contain no other ingredient than hydrogen, carbone, and oxygen. This last substance does not appear to them to be present, because it ought either to form water or carbonic acid, neither of which compounds appears*. The gas is not either dissolved in sulphureous acid gas; because after the combustion with vital air by means of electricity, as well as after its conversion into oil by the oxygenated muriatic acid, the muriate of barytes does not indicate sulphuric acid, which ought in that case to be formed. Neither does sulphur enter into its composition; for it does not exhibit the smell of sulphurated hydrogenous gas, nor does it throw down sulphur in proportion as the gas is burned. By this method of exclusion, the learned chemists of Amsterdam conclude that their inflammable gas can be compounded of nothing but hydrogen and carbone.

The tenth paragraph is employed in proving the existence of hydrogen in this gas. Though the formation of water and of oil by the oxygenated muriatic acid sufficiently declares this; yet, as the presence of the water over which the experiment was made might leave some doubt, the Dutch chemists had recourse to other proofs. By passing their gas through a tube filled with sulphur in fusion, they obtained sulphurated hydrogenous gas, and the sulphur was blackened.

In the eleventh paragraph they prove the presence of carbone in their gas, not only by the black colour of the sulphur indicated in the foregoing experiments, but by the formation of carbonic acid, which takes place whether the gas be burned with vital air, by means of the electric spark, or by passing it through an ignited tube filled with the oxide of manganese.

* To these two conditions may be added another, which the learned authors have not directly considered, namely, that it may form a triple compound. This, in fact, may happen in the oil; for which vide infra. N.

A later experiment, which likewise proves it, consists in mixing the gas with oxygenated muriatic acid gas, and burhing it before their reaction has presented the oil before mentioned. In this experiment the glass vessel is covered with coal resembling lamp-black.

The twelfth paragraph announces the name the authors have chosen for their gas, in consequence of its nature and properties: it is *carbonated oily hydrogenous gas*.

In the thirteenth, they speak of the elastic fluid disengaged from ether treated with the sulphuric acid. Only three-fourths of this gas is converted into oil by the oxygenated muriatic acid; the residue burns blue, and is no longer reducible into oil by this acid. Alcohol and ether passing through a tube of pipe-clay, when ignited, afford a gas of this nature.

The fourteenth paragraph announces the formation of an inflammable gas different from the last mentioned, or from the olefiant gas, by passing ether and alcohol through an ignited glass tube. This gas does not afford oil by the oxygenated muriatic acid gas. In order to determine whether the porosity of the tube of clay was the cause of the formation of the oily gas, by suffering some principles to pass through, or by admitting others, these philosophers included a tube of clay in another of glass, and then passed alcohol and ether, after having previously ignited the tubes. There was a production of olefiant gas in this case, as well as when the glass tube contained fragments of pipe-clay. Hence they conclude that the clay contributes to the formation of this gas.

Similar experiments form the object of the three following paragraphs, which Mr. Fourcroy abridges, by merely prefixing the number without any prefatory connection.

15. A tube of glass charged with alumine and ignited, through which they passed alcohol, afforded an oily gas, the residue of which, after the formation of the oil, burned with a blue flame: the same thing happened when flint was used instead of alumine. When the tube of glass was filled with lime, or magnesia, or vegetable alkali, or charcoal, or sulphate of pot-ash, and the alcohol was respectively passed through these in the ignited state, the product was a gas not capable of forming oil; the earths were blackened in these experiments. Sulphur, in a trial of the same nature, afforded sulphurated hydrogenous gas not oily.

16. Alcohol and ether must necessarily pass, according to these authors, over alumine or flint, ignited in a tube of glass, to afford the olefiant gas. The gas obtained from these two fluids by passing them through a tube of glass does not become olefiant when passed a second time over alumine or flint, or through the tube of clay. This property, therefore, when once lost, cannot be restored.

17. Carbonated oily hydrogenous gas passed through an ignited glass tube does not diminish its volume, and loses the property of forming oil with the oxygenated muriatic acid. The tube and the glass which have been used in this experiment, are blackened and covered with drops of empyreumatic oil; they have the smell of this last product; a black soot covers the water of the apparatus. Six hundred electric shocks passing through carbonated oily hydrogenous gas increased the bulk by two-fifths, and deprived it of its property of forming oil, without having precipitated carbone.

After having thus examined, in the first seventeen paragraphs, the properties of the carbonated oily hydrogenous gas, the chemists of the society of Amsterdam are employed in the five following upon two other kinds of elastic fluids obtained from ether and alcohol by different processes.

In the eighteenth paragraph they describe the process by which they have produced these

these last gases. It is performed by distilling ether and alcohol through a tube of ignited glass, the extremity of which is received under an inverted glass filled with water. The gas formed in this manner precipitates neither oil nor charcoal by the contact of the oxygenated muriatic acid. During its formation, part of the ether or alcohol passes unchanged.

The characteristic properties of the gas obtained from ether by this process are shewn in the nineteenth paragraph. Its weight, compared with that of air, is as 0.709 to 1.000. The smell of ether, which is perceptible in the first portions, is soon succeeded by a fetid smell. It burns with a compact oily flame, like the former gas; water neither dissolves nor alters it; it does not render lime-water turbid; and it is unalterable by acids or alkalis. The oxygenated muriatic acid reduces its volume $\frac{1}{3}$ th. It burns with a blue flame, and affords no trace of oil.

The researches which form the object of the twentieth paragraph shew the presence of, 1. Hydrogene, by the formation of sulphurated hydrogenous gas, which is produced when it passes over fused sulphur. 2. Carbone, by the black precipitate it affords by burning, after mixture with the oxygenated muriatic acid gas, as well as by the carbonic acid which results from its combination with vital air. The authors of the memoir, to distinguish this gas from the former, call it *carbonated hydrogenous gas obtained from ether*.

The gas which is afforded by alcohol distilled through an ignited glass tube, forms the subject of the twenty-first paragraph, and differs from the foregoing in two properties:—1. By its specific gravity, which is to that of air as 0.436 to 1.000. 2. By its paler and less oily flame, which resembles that of alcohol. This is named by the Dutch chemists, *carbonated hydrogenous gas obtained from alcohol*.

As the analysis of these three species of gas presented to their notice the same principles in their composition, and as they did not appear to them to differ but in the mere proportion of these principles, they have in their twenty-second paragraph united the results of their experiments in this behalf. Though they were not entirely satisfactory, they were nevertheless sufficiently clear to enable them to deduce an useful conclusion. In order to arrive at a knowledge of this proportion of the principles in the three kinds of gas they had to compare, they mixed each of them in tubes of glass closed at one end, or long glass vessels, over mercury, with a larger quantity of oxygenous gas than was sufficient for their complete combustion. These mixtures were set on fire by the electric spark; the diminution was carefully determined. The proportion of carbonic acid formed was estimated from the quantity of precipitate thrown down from lime-water introduced over mercury. The result of their trials on this point was, that these gases contained from 80 to 74 parts of carbone, with from 20 to 26 of hydrogen. In general the three gases exhibited little difference between each. The carbonated oily hydrogenous gas presented the largest proportion of carbone; the carbonated hydrogenous gas from ether, a medium proportion of this principle; and the carbonated hydrogenous gas from alcohol, the least of the three.

The twenty-third paragraph of the memoir, abridged by Mr. F. contains a retrospect of the principal facts exhibited in the whole treatise. This retrospect is so well drawn up, and presents a view of the whole with so much accuracy and condensation, that he has preferred the method of quotation without abridgment.

"Here then," say the Dutch chemists, "are three kinds of inflammable gas obtained from alcohol and ether treated in different manners.

"These

"These gases have this in common, that they are composed of hydrogen and carbone. They are species of carbonated hydrogenous gas.

"It appears probable, moreover, that the proportions of these constituent parts do not differ, or at least that they differ very little if equal weights of each were examined.

"They differ from each other in several other respects; as their specific gravity, their manner of burning, and the several methods by which they are produced.

"The most remarkable difference is certainly the formation of an oil by the mixture of oxygenated muriatic acid gas with the carbonated oily gas.

"This gas is produced in its greatest purity in the distillation of ether, or of a mixture of alcohol and concentrated sulphuric acid.

"Ether mixed with the same sulphuric acid also affords it, but less pure.

"It is also obtained by causing the vapours of alcohol and ether to pass through a tube of clay ignited by fire; but this likewise is not perfectly pure.

"This effect is equally observed by taking the component parts of the tube, namely, alumine and silex; which taken separately communicate to the gas this property of forming oil, if the vapour of the alcohol or the ether pass over these substances.

"These vapours passing through a simple tube of glass ignited, afford the two other species of gas, accordingly as either ether or alcohol is taken: and these two last gases do not prevent the least appearance of oil, formed by mixing them with the oxygenated muriatic acid gas.

"The gas which has the property of forming oil loses it by being made to pass through an ignited glass tube. It deposits charcoal.

"Electric shocks have the same effect, but without the precipitation of carbone. It is moreover observed, that the volume of the gas is augmented when it passes from the oily state to that which is incapable of forming oil.

"These species of gas, lastly, as well the oily as those obtained from ether or alcohol, are truly permanent gaseous fluids, and must not be regarded either as the vapours of ether preserving the aeriform state for a time, or as hydrogen gas holding particles of ether or alcohol in suspension. We have kept these gases for whole months over water; we have repeatedly passed them through that fluid; and we have exposed them to re-agents. They have always preserved their properties without alteration and without loss."

In the twenty-fourth and last paragraph, the Citizens Bondt, Deiman, Paats van Troostwyk, and Lauwerenberg, conclude their interesting researches by various questions which their experiments have not resolved. They demand, 1. How the oil is formed by the carbonated oily hydrogenous gas, in its mixture with the oxygenated muriatic acid gas: whether it be by the addition of a portion of oxygen from the latter, or a subtraction of part of the hydrogen from the former. 2. What is the nature of this oil, of which hitherto they have not yet procured a sufficient quantity for a proper examination; but which nevertheless appears to them to be a kind of ether. 3. Why is the oily gas formed when ether and alcohol pass over alumine, silex and clay, but not over glass. 4. Whether the difference do not arise from the circumstance (say they) that the one contains a greater quantity of caloric than the other; and whether it be not thus that the first gas loses its property of affording oil in proportion as its bulk is augmented by electricity, without occasioning a precipitation of carbone.—That they cannot positively reply to these questions is ascribed by them to the state of vegetable chemistry, which is less advanced than that of the air and of minerals.

The experiments made upon the olefiant gas by the citizens Hecht and Vauquelin, in

consequence of the invitation of the Philomathic Society of Paris, throw some light on the questions of the Dutch chemists. The olefant gas, passed through an ignited porcelain tube, afforded hydrogenic gas mixed with carbonic acid; a large quantity of carbone was deposited in the glass tube in which the tube of porcelain terminated. The difference remarked in this experiment and that of the Dutch chemists is owing probably to the high degree of heat given to the tube of porcelain*. The carbonated hydrogenous gas, deprived of carbonic acid, and afterwards mixed with the oxygenated muriatic acid gas, did not form oil as before. The olefant gas had deposited its carbone upon the alumine in passing through tubes which contained that earth. The ethereal gas burns with the oxygenated muriatic acid gas, and affords with it the same oil as the olefant gas; which appears to mark a great analogy between them. They may probably differ only in the unequal quantity of combined caloric.

In general it follows, from all these comparative experiments, that the most dense olefant gas, containing less of caloric than the carbonated hydrogenous gases afforded by alcohol and ether passed through tubes of glass, possesses the disposition to form oil only by virtue of the proximity of its constituent molecules; and that it loses this property only when they become separated more apart by the intromission of a greater quantity of caloric between them.

It follows also, that the olefant gas is converted into simple carbonated hydrogenous gas, whenever a process is executed which increases the distance between its component parts. In this way, the attraction between the particles of the hydrogenic and carbonic is diminished, upon separating them by virtue of a greater proportion of caloric introduced between them. This happens whenever the olefant gas is made to pass through ignited tubes, of what nature soever; or by strongly electrifying it. The same experiments prove also, that the difference observed, and so accurately noted, by the Dutch chemists, between the three kinds of carbonated hydrogenous gas, which they distinguish by the names of *oil*, *obtained from ether*, and *obtained from alcohol*, though each may be obtained indiscriminately either from alcohol or ether, arises only from the methods of treating these inflammable fluids, and is constantly reducible to this; that in forming the olefant gas a less quantity of caloric enters the mass, and a compound is determined, in which the hydrogenic and carbonic are more concentrated, and more disposed to form oil by the addition of oxygenic, more condensed itself than the state of vital air as it exists in the oxygenated muriatic acid gas; whereas, by more strongly heating the alcohol or the ether, by accumulating more caloric in their vapour, these fluids become more completely decomposed; their elements more widely separated; and their simultaneous attraction, which might be adapted to form oil with oxygenic, is so far diminished, that they are no longer capable of that transition. Hence we may conceive, how from the specific gravity of 0.909, which distinguishes the olefant gas, it becomes, at the same time that it loses this property, so light as 0.436.

We † are not of opinion with the Dutch chemists, from the alumen, filix, and tube of clay having served to form the olefant gas, while glass, lime, and magnesia afforded only the carbonated hydrogenous gas, not olefant, that the former of these bodies possess a

* Or perhaps to the difference between the tubes made use of. In the porcelain tube (like that of glass) the semi-vitrification might cover the filix and alumine, or change its qualities in other respects. N.

† Either M. Föhrer or MM. Hecht and Vauquelin. But this department of research certainly demands experiment. Is it probable that any considerable difference of temperature took place when clay and lime were respectively ignited in the glass tube? It would be easy to prove this by giving the higher temperature decisively to the clay. N.

peculiar tendency to produce the olefiant gas, which property is wanting in the lime and magnesia. These different effects certainly arise from a less degree of heat having been applied in all the cases where the olefiant gas was produced, and a greater in all those which presented the simple carbonated hydrogenous gas.

But the circumstance which is truly important for the general theory of science in these experiments, is the new light they throw upon the formation of oil; and the force they give to the notions already received and the considerations long ago presented in the pneumatic doctrine, respecting the nature and composition of vegetable oily bodies. We here see that an oil is a compound of hydrogen, carbone, and a small portion of oxygen. We find that, in the mixture of the olefiant gas, or dense carbonated hydrogen, with the oxygenated muriatic acid, an oil is immediately formed in the same manner as when wood, or any other vegetable matter, not in itself oily, is heated in a retort. It appears, that the olefiant gas, or a gas very similar to it, is disengaged from wood, or any solid vegetable not of an oily nature, which is slowly burned. This gas, when burned in the open air, deposits its foot or carbone, which blackens the wood, and clogs the channels through which the elastic fluid must pass. If wood, mucilage, &c. be more suddenly and strongly heated in close vessels, they afford less oil, more coal, and more carbonic acid; and in the open air there is more flame, and less coal or foot deposited. It is probable that, in the first case, the olefiant gas is disengaged, and in the second, the simple carbonated hydrogenous gas*. We are therefore in progress towards the artificial composition of oil. Nothing more seems to be required for its artificial production, than to obtain, without vegetable matter, a dense carbonated hydrogenous gas weighing 0.909, and to mix it with the oxygenated muriatic acid gas: and we may hope to succeed in the production of this olefiant gas with mineral matters containing much carbone, such as certain kinds of steel, which when dissolved in an acid, by the decomposition of water they promote, afford a disengagement of carbonated hydrogenous gas. This last object deserves all the attention of chemists, because it opens a new path to the knowledge of vegetables, and particularly of the formation of oil.

The ingenious experiments of the Dutch chemists are among the few which afford new prospects to philosophers. Together with the discoveries for which we are already indebted to them, on the decomposition and recomposition of water by electricity, on the alkaline and metallic sulphures, &c. they will possess a distinguished rank in the pneumatic chemistry, to the progress of which they have dedicated the reputation of their labours and their discoveries.

* This excellent course of experiments, and the general inductions founded upon them, bring to my recollection some observations I had occasion to make many years ago, when alcohol was burned in the lamp of Argand. In this experiment, the strata of ignited matter forming the interior and exterior flames (which have not yet been accounted for) are strikingly distinct. I have supposed them to be referable to a general law of the combustion of volatile matters at certain limits of temperature; as is most eminently seen in phosphorus and sulphur, both of which have the two kinds of flame in succession, and both begin their slow combustion at very moderate temperatures. But the fact immediately referable to the present theory is this: When the lower aperture or air passage of the lamp is left open, the flame of the alcohol is as usual faint and blueish, resembling that of carbonated hydrogen; but if the passage be gradually obstructed by slowly applying the palm of the hand, or any more suitable obstacle, the flame becomes more and more luminous, like that of oil, until, in the progress of obstruction, the aperture is so much closed that the combustion begins to decay for want of air. By a proper adjustment of the aperture, the alcohol may be made to burn with a constantly luminous white internal flame. In this case it may be supposed that the temperature is precisely such as to extricate the olefiant gas. N.

II.

On the Methods of obviating the Effects of Heat and Cold in Time-Pieces.

IN every mechanical instrument for measuring the flow of time, some natural power is applied to produce motion; and the portions of this motion being usually registered by an index, are taken to denote certain correspondent portions of time. The simplest instruments of this nature are the hour-glasses, which is still in use; and certain clocks, regulated by the flow of sand or water, upon the same principle as the hour-glasses; of which a considerable number are described in Ozanam's *Mathematical Recreations*. Of these it is unnecessary to say more in this place, than that the passage of sand through an aperture is far from being uniform, from a variety of obvious causes; and that the flow of water not only varies from circumstances capable of hydrostatical estimation, but also from its fluidity, which these very instruments have shewn to be greater or less in proportion to its heat. The clock and watch, which consist of an assemblage of wheels acted upon by a weight or spring, and regulated by a pendulum or balance, are so much superior in their performance to every ancient instrument applied to this purpose, that the attention of men of science has long been confined exclusively to these instruments.

When a larger wheel drives a smaller, the number of revolutions performed by the latter will be more numerous in a given time than those of the driving wheel in the inverted proportion of their diameters, or the numbers of their teeth. Hence it may easily be deduced, from the infinity of assumable ratios, that combinations or trains of wheels may be made so that the slow descent of a weight, or other equivalent agent, may cause certain wheels in the system to revolve with any determinate velocity, provided the rate or velocity of the first mover be determined. It is an object of mechanical skill to adjust a train of wheels, applied to the solution of a problem of this nature, in the best manner possible. For the frictions will increase the more complex the train, the lower the numbers of the teeth, and the larger the pivots. These are not, however, the objects of the present dissertation.

When a train of wheels is acted upon by a weight, this last, instead of descending with the accelerated velocity produced by the force of gravitation, is continually checked by a resistance from the train, chiefly arising from the sum of the frictions of the parts against each other, and in a certain degree from the resistance of the air, which against the quicker moving parts is considerable. These retarding causes speedily increase, until they counterbalance the whole force of gravitation; when the weight of course being deprived of the cause of acceleration, descends uniformly. Here then we observe an uniform motion, produced simply by the application of mechanism, to impede the free descent of a weight: but a mere train of wheels, when acted upon by a large weight, suffers it very soon to run through any moderate height; and if the weight be diminished, the resistances from friction and imperfection of work being far from uniform, and occurring at certain periods of the rotations, are liable to stop the machine altogether. For this reason it has been found expedient to increase that part of the resistance which arises from the train, by the addition of a fly at the end of the train. The common jack for roasting meat, which is to be seen in every kitchen, is an engine of this kind, in which the weight descends

descends very slowly and uniformly, because resisted by the friction of a screw in the last arbor of the train, and a fly which terminates the whole. The late Mr. John Whitehurst, F. R. S. constructed a clock on this principle for measuring short portions of time, to the hundredth part of a second, in which the regulating agent was a broad-leaved fly, which acted upon the air. It was regulated in the first place by adjusting the weight, and in the next place by altering the obliquity of the striking surface of the fly. It may readily be imagined, as was in fact the case, that its rate would vary according to the density of the air. Perhaps it might have been of use in some experiments to ascertain that varying resistance. Huygens, in his treatise *De Horologio Oscillatorio*, describes a clock regulated by a fly, which derived its resistance from the gravitating power of a ball at the end of a suspension by strings. When the maintaining power was weak, it swung out to a small distance from a vertical axis, to the upper part of which the strings were fastened, and was therefore easily carried round; but when a greater force was applied, the ball swung out farther, and consequently afforded a greater resistance. The strings were made to apply against a curve, by virtue of which the divergencies of the ball produced isochronal rotations.

It is probable that the first wheel clocks were regulated by a fly; an organ, of which the inconveniences are too great to require enumeration to such as are moderately skilled in pursuits of this nature. Striking and repeating trains are still regulated in this way. M. Le Roy, in the *Encyclopédie**, article *Balancier*, affirms, that before the admirable invention of the pendulum by Huygens, clocks were regulated by an horizontal balance, having a vertical axis, that passed through two holes, with liberty to play up and down; and that it was suspended by means of a string passed through a hole in the axis, and fastened at both ends, so as to form equal angles with the axis itself. Consequently, when the balance revolved in one direction, the string was wound upon the verge, and, being thus shortened, raised it up until the weight of the balance had overcome the force of rotation: after which it revolved the contrary way, and descended to perform a similar ascent by winding the string the opposite way. He does not tell us the nature of the connection between this balance and the wheels which were regulated by it.

The application of the pendulum to clock-work constituted so important an improvement in engines for measuring time, that it will probably be very long before our contemporaries, or even their descendants, will be able to bring the balance in competition with it. In this organ, the regulating force is so steady, and the loss from the resistance of the air so small, that the imperfections of the train through which the force of the first mover must be conveyed, are capable of producing no more than a small part of that irregularity which necessarily follows when the actual regulator derives most of its effect from the train itself.

From the contemplation of this most valuable measure of time, and the sources of its regularity, the artists of modern times have endeavoured to place it in a situation which shall as nearly as possible resemble that of a pendulum detached from a train of wheels, and vibrating in vacuo without friction or resistance. This similarity is aimed at by a skilful disposition of that part of the engine which connects the pendulum with the train, and is called the escapement, for which there are many contrivances well entitled to the attention of artists and men of science. Another most essential particular is, that the organ for measuring time, whether it be a balance or a pendulum, should invariably preserve its

* Geneva edition, 1778, vol. iv

dimensions with regard to the centre of oscillation. But heat and cold alter the dimensions of all the substances we know, though not equally in each. The remedy is derived from this inequality, and constitutes the subject of the present memoir.

Since the dimensions of all bodies increase with their temperature, it naturally follows that the pendulums of clocks are longer in hot weather than in cold; and will therefore make their vibrations in longer times. As far, therefore, as this cause of irregularity extends, every time-piece, regulated by a pendulum or balance, will go slower in summer and faster in winter. In the pendulum for seconds, the quantity required to be added to, or subtracted from, the length, to cause a gain or loss of one minute per day, is 0.0545 inch at a medium, which does not differ from either extreme more than the half of unity in the last figure. Hence the mean variation of length answering to one second is 0.000908 inch. And by General Roy's experiments, in the seventy-fifth volume of the *Philosophical Transactions*, the expansions of five feet of the undermentioned substances, by a variation of temperature of 180 degrees of Fahrenheit, namely from 32° to 212° are,

					Inch.
Rod of English plate brass	—	—	—	—	0.113568
Rod of steel	—	—	—	—	0.068684
Prism of cast-iron	—	—	—	—	0.066563
Glass tube	—	—	—	—	0.046569
Solid glass rod made long before	—	—	—	—	0.048472

Hence it is seen that, in a clock with a simple pendulum, having the rod of steel, every difference of temperature of four degrees will cause a variation of one second per day.

That the variation with a brass rod is nearly twice as much.

And with a glass rod, not much more than half the quantity.

The difference between the mean heights of the thermometer within, at the Royal Society's apartments, in summer and winter, is about 25°. Hence the difference of rate between the going of a clock with a simple pendulum in summer and in winter, will be about six seconds per day, or one minute in ten days.

The difference of length in straight grained deal wood is found to be very small; whence it is a general opinion, that in clocks of the common constructions, in which the pendulum is without intermission connected with the train, and the parts of the escapement are oiled, the errors from expansion in such a pendulum are much less than those which arise from the unequal transmission of the maintaining power.

But the best method, in practice as well as in theory, for correcting the effects of heat and cold in pendulums, consists in opposing the expansions and contractions of different kinds of metal to each other. The celebrated George Graham was, as far as I am informed, the first who applied this expedient to clocks. His account is in some of the earlier volumes of the *Philosophical Transactions*, which I have not at hand, and therefore make reference to it from memory. Having ascertained that the expansion of mercury greatly exceeds that of iron, he formed a pendulum consisting of an iron tube filled with mercury to a certain height. In this compound pendulum it is clear that, if the whole had been iron, the centre of oscillation would have descended by heat; and it is equally clear that, if the iron tube could be deprived of all expansibility whatever, the mercury supported beneath would be lengthened upwards by increase of temperature; and

in

in this assumed case, the centre of oscillation would ascend by heat. But, as the expansion of mercury is about fifteen times greater than that of iron, if the tube had been nearly filled, the ascent of the mercurial column would be only one-fifteenth part less than if the iron had not expanded at all. When the mercurial column is shortened by pouring out some of the fluid, the effect of its expansion will also be diminished. There is, consequently, a state in which these two contrary expansions shall counteract each other, and the centre of oscillation continue invariable. When a clock thus fitted up gains by heat, the quantity of mercury must be diminished; and the contrary if it loses. I have seen an improvement of this pendulum by Mr. Troughton of Fleet-street, in which a bulb and tube of glass, resembling the common thermometer, was substituted instead of the simple tube of Graham. The variable surface of the mercury was brought into that part of the rod where its changes of elevation were best calculated to counteract the opposite expansion of the whole apparatus.

John Harrison is admitted to be the first who applied the opposite expansions of brass and steel to correct each other, in the apparatus called the gridiron pendulum*. Since the increase by expansion in brass and steel is as 113 to 68, and the expansion in any piece of metal is proportionate to its length, it will follow that these increments will be equal in two rods of these metals respectively, when their lengths are inversely as those numbers. That is to say, if a rod of brass 68 inches long be laid upon a rod of steel 113 inches long, so that one end of each may coincide, and these ends be fastened together; then, if the opposite end of the steel be fixed to some invariable support, the unconfined extremity of the brass rod will also keep its position invariably in all temperatures. For the expansion of the steel will in every case carry the whole bar of brass as much in one direction as the expansion of this last bar would have carried its inner extremity in the opposite direction. We may imagine the ball of a pendulum to be fixed at this invariable point, and the centre of suspension to be at the single end of the steel bar.

But it is inconvenient to have nearly half the pendulum rod beneath the ball; for which reason Mr. Harrison cut the steel bar into three parts, and the brass into two. The first steel bar hung downwards from the centre of suspension. To its lower extremity was attached the first brass bar leading upwards. To the upper extremity of this was attached the second steel bar, which in like manner supported by its lower extremity the second brass bar leading upwards. A double combination, of the nature of the simple pair of bars first described, was thus formed, in which the predominant expansion was upwards, because the brass and steel were nearly equal in length. But the addition of the third steel bar leading downwards from the upper end of the second brass bar, answered the purpose of making the desired compensation. To its lower extremity then the ball of the pendulum was attached.

Pendulums of this construction have been made, though it is certain that such a combination of bars, instead of remaining parallel, would tend, by their own weight and that of the ball, to open like the legs of a pair of compasses, and that the remedy of bracing to keep them together would certainly impede their regular action. Mr. Harrison had recourse to another expedient. Instead of a single steel bar, in the first instance, he made use of

* Elements of Clock and Watch Work, by Alexander Cumming, London, 1766, page 92.

two, connected at some distance asunder by a cross piece above and below, at their extremities, forming a parallelogram. Within and nearly contiguous to these were placed two brass bars, pinned to the lower cross piece, and proceeding upwards to another cross piece which connected their superior extremities. From this last piece hung a second pair of steel bars, within and nearly contiguous to those of brass. This last pair was also connected below by a separate cross piece, like the first, from which a second pair of brass bars proceeded upwards, as before, and were connected at top, like the former pair of the same metal. And lastly, from the upper connecting piece of these brass bars descended one single bar of steel, which passed clear through the lower framing, and served to suspend the lens or ball. In this ingenious contrivance it is evident that the bars of the same metal, which were used in pairs, performed the office of single rods, excepting that they mutually supported each other, so as to need no bracing. It is likewise clear that, since the pairs of brass and steel bars compared with each other are nearly equal in length, the repetition must be more frequent, in order to counteract the expansion of the single steel bar in the middle, in proportion as the difference of their expansions is less. Thus Harrison and the earlier artists cut the steel bar into three parts, and the brass into two, which, with the doubling of the three outer bars, formed a gridiron of seven bars. But at present, by substituting a mixture of zinc and silver in the place of the brass, the expansion of the compound rod is found to be so great, that two pieces of steel and one of the zinc and silver may be conveniently used, which contrivance affords a compound bar of five pieces only. This is the form generally used.

The gridiron pendulum deservedly possesses a higher degree of reputation with astronomers than any other contrivance upon the same principle. But a considerable number of pendulums has been made upon another construction, known by the name of its inventor, Mr. John Ellicott. This artist formed his pendulum-rod of iron; but instead of attaching the ball to this rod, he caused it to rest upon the outer arms of two short levers, moveable upon centre-pins fixed in the iron bar. The inner ends of these levers were acted upon by the lower extremity of a bar of brass of the same figure as the pendulum-rod against which it was applied, and to which its upper extremity was attached. From this arrangement it followed that, upon an increase of temperature, the brass bar, being lengthened more than the iron, would cause the inner tails of the levers to descend more than the fulcrum-pins, and consequently would raise the outer tails, and along with them the ball. By the ingenious application of screws to adjust the bearing parts of the ball upon the levers, the quantity of ascent in the ball itself was so regulated as to counteract the effect of lengthening in the iron bar, which is the true pendulum-rod.

The objections made by curious reasoners to this contrivance are, that the levers may be prevented by friction from obeying minute alterations of temperature; and that the two bars may be bended during the increase of temperature. Mr. Cumming, in his work before quoted, has considerably improved this invention, though, after all, it is liable to more objections than the gridiron. Its chief merit appears to consist in the facility with which it may be adjusted to temperature, with little or no alteration of the adjustment for time.

Several writers on this subject have strongly insisted, that every compensation for temperature, whether in a balance or pendulum, ought to move through the air along with it, in order that the several parts may be heated or cooled together. To this it has been answered,

answered, that, from the necessary circulation of heated air, through its difference of density, and the speed with which metals conduct heat, it is not probable that any appreciable difference of effect can arise from the pendulum itself becoming heated or cooled more speedily than any fixed part of the apparatus which may be applied to obviate its change of figure; and still more it is remarked, that the small variations would correct each other in their return. These arguments appear to possess considerable practical force in favour of fixed compensations, which in some instances are simpler and cheaper than such as move with the pendulum.

The ordinary principle of these combinations is nevertheless such as to imply the use of a lever. If we suppose an inflexible bar of brass, standing upright upon a steady support, to afford the fulcrum to an horizontal lever extending each way; and that a vertical bar of steel be attached to one extremity of the lever, and fastened beneath to the same support, it is certain that the other extremity of the lever will ascend by heat, and descend by cold, accordingly as the fulcrum rises and falls through a greater space than the extremity of the steel bar attached to the opposite end of the lever. If the pendulum, therefore, be supported by a flexible piece of spring to that end of the lever which is farthest from the steel bar, and the spring be slightly confined by a notch in a metallic cock, as usual, the ascent and descent of the whole pendulum, by the operation of this contrivance, will counteract the descent and ascent of the ball by heat and cold. In order that it may accurately do this, it is requisite that the proportion of the arms of the lever should be variable by adjustment.

The direct opposition of the expansion of metals is certainly the most simple and effectual; and that by levers is evidently less so, though capable of being magnified at pleasure. This direct action has been applied in watches to carry a curb or piece of metal which served to lengthen or shorten the effective part of the pendulum-spring, accordingly as the difference of temperature, by altering the diameter of the balance, might render it more or less easy to be moved with a determinate velocity. But I have not heard of any simple contrivance of this nature having been applied to the balance itself. It might not be difficult to construct a balance upon the principle of the gridiron pendulum; but the difficulties of adjustment for temperature, position, and rate would either prove exceedingly great, or call for expedients, hitherto unthought of, to simplify the processes. A very curious contrivance is at present used in our best portable time-pieces, which differs very much from those before described. I do not know that it has ever been scientifically treated, nor even who was the artist that first applied it to watch-work. One artist in a loose uncertain manner informed me, that he understood the contrivance to belong to M. Berthoud at Paris; another positively assured me, that the well-known Pinchbeck in Cockspur-street exhibited a metallic solid thermometer on this principle many years ago. Cumming appears to have known nothing of it in 1766, when he published his *Elements*; neither do I find it in the French *Encyclopédie* published in 1779. I should be happy to do justice to the inventors by the assistance of communications from correspondents. The principle of this contrivance is, simply, that if the faces of two straight bars of metal, differing in their expansibility, be fastened together by riveting or soldering, they will continue straight only so long as the original temperature remains; but if the temperature be augmented, the most expansible metal will become the longer, and a flexure will take place, so as to produce convexity on that side and concavity

on the other. But if the temperature be diminished, the most variable of the two metals, by contracting more than the other, will form a concavity on its side.

It might afford data for interesting mathematical speculation to ascertain what would be the nature of the curves formed by such a combination of materials in all the assumable varieties of figure, tenacity, flexibility, and temperature. The considerations which present themselves in a loose prospect of the subject may be exemplified in two uniform bars of brass and steel, differing only in thickness, measured from the common face of contact.

Metals are capable of being lengthened or shortened by mechanical force, as well as by altering their temperature. Hence, if the temperature of a piece of metal be increased at the same time that its expansion is prevented, the consequence must be a contraction or change in the texture, of the nature of what workmen call hammer hardening. And the contrary effect may be supposed to take place if a mechanical obstacle be opposed to prevent the contraction which would take place by cold. It is known from experiment, that the dilatations and contractions from change of temperature are made with an extremely great force; but from other experiments it is also known, that the tenacities of bodies are sufficient to oppose and greatly diminish the effect of those forces.

If a thin bar of steel be foldered to a very thick bar of brass, it may be inferred that the greater expansions and contractions of the brass will alternately rarefy and condense the adherent steel, at the same time that the face of the brass will probably undergo an opposite change of the same nature, in the vicinity of the steel. It may be imagined that the immediate properties of the metals would in the course of time become changed by this continual effort; that is to say, they might become more or less flexible, tenacious, hard, expansible, dense, &c. Thus bell-wires become hard by pulling; gold, in the hands of the gold-beaters, becomes soft by bending backward and forward; brittleness is produced by twisting, and so forth. The power of the brass to alter the texture of the steel must be derived from two of its qualities; namely, its resistance to flexure, and its strength or resistance to change, of the same kind which was inferred to take place chiefly in the smaller bar. Whenever, therefore, it is required that the expansions and contractions from temperature should be exerted chiefly in bending the compound bar, it is necessary that the strengths and flexibilities of the two bars should be so proportioned that the texture of each should suffer alike.

The increase of dimensions in brass and steel in like elevations of temperature is nearly as two to one. The weight required to break a wire of steel is about double that required to produce the same effect in brass. If a bar of brass be foldered to a bar of steel of half the thickness, their strengths will then be nearly equal; and the yielding, except so far as depends on the mechanical difficulty of bending a thicker bar, will be nearly the same in both. Steel, being less flexible than brass, diminishes the effect of the mechanical advantage of the latter from its thickness, and renders it less necessary to take it into consideration.

When the strengths of the two bars are nearly equal, and the flexibilities great, there will most probably be some line within each bar which will possess the same length as that of the unconfined bar at the same temperature.

If a right line be divided into any number of equal parts, and a number of equal perpendiculars be erected at the several points of division, the distances between the upper extremities of the several adjacent perpendiculars will be equal. But if these intervals be

every one alike increased while the lines joining the lower extremities continue strait and unaltered in length, it may easily be shewn that the right line will become regularly polygonal: and if the number of divisions were indefinitely great, this polygonal line would become circular. The steel and brads, in our experiments, are referable to this theorem; for, while they continue strait, an indefinite number of perpendiculars may be conceived to be drawn from the surface of the steel to the surface of the brads, at right angles to the plane of contact. Change of temperature will render one bar longer than the other; the perpendiculars will diverge, and the right line will become a circle.

The shorter the perpendiculars, that is to say, the thinner the bars, the greater will be the divergency from a given difference of length, and consequently the smaller the circle; that is to say, the diameters will be nearly in proportion to the thicknesses. Accordingly it is found that very thin bars of this construction are much more bended by the same change of temperature than such as are thicker. If a narrow piece of the thin tinned iron called sheet-tin, about eight or ten inches long, be rubbed bright, and wetted with a solution of sal-ammoniac, and upon this there be applied an equal thin slip of sheet brads, also brightened, and wetted with the same solution, and the faces be pressed firmly together by confining them between two thick strait bars of metal, bound together with wire (while in the vice), the whole apparatus may be then gently heated, till the coating of the tin is fused, and folders the faces together. This period is known by placing a small piece of tin upon the outside, which will flow when the heat is sufficiently elevated. When the compound bar thus obtained is bended up in the figure of a pair of tea-tongs, the change of position of its extremities is very perceptible, and is such that (the iron being outside) it not only opens visibly when held to the fire or touched by the hand, but even when breathed upon.

The practical advantage of this construction is, that the range of motion greatly exceeds that of the mere expansion or contraction. The disadvantage to be suspected is, that the properties of the combination may change, so that the original motion produced from a known alteration of temperature may not take place after some years heating and cooling.

When a thin bar of brads is attached to another of steel of nearly the same tenacity and flexibility, and both become expanded by heat, the form of a circular arc will be assumed, of which the radius may be discovered by admeasurement; and the difference in length of the two bars ascertained by an easy calculation. Or conversely, if their expansion be known at any given temperature, it will be easy to determine the curve. In order to ascertain from direct experiment, without reference to pyrometrical tables, the proportion between the lengthening of the brads beyond the steel, and the space moved through by the extremity of a bar of this kind, I had recourse to experiment. A bar of plate steel, 0.033 inch thick, 0.55 inch wide, and 6.4 inches long, was riveted to a perfectly similar and equal bar of plate brads by small iron rivets, as in Figure 1, Plate V. It was then pinned at one end against a strait bar of copper, which it touched through its whole length, from the pin to the other end that was left at liberty. The distance from the pin to the free end was 6.35 inches. Immediately opposite the last-mentioned end a pin was fixed upright in the copper, very near the end, but not touching it. On the pin was slid a spring clip, with a short tail as at C in Figure 2, where AB represents the compound bar and DE the pin. The brads side of the bar was next the copper. When the temperature was 40°
of

of Fahrenheit, both bars touched; but upon immersing the whole in boiling water, the compound bar bended, and pushed the clip through the space of 0.16 inch. This space is the versed sine of the circular arc AC, produced by raising the temperature from 40° to 212° , namely 172° . But as the versed sine 0.16 is to the sine 6.35, so is the sine 6.35 to a fourth number 252; which added to the versed sine gives the diameter 252.23 inches of the circle into which the upper face of the steel bar had bended, and its circumference 792 inches. But the difference between the diameter of the circle, measured on the brads convex outer face, and that bounded by the interior or concave face of the steel, was twice the thickness of the bar, or 0.132 inch. But it seems fair to measure the real lengths of the bars through the middle of their solidities. The difference of diameter of two circles passing through the bars in this manner would consequently be no more than 0.066 inch; and of their circumferences 0.207, which is the expansion of the whole circle of brads beyond the steel. Whence the whole periphery 792 inches is to the length of the bar (neglecting the expansion endways) 6.35 as the whole difference of the circumferences 0.207 inch is to the excess of expansion in the brads beyond the steel 0.00166 inch. Now as the motion of the end of the compound bar by flexure was 0.16 inch, it was nearly one hundred times as great as the linear difference of expansion in the two metals.

Since the versed sines of very small arcs may be taken to be as the squares of the lengths of the arcs themselves, and in similar arcs they are as the diameters, the quantities of deflection measured by the versed sines of equal small arcs in circles of different diameters will be inversely as the squares of the diameters, and directly as the diameters, or more simply in the inverse ratio of the diameters themselves. And, as the steel made use of in this experiment was about five times the thickness of ordinary watch-spring, it will follow, that a compound bar made of watch-spring and brads of the same thickness would have been deflected five hundred times as much as the mean excess of expansion of the brads beyond the steel.

Fig. 10, Plate V, represents the balance constructed on this principle, and known by the name of the expansion balance. The outer part of the rim is brads, and the inner steel. After this compound rim is brought to its figure by turning, it is cut through in three places A, B, C, which sets one end of each third part of the periphery at liberty to move outwards when the temperature is diminished, or inwards when it is increased. D, E, F are three similar and equal masses of metal fitted upon the circular bars in a proper manner to admit of their being fixed at any required distance from the extremity, where the motion is most considerable. G, H, I are three screws, the heads of which may be set nearer to or farther from the centre, and serve as weights to effect the adjustments for position and rate. The peculiar advantage of this balance may be explained as follows: When an increase of heat diminishes the elastic force of the pendulum-spring K, the outer brads rim being lengthened more than the steel, must throw the weights DEF nearer to the axis, and diminish the effect of the inertia of the balance, which consequently is as speedily carried through its vibration as before. And on the contrary, when cold weather adds to the elastic force of the spring, the same weights are also thrown farther out, and prevent the acceleration which would have followed. The exact adjustment of the weights is found by trial of the going of the machine. If it gain by heat, the weights compensate too much, and must be moved farther from the extreme ends of the circular compound bars;

but

but if the gain be produced by cold, the spring predominates, and the weights will accordingly require to be set farther out.

I have not met with any application of this principle to pendulums, though it may be applied to them in a variety of methods. Fig. 3 represents an apparatus by which I have suspended the simple pendulum of a clock in my possession. Its chief advantage consists in the facility of adjusting for temperature and rate, independently of each other, without stopping the clock. It is peculiarly applicable to the pendulum for obtaining an universal measure, contrived by Hatton and improved by Whitehurst and by Dr. George Fordyce*.

In the Figures 3, 4, 5, 6, 7, 8, 9, Plate V, the same letters denote the same things. Fig. 3 exhibits the entire apparatus together, and the other figures shew the parts separately taken. II is a brass platform, which is to be firmly screwed in the horizontal position upon the steady support intended to sustain the pendulum. At OO are two slits through which a pair of springs pass, and serve in the usual manner to afford the centres of suspension of the pendulum. Between two edge bars KK is included the tapped part of a piece of turned steel FF, of which nearly one half L is tapped with a left-hand screw, and the other half R with a right-hand screw of the same thread, a small portion near the middle being reduced to afford a complete termination to the screws. The outer tails at the extremities FF are squared to receive a milled head G for the purpose of moving it round. Two bridge-pieces HH are tapped to serve as nuts to the screws, and are so carefully filed at bottom, together with the edge bars, that when the bars are fixed to the platform the bridge-pieces move stiffly along the face when the screw is turned. The motion, from the nature of the screws, is in each equal, and contrary to the other. At the outer corner above of each bridge-piece an edge is prominent, which is reduced at top, all but the corners, to afford a steady lodgement for a compound bar EE, without suffering it to touch the rest of the surface of those pieces, or to move at all horizontally. The bar EE is composed of brass and steel, the brass being laid uppermost. It is perforated in the middle at Q, Fig. 8, and properly opened, to afford a steady bearing for the lower extremity of the screw AB, Fig. 3, of which the figure may be seen at S, in Figure 7. The screw is provided with a double milled head, and graduated circle, each division of which answers to a difference of one second in the daily rate. The branches MM support a face, bearing either a nonius, or, as I have it, a single stroke, for the purpose of setting the screw AB. The nut C receives the springs of the pendulum in a slit P, terminating above in grooves VV, Fig. 7; and the extremities of the legs TT are sufficiently below S to allow the weight of the pendulum itself to preserve the vertical situation of the screw. From the nut C proceeds a flat plate DD, which fills the whole space between the edge bars KK, and prevents the nut from moving horizontally, while it is left at liberty to rise or fall in the vertical direction.

The effect of this apparatus will be understood without difficulty after this description.

* I do not pretend to insinuate that the practical solution of this problem has yet been cleared of the many difficulties which attend it. The pendulum here mentioned consists of a flat steel wire suspending a weight, which vibrates from a notch in a plate adjustable at different heights to measure the linear difference between the seconds and half-seconds pendulum. Mr. Hatton, as I am informed, applied many years ago to the Society for the Encouragement of Arts for a premium in favour of this invention. Mr. Whitehurst pursued the same object afterwards; and at his death the apparatus was purchased by Dr. George Fordyce, who has given an account of it, with drawings, in the Philosophical Transactions for the year 1794, page 2.

When the pendulum N, Fig. 3, becomes lengthened by heat, it is evident that it would go slower if the greater expansion of the brads in the compound bar EE did not at the same time render the whole bar more convex above, and raise the screw and its nut, and with it the pendulum itself, which is by that means correspondently shortened at O. If the common rate of going from day to day differ from mean time, the adjustment must be made by the screw A. But if this rate, being steady while the temperature continues unaltered, should be found to change when the temperature is changed, the adjustment must be made by the screw FF. That is to say, the bridges HH are to be moved further asunder, if the pendulum loses by heat; but if, on the contrary, heat causes it to gain, the effect of the compensation in the bar EE is too considerable, and the remedy will consist in moving the bridges nearer to each other.

Thus it is seen that an adjustable compensation for temperature may be applied by this method with the same facility as the adjustment for rate. It only remains to be observed, for the instruction of actual workmen, by what method it is ascertained that the bar EE shall possess sufficient variation in its flexure to produce the desired effect within the limits of the bridge-pieces. For this purpose it is advisable to make a compound bar of brads and steel, fastened together by riveting, soft-folding, or brazing; the latter method of which is most advisable. The length of this bar may be six or eight inches, and the thickness of each metal may be between the twentieth and thirtieth part of an inch. After reducing the two bars in their conjoined state to an uniform thickness by filing and calipering as much as may be requisite, the whole flexure of the bar, by a considerable number of degrees alteration of temperature, may be ascertained in the method described by reference to Fig. 2. The space run through by the clip C will be the versed sine of the arc of flexure; whence the versed sines or deviations from the right line may be deduced in shorter lengths; for these are very nearly in the present case as the squares of the lengths of the bar. The effective length of the compound bar EE must be measured from one of the bridges E to the middle point; that is to say, it will be equal to half the distance between the bearing edges when drawn as far as possible from each other. The proportion will therefore be: as the square of the whole bar is to the space moved through by the clip, so is the square of the effective bar of the apparatus to its flexure by expansion. From the length of the intended pendulum, and the nature of the metal of its rod, its direct expansion for the number of degrees in question may be known from various tables, or by experiments. The rates of expansion in brads and steel have already been quoted in this paper. It is taken for granted that the compound bar here spoken of has been made sufficiently thick, and that the deviation by flexure in the effective bar, deduced from the proportion of the squares, will prove less than the direct expansion of the pendulum. If it should prove greater, the bar is too thin, and there is no remedy but to make a thicker. We may however proceed on the supposition that the quantity of deviation in the effective bar is considerably less. It will be most convenient that this deviation should be about one-third less. If the difference do not exceed this quantity, a piece of the long bar may be cut, and fitted to the apparatus. If it do exceed, the flexibility of the bar from temperature will be increased by reducing it on both sides with the file. The quantity of deviation will be nearly in the inverse proportion of the thickness, as has already been stated. Whence it is obvious, that the requisite thickness may be deduced and acquired from the original experiment of flexure by change of temperature. But it is safer and better to take the small additional trouble of

ascertaining the rate of flexure from time to time during the filing, by means of the simple apparatus, Fig. 2.

III.

Observations and Experiments on the Light, Expence and Construction of Lamps and Candles, and the Probability of rendering Tallow a Substitute for Wax.

IF we distribute the catalogue of human wants in the order of the necessity of each, food will occupy the first place; and, next to this, in most climates of the earth, the articles of fuel, lodging, and clothing will immediately present themselves. The total want of any of these necessarily implies extreme distress; and if such a privation be applied even in fancy to an entire society, every notion of comfort and civilization seems at once to disappear. Inferior only to these in its urgency is the necessity of artificial light during the absence of the sun. We might indeed exist without it; but how large a portion of our lives would in that case be condemned to a state little superior in efficacy to that of the animals around us! It might be a curious speculation to enquire how far and in what respects the morals of man would become degraded by the want of so important an art; since every useful art must surely in its consequences affect the moral principles of society. But it is sufficient on the present occasion that, previous to entering upon a dissertation respecting the art of illumination, a train of ideas has slightly been pointed out, which cannot fail to shew its magnitude and importance.

We are acquainted with no means, unless we may except electricity, of producing light but by combustion, and this is most probably of the same nature. The rude method of illumination consists in successively burning certain masses of fuel in the solid state. Common fires answer this purpose in the apartments of houses, and in some light-houses: small pieces of resinous wood, and the bituminous coal called kannel-coal, are in some countries applied to the same use; but the most general and useful method is that in which fat oil, of an animal or vegetable kind, is burned by means of a wick. These instruments of illumination are either lamps or candles. In the lamp, the oil must be one of those which retains its fluidity in the ordinary temperature of the atmosphere. The candle is formed of an oil, or other material, which is not fusible but at a temperature considerably elevated.

The method of measuring the comparative intensities of light is one of the first requisites in an enquiry concerning the art of illumination. Two methods of considerable accuracy are described in the *Traité d'Optique* of Bouguer, of which an abridged account is given by Dr. Priestley in his *Optics**, page 540. The first of these two methods has been used by others since that time, and probably before, from its very obvious nature, but particularly by Count Rumford, who has given a description and drawings of an instrument called the Photometer, in the *Philosophical Transactions* for 1794. The principle it is grounded upon is, that if two lights shine upon the same surface at equal obliquities, and an opaque body be interposed, the two shadows it will produce must differ in blackness or intensity in the same degree. For the shadow formed by intercepting the greater light

* The title of this well-known work is: *The History and present State of Discoveries relating to Vision, Light, and Colours.* By Joseph Priestley, LL. D. F. R. S. London. Johnson, 1772.

will be illuminated by the smaller light only, and reversely the other shadow will be illuminated by the greater light. That is to say, in short, the stronger light will be attended with the deeper shadow. But it is easy, by removing the greater light to a greater distance, to render the illumination it produces at the common surface equal to that afforded by the less. Experiments of this kind may be conveniently made by fastening a sheet of white paper against the wall of a room. The two lights or candles intended to be compared, must then be placed so that the ray of light from each shall fall with nearly the same angle of incidence upon the middle of the paper. In this situation, if a book or other object be held to intercept part of the light which would have fallen on the paper, the two shadows may be made to appear as in Figure 10, Plate V, where A represents the surface illuminated by one of the lights only; B, the surface illuminated by the other light; C, the perfect shadow from which both lights are excluded. It will easily be understood that the lights about D and E, near the angle F, will fall with equal incidences when the double shadow is made to occupy the middle of the paper; and consequently, if one or both of the lights be removed directly towards or from the paper, as the appearances may require, until the two shadows at E and D have the same intensity, the quantities of light emitted by each will be as the squares of the distances from the paper. By some experiments made in this way in the year 1785, I was satisfied that the degree of illumination could be thus ascertained to the eightieth or ninetieth part of the whole.

By experiments of this kind many useful particulars may be shewn. Thus, for example, the light of a candle, which is so exceedingly brilliant when first snuffed, is very speedily diminished to one-half, and is usually not more than one-fifth or one-sixth before the uneasiness of the eye induces us to snuff it. Whence it follows, that if candles could be made so as not to require snuffing, the average quantity of light afforded by the same quantity of combustible matter would be more than doubled. In the same way, likewise, since the cost and duration of candles, and the consumption of oil in lamps, are easily ascertainable, it may be shewn whether more or less of light is obtained at the same expence during a given time, by burning a number of small candles instead of one of greater thickness. From a few experiments already made out of the numerous and useful series that presents itself, I have reason to think that there is very much waste in this expensive article of accommodation.

In the lamp there are three articles which demand our attention, the oil, the wick, and the supply of air. It is required that the oil should be readily inflammable, without containing any fetid substance which may prove offensive, or mucilage, or other matter, to obstruct the channels of the wick. I do not know of any process for meliorating oils for this purpose, excepting that of washing with water containing acid or alkali. Either of these is said to render the mucilage of animal oils more soluble in the water; but acid is preferred, because it is less disposed to combine with the oil itself. The office of the wick appears to be chiefly, if not solely, to convey the oil by capillary attraction to the place of combustion. As the oil is consumed and flies off, other oil succeeds, and in this way a continued current of oil and maintenance of the flame are effected. But as the wicks of lamps are commonly formed of combustible matter, it appears to be of some consequence what the nature and structure of this material may be. It is certain that the flame afforded by a wick of rush differs very considerably from that afforded by cotton; though perhaps this difference may, in a great measure, depend on the relative dimensions of each. And if

we may judge from the different odour in blowing out a candle of each sort, there is some reason to suspect that the decomposition of the oil is not effected precisely in the same manner in each. We have also some obscure accounts of prepared wicks for lamps, which are stated to possess the property of facilitating the combustion of very impure oils, so that they shall burn for many hours without smoke or smell*.

The access of air is of the last importance in every process of combustion. When a lamp is fitted up with a very slender wick, the flame is small, and of a brilliant white colour: if the wick be larger, the combustion is less perfect, and the flame is brown: a still larger wick not only exhibits a brown flame, but the lower internal part appears dark, and is occupied by a portion of volatilised matter, which does not become ignited until it has ascended towards the point. When the wick is either very large or very long, part of this matter escapes combustion, and shews itself in the form of coal or smoke. The different intensity of the ignition of flame, according to the greater or less supply of air, is remarkably seen by placing a lamp with a small wick beneath a shade of glass not perfectly closed below, and more or less covered above. While the current of air through the glass shade is perfectly free, the flame is white; but in proportion as the aperture above is diminished, the flame becomes brown, long, wavering, and smoky; it instantly recovers its original whiteness when the opening is again enlarged. The inconvenience of a thick wick has been long since observed, and attempts made to remove it; in some instances by substituting a number of small wicks instead of a larger; and in others, by making the wick flat instead of cylindrical. The most scientific improvement of this kind, though perhaps less simple than the ordinary purposes of life demand, is the well-known lamp of Argand. In this the wick forms a hollow cylinder or tube, which slides over another tube of metal, so as to afford an adjustment with regard to its length. When this wick is lighted, the flame itself has the figure of a thin tube, to the inner as well as the outer surface of which the air has access from below. And a cylindrical shade of glass serves to keep the flame steady, and in a certain degree to accelerate the current of air. In this very ingenious apparatus many experiments may be made with the greatest facility. The inconvenience of a long wick, which supplies more oil than the volume of flame is capable of burning, and which consequently emits smoke, is seen at once by raising the wick; and on the other hand, the effect of a short wick, which affords a diminutive flame merely for want of a sufficient supply of combustible matter, is observable by the contrary process.

The most obvious inconvenience of lamps in general, arises from the fluidity of the combustible material, which requires a vessel adapted to contain it, and even in the best

* The economical wicks of M. Leger, concerning which a Report was presented to the Academy at Paris in 1782 by Condorcet, Lavoisier, and De Milly, were composed of cotton of different sizes and forms, namely, round and flat, according to the use they were intended to serve. They were covered with a fat substance, of a smell not disagreeable, but feebly aromatic. From the trials of these commissaries it was ascertained: 1. That they afforded a clearer flame, with less undulation. 2. That they consumed somewhat less oil; and 3. That they possessed the remarkable property of affording neither smell nor smoke, however common the oil made use of. I find a difficulty respecting these experiments, where it is said that the flame with the prepared wick was only ten lines long, and very steady; while that of the unprepared wick of comparison was four inches and a quarter long, of a conical figure, and vibrated much. It seems as if this last had been disadvantageously trimmed, or too much raised, in order to have produced such an extravagant flame. It soon blackened a silver plate at upwards of a foot distance.

constructed lamps is more or less liable to be spilled. When the wick of a lamp is once adjusted as to its length, the flame continues nearly in the same state for a very considerable time.

It is almost unnecessary to describe a thing so universally known as a candle. This article is formed of a consistent oil, which envelopes a porous wick of fibrous vegetable matter. The cylindrical form and dimensions of the oil are given either by casting it in a mould, or by repeatedly dipping the wick into the fused ingredient. Upon comparing a candle with a lamp, two very remarkable particulars are immediately seen. In the first place, the tallow itself, which remains in the unfused state, affords a cup or cavity to hold that portion of melted tallow which is ready to flow into the lighted part of the wick. In the second place, the combustion, instead of being confined, as in the lamp, to a certain determinate portion of the fibrous matter, is carried, by a slow succession, through the whole length. Hence arises the greater necessity for frequent snuffing the candle; and hence also the station of the freezing point of the fat oil becomes of great consequence. For it has been shewn that the brilliancy of the flame depends very much on the diameter of the wick being as small as possible; and this requisite will be most attainable in candles formed of a material that requires a higher degree of heat to fuse it. The wick of a tallow candle must be made thicker in proportion to the greater fusibility of the material, which would otherwise melt the sides of the cup, and run over in streams. The flame will therefore be yellow, smoky, and obscure, excepting for a short time immediately after snuffing. Tallow melts at the 92d degree of Fahrenheit's thermometer; spermaceti at the 133d degree; the fatty matter formed of flesh after long immersion in water melts at 127°; the *pela** of the Chinese, at 145°; bees-wax at 142°; and bleached wax at 155°. Two of these materials are well known in the fabrication of candles. Wax in particular does not afford so brilliant a flame as tallow: but, on account of its less fusibility, the wick can be made smaller; which not only affords the advantage of a clear perfect flame, but from its flexibility it is disposed to turn on one side, and come in contact with the external air, which completely burns the extremity of the wick to white ashes, and thus performs the office of snuffing. We see, therefore, that the important object to society of rendering tallow candles equal to those of wax, does not at all depend on the combustibility of the respective materials, but upon a mechanical advantage in the cup, which is afforded by the inferior degree of fusibility in the wax; and that, to obtain this valuable object, one of the following effects must be produced: Either the tallow must be burned in a lamp, to avoid the gradual progression of the flame along the wick; or some means must be devised to enable the candle to snuff itself, as the wax candle does; or, lastly, the tallow itself must be rendered less fusible by some chemical process. I have no great reason to boast of success in the endeavour to effect these; but my hope is, that the facts and observations here presented may considerably abridge the labour of others in the same pursuit.

The makers of thermometers and other small articles with the blow-pipe and lamp, give the preference to tallow instead of oil, because its combustion is more complete, and does not blacken the glass. In this operation, the heat of the lamp melts the tallow which is occasionally brought into its vicinity by the workman. But for the usual purposes of illumination, it cannot be supposed that a person can attend to supply the combustible matter. Considerable difficulties

* Pearson, in the *Philosophical Transactions* for 1794.

arise in the project for affording this gradual supply as it may be wanted. A cylindrical piece of tallow was inserted into a metallic tube, the upper aperture of which was partly closed by a ring, and the central part occupied by a metallic piece nearly resembling that part of the common lamp which carries the wick. In this apparatus the piece last described was intended to answer the same purpose, and was provided with a short wick. The cylinder of tallow was supported beneath in such a manner that the metallic tube and other part of this lamp were left to rest with their whole weight upon the tallow at the ring or contraction of the upper aperture. In this situation the lamp was lighted. It burned for some time with a very bright clear flame, which, when compared with that of a candle, possessed the advantage of uniform intensity, and was much superior to the ordinary flame of a lamp in its colour, and the perfect absence of smell. After some minutes it began to decay, and very soon afterwards went out. Upon examination it was found, that the metallic piece which carried the wick had fused a sufficient quantity of tallow for the supply during the combustion; that part of this tallow had flowed beneath the ring, and to other remote parts of the apparatus, beyond the influence of the flame; in consequence of which, the tube, and the cylinder of tallow were fastened together, and the expected progression of supply prevented. It seems probable, that in every lamp for burning consistent oils, the material ought to be so disposed that it may descend to the flame upon the principle of the fountain reservoir. I shall not here state the obstacles which present themselves in the prospect of this construction, but shall dismiss the subject by remarking, that a contrivance of this nature would be of the greatest public utility.

The wick of a candle, being surrounded by the flame, is nearly in the situation of a body exposed to destructive distillation in a close vessel. After losing its volatile products, the carbonaceous residue retains its figure, until, by the descent of the flame, the external air can have access to its upper extremity. But, in this case, the requisite combustion, which might snuff it, is not effected. For the portion of oil emitted by the long wick is not only too large to be perfectly burned, but also carries off much of the heat of the flame while it assumes the elastic state. By this diminished combustion and increased efflux of half-decomposed oil, a portion of coal or soot is deposited on the upper part of the wick, which gradually accumulates, and at length assumes the appearance of a fungus. The candle does not then give more than one-tenth of the light emitted in its best state. Hence it is that a candle of tallow cannot spontaneously snuff itself. It was not probable that the addition of a substance containing vital air or oxygen would supply that principle at the precise period of time required; but, as experiment is the test of every probability of this nature, I soaked a wick of cotton in a solution of nitre, then dried it, and made a candle. When this came to be lighted, nothing remarkable happened for a short time; at the expiration of which a decrepitation followed at the lower extremity of the flame, which completely divided the wick where the blackened part commences. The whole of the matter in combustion therefore fell off, and the candle was of course instantly extinguished. Whether this would have happened in all proportions of the salt or constructions of the candle I did not try, because the smell of azote was sufficiently strong and unpleasant to forbid the use of nitre in the pursuit. From various considerations I am disposed to think that the spontaneous snuffing of candles made of tallow, or other fusible materials, will scarcely be effected but by the discovery of some material for the wick which shall be voluminous enough

enough to absorb the tallow, and at the same time sufficiently flexible to bend on one side.

The most promising speculation respecting this most useful article, seems to direct itself to the cup which contains the melted tallow. The imperfection of this part has already been noticed, namely, that it breaks down by fusion, and suffers its fluid contents to escape. The Chinese have a kind of candle about half an inch in diameter, which, in the harbour of Canton, is called a *lobchock*; but whether the name be Chinese, or the corruption of some European word, I am ignorant. The wick is of cotton, wrapped round a small stick or match of the bamboo cane. The body of the candle is white tallow; but the external part to the thickness of perhaps one-thirtieth of an inch, consists of a waxy matter coloured red. This covering gives a considerable degree of solidity to the candle, and prevents its guttering, because less fusible than the tallow itself. I did not observe that the stick in the middle was either advantageous or the contrary; and, as I now write from the recollection of this object at so remote a period as twenty-five years ago, I can only conjecture that it might be of advantage in throwing up a less quantity of oil into the flame than would have been conveyed by a wick of cotton sufficiently stout to have occupied its place unsupported in the axis of the candle.

Many years ago I made a candle in imitation of the *lobchock*. The expedient to which I had recourse consisted in adapting the wick in the usual pewter mould: wax was then poured in, and immediately afterwards poured out: the film of wax which adhered to the inner surface of the mould soon became cool; and the candle was completed by filling the mould with tallow. When it was drawn out, it was found to be cracked longitudinally on its surface, which I attributed to the contraction of the wax, by cooling, being greater than that of the tallow. At present I think it equally probable that the cracking might have been occasioned by too sudden cooling of the wax before the tallow was poured in; but other avocations prevented the experiments from being varied and repeated. It is probable that the Chinese external coating may not be formed of pure hard bleached wax.

But the most decisive remedy for the imperfection of this cheapest, and in other respects best material for candles, would undoubtedly be to diminish its fusibility. Various substances may be combined with tallow, either in the direct or indirect method. In the latter way, by the decomposition of soap, a number of experiments were made by Berthollet, of which an account is inserted in the Memoirs of the Academy at Paris for the year 1780, and copied into the 26th volume of the Journal de Physique. None of these point directly to the present object; besides which, it is probable that the soap made use of by that eminent chemist was formed not of tallow, but oil. I am not aware of any regular series of experiments concerning the mutual action of fat oils and other chemical agents, more especially such as may be directed to this important object of diminishing its solubility; for which reason I shall mention a few experiments made with this view.

1. Tallow was melted in a small silver vessel. Solid tallow sinks in the fluid, and dissolves without any remarkable appearance.
2. Gum sandarach in tears was not dissolved, but emitted bubbles, swelled up, became brown, emitted fumes, and became crisp or friable. No solution nor improvement of the tallow.
3. Shell-lac swelled up with bubbles, and was more perfectly fused than the gum sandarach in the former experiment. When the tallow was poured off, it was thought to congeal rather more speedily. The lac did not appear

appear to be altered. 4. Benzoin bubbled without much swelling, was fused, and emitted fumes of an agreeable smell, though not resembling the flowers of benzoin. A slight or partial solution seemed to take place. The benzoin was softer and of a darker colour than before, and the tallow less consistent. 5. Common resin unites very readily with melted tallow, and forms a more fusible compound than the tallow itself. 6. Camphor melts easily in tallow, without altering its appearance. When the tallow is near boiling, camphoric fumes fly off. The compound appeared more fusible than tallow. 7. The acid or flowers of benzoin dissolves in great quantities without any ebullition or commotion. Much smoke arises from the compound, which does not smell like the acid of benzoin. Tallow alone does not fume at a low heat, though it emits a smell something like that of oil-olive. When the proportion of the acid was considerable, small needled crystals appeared as the temperature diminished. The appearances of separation are different according to the quantity of acid. The compound has the hardness and consistence of firm soap, and is partially transparent. 8. Vitriolated tartar, nitre, white sugar, cream of tartar, crystallized borax, and the salt sold in the markets under the name of salt of lemons, but which is supposed to be the essential salt of sorrel, or vegetable alkali supersaturated with acid of sugar, were respectively tried without any obvious mutual action or change of properties in the tallow. 9. Calcined magnesia rendered tallow opaque and turbid, but did not seem to dissolve. Its effect resembled that of lime.

It is proposed to try the oxygenated acetous acid, or radical vinegar; the acid of ants, of sugar, of borax, of galls, the tanning principle, the serous and gelatinous animal matter, the fecula of vegetables, vegetable gluten, bird-lime, and other principles, either by direct or indirect application. The object, in a commercial point of view, is entitled to an extensive and assiduous investigation. Chemists in general suppose the hardness or less fusibility of wax to arise from oxygen, and to this object it may perhaps be advantageous to direct a certain portion of the enquiry. The metallic salts and calces are the combinations from which this principle is most commonly obtained; but the combinations of these with fat oils have hitherto afforded little promise of the improvement here sought. The subject is however so little known, that experiments of the loosest and most conjectural kind are by no means to be despised.

IV.

A Memoir upon the Discovery of America. By M. OTTO.*

IT has always been looked upon as a piece of injustice, not to have given the name of Columbus to that valuable part of the world which he discovered; and that Americus Vespucius, who did nothing but follow his footsteps, has had the good fortune of having his name handed down to the most distant posterity, to the prejudice of his predecessor. What then will be said, if it shall be proved that neither of those celebrated navigators was the first discoverer of this immense country; and that this honour belongs to a man scarcely known in the republic of letters? This, however, is what I shall attempt in the following paper; and if the obscurity of cotemporary writers, and the distance of time, do not afford arguments sufficient for an absolute demonstration, there will, however, be enough to call in question the pretensions of Christopher Columbus.

* From the American Transactions, Vol. II. in French and English. The former appears to be the original; for which reason I have compared this with it, and corrected several passages.

I shall not here enter into an examination of the reveries of some historians, on the voyages of the Carthaginians, the Atlantis of Plato, the bold expedition of Mador Prince of Wales, and son of Owen Guinnett, of which Hakluyt has preserved some account, nor on the voyages of Bæchus, or the land Ophir of Solomon. Conjectures of this kind, whether true or false, cannot lessen the glory of Columbus, were there not proof that he received, just before his expedition, the charts and journal of a learned astronomer who had been in America.

Garcilasso de la Vega, born at Cusco in Peru, has given us an history of his country, in which, to take from Columbus the merit of the discovery of America, and to give the honour of it to the Spaniards, he assures us, that this navigator had been informed of the existence of another continent by Alonzo Sanchez de Huelva, who, in his voyage to the Canaries, had been driven by a gale of wind to the Antilles; but that his chief information was procured from a celebrated geographer of the name of Martin Behenira. Garcilasso says nothing more of this Behenira: and since we know of no Spanish geographer of this name, Garcilasso has been suspected of making a sacrifice of truth to the desire of wresting from a Genoese the glory of discovering the New World.

On looking over with attention a list of all the learned men of the fifteenth century, I find the name of Martin Behem, a famous geographer and navigator. The christian name is the same with that mentioned by Garcilasso, and I find that the syllables *ira* added to his name are owing to a particular circumstance, namely, the honour conferred on him by John II. King of Portugal. It is then possible that this Martin Behem is the same person as Martin Behenira mentioned by Garcilasso; but this vague conjecture will receive the stamp of truth by the following detail:

The literary history of Germany gives an account of a Martin Behem, Beheim or Behin, who was born at Nuremberg, an Imperial city of the circle of Franconia, of a noble family, some branches of which are yet extant. He was much addicted to the study of geography, astronomy, and navigation from his infancy. At a more mature age, he often thought on the possibility of the existence of the Antipodes, and of a western continent. Filled with this great idea, he paid a visit in 1459 to Isabella, daughter of John I. King of Portugal, and Regent of the duchy of Burgundy and Flanders. Having informed her of his designs, he procured a vessel, in which he made the discovery of the island of Fayal in 1460. He there established a colony of Flemings, whose descendants yet exist in the Azores, which were for some time called the Flemish Islands. This circumstance is proved, not only by the writings of cotemporary authors, but also by the manuscripts preserved in the records of Nuremberg, from the Latin of which the following is translated: "Martin Behem tendered his services to the daughter of John King of Lusitania, who reigned after the death of Philip of Burgundy, surnamed the Good; and from her procured a ship, by means of which, having sailed beyond all the then known limits of the Western Ocean, he was the first who in the memory of man discovered the island of Fayal, abounding with beech trees, which the people of Lusitania call Faye; whence it derived its name. After this he discovered the neighbouring islands, called by one general name, the Azores, from the multitude of hawks which build their nests there (for the Lusitanians use this term for hawks, and the French too use the word *essos* or *esores* in their pursuit of this game); and left colonies of the Flemish on them, when they began to be called Flemish Islands, &c." Although this record is contrary to the generally received opinion that the Azores were discovered by Gonçalva Velho, a Portuguese, yet its authenticity cannot be doubted: it is confirmed by several cotemporary

temporary writers, and especially by Wagenfeil, one of the most learned men of the last century; who, after having travelled into Africa, and throughout all Europe, was made Doctor of Laws at Orleans, and chosen Fellow of the Academies of Turin and Padua, although he was a German by birth. The particulars are to be found in his *Universal History and Geography*. I have moreover received from the records of Nuremberg a note written in German, on parchment, which contains the following facts: "Martin Behem, Esquire, son of Mr. Martin Beham, of Scoperin, lived in the reign of John II. King of Portugal, in an island which he discovered, and called the Island of Fayal, one of the Azores, lying in the Western Ocean."

After having obtained from the Regent Isabella a grant of Fayal, and resided there about twenty years, during which time he was busied in making fresh discoveries in geography, by small excursions which need not be mentioned, Behem applied in 1484 (which was eight years before Columbus's expedition) to John II. King of Portugal, to procure the means of undertaking a great expedition towards the south-west. This prince gave him some ships, with which he discovered that part of America which is now called Brazil; and he even failed to the Straits of Magellan, or to the country of some savage tribes whom he called Patagonians, from the extremities of their bodies being covered with a skin more like a bear's paws than human hands and feet. This fact is proved by authentic records, preserved in the archives of Nuremberg, one of which in particular deserves attention: "Martin Behem, traversing the Atlantic Ocean for several years, examined the American islands, and discovered the Strait which bears the name of Magellan, before either Christopher Columbus or Magellan failed those seas; whence he mathematically delineated on a geographical chart, for the King of Lusitania, the situation of the coast around every part of that famous and renowned Strait long before Magellan thought of his expedition." This assertion is supported by Behem's own letters, written in German, and preserved in the archives of Nuremberg, in a book which contains the birth and illustrious actions of the nobility of that city. These letters are dated in 1486, that is, six years before the expedition of Columbus. This wonderful discovery has not escaped the notice of cotemporary writers. The following passage is translated from the Latin chronicle of Hartman Schedl: "In the year 1485, John II. King of Portugal, a man of a magnanimous spirit, furnished some galleys with provisions, and sent them to the southward, beyond the Straits of Gibraltar. He gave the command of this Squadron to James Canus, a Portuguese, and Martin Behem, a German of Nuremberg in Upper Germany, descended of the family of Bonna: a man very well acquainted with the situation of the globe; blessed with a constitution able to bear the fatigues of the sea; and who, by actual experiments and long sailing, had made himself perfectly master with regard to the longitudes and latitudes of Ptolemy in the west. These two, by the bounty of Heaven, coasting along the Southern Ocean, and having crossed the Equator, got into the other hemisphere, where, facing to the eastward, their shadows projected towards the south and right hand. Thus, by their industry, they have opened to us another world, hitherto unknown, and for many years attempted by none but the Genoese, and by them in vain. Having finished this cruise in the space of twenty-six months, they returned to Portugal with the loss of many of their seamen by the violence of the climate."

This passage becomes more interesting from being quoted in a book on the state of Europe during the reign of the Emperor Frederick III. by the learned historian *Aeneas Sylvius*, afterwards Pope Pius II. This historian died before the discoveries of Behem were made;

made; but the publishers of his works thought the passage in Hartman Schedl so important, that they inserted it in the history. We also find the following particulars in the remarks made by Petrus Mateus on the canon law, two years before the expedition of Columbus: "*Prime navigationes, &c.*—The first Christian voyages to the newly discovered islands became frequent under the reign of Henry, son of John, King of Lusitania. After his death Alphonfus V. prosecuted the design; and John, who succeeded him, followed the plan of Alphonfus, by the assistance of Martin Bohem, a very skilful navigator; so that in a short time the name of Lusitania became famous over the whole world." Cellarius, one of the most learned men of his age, says expressly, "*Behaimus non modo, &c.*—Bœhm did not think it enough to survey the island of Fayal, which he first discovered, or the other adjacent islands which the Lusitanians call Azores, and we, after the example of Bœhm's companions, call Flemish islands, but advanced still farther and farther south, until he arrived at the remotest strait, through which, Ferdinand Magellan, following his track, afterwards sailed, and called it after his own name." All these quotations, which cannot be thought tedious, since they serve to prove a fact almost unknown, seem to demonstrate, that the first discovery of America is due to the Portuguese, and not to the Spaniards; and that the chief merit belongs to a German astronomer. The expedition of Ferdinand Magellan, which did not take place before the year 1519, arose from the following fortunate circumstance: This person, being in the apartment of the King of Portugal, saw there a chart of the coast of America, drawn by Behem, and at once conceived the bold project of following the steps of this great navigator. Jerome Benzon, who published a description of America in 1550, speaks of this chart; a copy of which, sent by Behem himself, is preserved in the archives of Nuremberg. The celebrated astronomer Riccioli, though an Italian, yet does not seem willing to give his countryman the honour of this important discovery. In his *Geographia Reformata*, book iii. p. 90, he says, "Christopher Columbus never thought of an expedition to the West Indies until his arrival in the island of Madeira, where, amusing himself in forming and delineating geographical charts, he obtained information from Martin Bœhm, or, as the Spaniards say, from Alphonfus Sanchez de Huelva, a pilot, who had chanced to fall in with the island afterwards called Dominica." And in another place: "Let Bœhm and Columbus have each their praise; they were both excellent navigators; but Columbus would never have thought of his expedition to America, had not Bœhm gone there before him. His name is not so much celebrated as that of Columbus, Americus, or Magellan, although he is superior to them all."

But the most positive proof of the great services rendered to the crown of Portugal by Behem, is the recompense bestowed on him by King John, who in 1485 knighted him in the most solemn manner, in the presence of all his court. I have before me a German paper, extracted from the archives of Nuremberg, to the following purport: "In the year 1485, on the 18th of February, in Portugal, in the city of Allavavas, and in the church of St. Salvador, after the mass, Martin Behem of Nuremberg was made a knight, by the hands of the most puissant Lord John the Second, King of Portugal, Algarve, Africa, and Guinea; and his chief Squire was the King himself, who put the sword in his belt; and the Duke of Begia was his second Squire, who put on his right spur; and his third Squire was Count Christopher de Mela, the King's cousin, who put on his left spur; and his fourth Squire was Count Martini Marbarinis, who put on his iron helmet; and the King himself gave him the blow on the shoulder; which was done in the presence of all the princes,

princes, lords, and knights of the kingdom; and he espoused the daughter of a great lord, in consideration of the important services he had performed; and he was made governor of the island of Fayal." These marks of distinction, conferred on a stranger, could not be meant as a recompense for the discovery of the Azores, which was made twenty years before, but as a reward for the discovery of Congo, from whence the Chevalier Behem had brought gold and different kinds of precious wares. This discovery made much greater impression than that of a western world, made at the same time, but which neither increased the wealth of the royal treasury, nor satisfied the avarice of the merchants.

In 1492 the Chevalier Behem, crowned with honours and riches, undertook a journey to Nuremberg, to visit his native country and his family. He there made a terrestrial globe, which is looked on as a master-piece for that time, and which is still preserved in the library of that city. The outline of his discoveries may there be seen, under the name of western lands; and from their situation it cannot be doubted that they are the present Coasts of Brazil, and the environs of the Straits of Magellan. This globe was made in the same year that Columbus set out on his expedition; therefore it is impossible that Behem could have profited by the works of this navigator, who besides went a much more northerly course.

[To be concluded in the next Number.]

V.

Analysis of the Oriental Lapis Lazuli. By M. KLAPROTH.*

THE analysis of lapis lazuli made by Margraff has shewn that the blue colour of this stone is not owing to copper, as has commonly been thought, but that it arises from iron. But as we have not hitherto possessed any accurate analysis of this stone, I have thought it might be useful to examine it anew. In fact, Margraff informs us, that lime, gypsum, and silice, together with iron, are its component parts; but he does not determine their proportions; and his analysis is incomplete, as he does not mention the alumine which this stone also contains.

According to Rinmann, the lapis lazuli contains lime, quartz, iron, and the acid of fluor. I have not found the latter; and it is probable that Rinmann's opinion was grounded upon the phosphorescence of this stone when it is heated.

Cronstedt and some others have affirmed that the lapis contains a quantity of silver, after the rate of two ounces per quintal. But my essays have afforded no certain indication of the presence of this metal.

I selected for experiment a species of the lapis of a beautiful deep blue colour, and carefully separated the white and pyritous specks.

A. One hundred parts of lapis lazuli in thin flakes were kept in a state of ignition for half an hour in a porcelain crucible. They lost two parts of their weight; the colour was not at all changed. This permanence at a great heat induced me to think that the stone might be of advantage in enamelling; to which opinion I was the more inclined, from

* From the *Annales de Chimie*, XXI. 150. The French translation is made from the German, by Citizen Tassaert, but whence taken is not said.

Bergmann having presumed that the Chinese and Japanese make use of it for the blue colour of their porcelain *. To ascertain the truth, I mixed some of the very fine powder of lapis lazuli with a proper flux, and disposed it upon porcelain, which I afterwards placed in the enameller's furnace. My expectations were not realised, for the colour changed to a blueish grey.

B. By exposure to a more violent fire the lapis was deprived of twelve centenaries of its weight, and was vitrified. I apprehend that the two parts lost by the first ignition consisted merely of water, and that the additional ten in the second essay consisted for the most part of carbonic acid. This opinion is supported by the effervescence of the stone, which takes place when an acid is poured upon it, and indicates, though very feebly, that part of the calcareous earth is united to carbonic acid.

C. Upon two hundred grains of the lapis in impalpable powder I poured muriatic acid diluted with an equal measure of water (*étendu de moitié eau*), and digested the whole together by a progressive heat. The colour became gradually changed to a grey ash-colour; and when the ebullition commenced the powder was more strongly attacked, and at length became converted into a thick jelly. This was diluted with water; after which nitric acid was added, and the whole boiled until the residue had become white. The filtered solution was of a pale yellow colour.

D. The product which remained upon the filter had the appearance of sand, and weighed one hundred and thirty-eight grains. When this was mixed with three parts of caustic pot-ash, and ignited, it afforded a greenish mass. The solution of the mass in water was perfectly colourless. By an excess of muriatic acid I separated the siliceous matter, which after ignition weighed 57 grains.

E. The solutions C and D, decomposed by the carbonate of pot-ash, afforded a yellow white precipitate, which, when dried, amounted to 221 grains, and was re-dissolved in the muriatic acid.

F. Ammoniac separated from the solution E a gelatinous precipitate. This was thrown still wet into a caustic lixivium, in which I digested it. It was not totally dissolved, but left a yellowish residue weighing 113 grains.

G. The fluid which remained after the precipitation by ammoniac was treated with carbonate of pot-ash, and afforded 59 grains of the carbonate of lime.

H. Upon the 113 grains which were insoluble in the caustic pot-ash, I poured sulphuric acid diluted with water. This mixture, after having been heated, assumed the form of a jelly. It was diluted with a large quantity of water, and afforded a precipitate of silica which after ignition weighed 29 grains.

I. After the separation of the silica I poured ammoniac into the solution. The precipitate, still humid, being mixed with a caustic lixivium, deposited brown flocks in the liquor, which when dry weighed 13 grains. I dissolved these in the muriatic acid; and this solution treated with ammoniac let fall the oxide of iron, which after ignition weighed six grains. The carbonate of ammoniac likewise precipitated five grains of calcareous earth.

K. The solutions F and I by the caustic alkalis were treated with the muriatic acid. The

* *Opusculs Physiques et Chymiques de Bergmann, Vol. IV. page 32.* From the good quality of the cobalt at present used for this purpose in England and elsewhere, there is no reason to think that any other material is used for full deep blues in the East. N.

precipitate which was formed and re-dissolved by excess of acid, and afterwards formed again by carbonate of pot-ash, was dissolved again by sulphuric acid. A new precipitate of flex was thus obtained, which after ignition weighed six grains. The flex was separated, and pot-ash being then added, crystals of alum were obtained. These were dissolved in water, and the alumine, after precipitation, drying, and ignition, weighed 29 grains.

L. I had ascertained by a previous experiment, that all the calcareous earth contained in the lapis was not saturated by the carbonic acid, but that part was combined with the sulphuric acid. I had boiled in a large quantity of water a portion of the pulverised stone; the water, after filtration, did not appear very transparent. I poured in the muriate of barytes, and a precipitate was immediately formed consisting of sulphate of barytes. In order to ascertain the proportion of sulphate of lime, I supersaturated with muriatic acid the liquor which remained from the precipitate C and the water of edulcoration; after which I poured in muriate of barytes, and obtained a precipitate of sulphate of barytes, which when perfectly dried weighed 19½ grains.

I suspected that the alkalis made use of in the experiments D and E might contain a small portion of sulphate of pot-ash, and by that means have contributed to the precipitate of the sulphate of barytes. To ascertain this fact, I dissolved an equal quantity of alkali; and having supersaturated it with muriatic acid, and treated it with the muriate of barytes, the sulphate of barytes which fell down was carefully collected and dried, and weighed 1½ grain; a quantity to be deducted from the foregoing, and leaves 18 grains of sulphate of barytes to determine the proportion of the sulphate of each; and by computation I find that 200 grains of the lapis contain 8.18 of sulphuric acid, of the specific gravity 1.850, or in combination with calcareous earth 13 grains of sulphate of lime. This calculation is founded upon my experiments, which shew that with the difference of a very small fractional quantity, 100 parts of sulphuric acid of the specific gravity 1.850 form 220 of sulphate of barytes. The same quantity of acid for its saturation with calcareous earth demands either 100 parts of carbonate of lime, or 55 of pure lime, and forms 160 of sulphate of lime.

The 200 grains of lapis lazuli contain therefore as component parts :

					Grains.
Siliceous earth	—	{ D 57 H 29 K 6 }	—	—	92
Calcareous earth	—	{ G 59 I 5 }	64	but ignited	35
Alumine	—	K	—	—	29
Oxide of iron	—	I	—	—	6
Sulphuric acid	—	L	—	—	8
Carbonic acid	—	B	—	—	20
Water	—	A	—	—	4
					<hr/>
					194
				Lofs	6
					<hr/>
					200

But

But because the calcareous earth is combined partly with the sulphuric acid and partly with the carbonic acid, it follows that the combination of the lapis is

				Grains.	
Silex	—	—	—	46	} 100
Alumine	—	—	—	14.50	
Calcareous carbonate	—	—	—	28	
Calcareous sulphate	—	—	—	6.50	
Oxide of iron	—	—	—	3	
Water	—	—	—	2	

The fum is here complete, because I have considered the calcareous earth as perfectly saturated with the carbonic acid; which does not however appear in fact to be the case.

VI.

Useful Notices respecting various Objects.—Rose-Water—Eau de Luce—Soap of Wool—Sea Sicknefs.

1. Rose-Water.

THE simple distilled water from rose-leaves, which is sold by the name of rose-water, has the disadvantage of losing its fragrance, by a spontaneous change which seems to be of the nature of the putrid fermentation. This happens in much less time than must elapse between the annual seasons when fresh rose-leaves are to be had. The article is nevertheless to be purchased at any time of the year; from which circumstance it has been supposed that the manufacturing perfumers were in possession of some method of preventing the process by which it is changed. A philosophical friend assures me that this is not the case, but that they distil only so much rose-water at a time as they know will keep during the period of the regular demand for that quantity; at the end of which they distil the same quantity from other rose-leaves. Their management for insuring a regular supply consists in packing the fresh rose-leaves with common salt in a mass, to a portion of which, when required, they add water, and distil from the mixture.

2. Eau de Luce.

THE same intelligent friend informs me, that the usual recipes in the London Pharmacopœia and other books, for making the fragrant alkaline liquor called eau de luce, the leading perfection of which is, that it shall possess and retain a milky opacity, do not succeed, but that a separation takes place, and the fluid becomes more or less clear by keeping. The use of mastic in this composition has hitherto been kept a secret. Upon his assurance that this is the chief ingredient, I made the following trials. One dram of the rectified oil of amber was dissolved in four ounces of the strongest ardent spirit of the shops; its specific gravity being .840 at 60 degrees of Fahrenheit. This is the oily spirit which is to be added to volatile alkali to form eau de luce, according to Macquer, in his Dictionary, who speaks highly of a recipe to this effect, but with the addition of ten or twelve grains of white soap to the spirit, previous to the oil. The purposes of my experiment did not require the soap. A portion of the clear spirit was poured upon a larger quantity of fine

powdered mastic, than it was judged could be taken up. This was occasionally agitated without heat, by which means the gum resin was for the most part gradually dissolved. One part of the oily solution was poured into a phial, and to this was added one part of the solution of mastic. No opacity or other change appeared. Four parts of strong caustic volatile alkali were then poured in, and immediately shaken. The fluid was of a dense opaque white colour, affording a slight ruddy tinge when the light was seen through a thin portion of it.

In a second mixture four parts of the alkali were added to one of the solution of mastic; it appeared of a less dense and more yellowish white than the former mixture. More of the gum resinous solution was then poured in, but it still appeared less opaque than that mixture. It was ruddy by transmitted light.

The last experiment was repeated with the oily solution instead of that of mastic. The white was much less dense than either of the foregoing compounds, and the requisite opacity was not given by augmenting the dose of the oily solution. No ruddiness nor other remarkable appearance was seen by transmitted light.

These mixtures were left at repose for two days; no separation appeared in either of the compounds containing mastic; the compound consisting of the oily solution and alkali became paler by the separation of a cream at the top.

It appears, therefore, that the first of these three mixtures, subject to variation of the quantity of its ingredients, and the odorant additions which may be made, is a good eau de luce.

Chemical writers speak of a milky fluid, under the name of *lac virginale*, made by pouring tincture of benzoin into some perfumed water. As this fragrant balsam promised to be in some respects superior to mastic, I was induced to try it. A spirituous solution was made of the brown benzoin, which happened to be at hand. To one part of the oily solution first mentioned was added one part of the clear filtered solution of benzoin. These mixed uniformly. Four parts of the caustic volatile alkali were then added. The mixture became opaque, fawn-coloured and curdled, with much less pungency of smell than was afforded by the other alkaline compounds. The next day the opaque part had considerably subsided, and left a brownish transparent though turbid fluid above. It appears probable, therefore, that the acid of the benzoin had not only united with part of the volatile alkali, but that the oily and resinous parts had likewise been disposed to coagulate in this arrangement. It is undoubtedly of no value in the present point of view.

3. Soap of Wool.

I HAVE made a few experiments on the saponaceous combination of wool and alkali*. A caustic lixivium was made by mixing a solution of the crystals of soda with a due portion of lime and water. To the clear ley, in a state of ebullition, wool was added, a little at a time. It was speedily dissolved by stirring; but on account of the lixivium being too strong, the compound appeared thick, and was poured out before, as it afterwards appeared, the saturation of the alkali was effected. The compound, after being left in an earthen vessel for several days, was found to be scarcely more consistent than treacle. Its smell was offensive, though not strong; it readily dissolved in water, but scarcely lathered at all; and

* Philos. Journal, I. 40.

by its action when smeared on the hands it appeared to possess a considerable portion of disengaged alkali. The whole was then dissolved in water, and left for several days on account of other avocations. In the mean time a weaker lixivium was boiled with the addition of wool in small portions, till the last quantity added remained for a considerable time undissolved, and was taken out. The fluid poured into cups had the consistence of treacle. It lathered scarcely at all with water, in which, however, it readily dissolved; and upon being used with a piece of flannel, it seemed admirably adapted for scouring that cloth. Four successive rinsings in different clear waters seemed to have washed off the soap; but the smell still remained very strong in the flannel. It went off in six or seven hours completely.

I was desirous of ascertaining the state of the wool when it should be again separated by means of an acid. For this purpose I added diluted vitriolic acid to the aqueous solution of the first imperfect soap, which contained about half a pound of wool. A separation instantly followed; and, to my surprise, upon stirring the liquor, it lathered very well. The next day the consistent part had settled to the bottom. The clear liquor, which was considerably acid, and had a sulphureous smell, lathered with agitation. The consistent part had the appearance as if mostly composed of broken fibres. The whole was poured into a coarse cloth, and, after straining away the acid, water was suffered to run from a cock into the bag of the cloth, and fell clear into a small basin which it overflowed below. It was remarkable that this clear tasteless water bore a strong head or head by agitation. Theedulcorated matter, when dry, weighed less than half an ounce. Hence it should seem in this loose experiment that the acid had not only saturated the disengaged alkali, but had either combined with the wool of the soap, or formed a triple compound with the soap itself, which was mostly carried off in theedulcoration. The residue was probably wool, which had not been completely deprived of its organization in the first boiling. But I mean to examine the perfect soap with greater attention to quantities.

4. *Sea Sickness.*

IN an account of the islands of Goree and Senegal, by Citizen Prélong, printed in the xviiith volume of the *Annales de Chimie*, the author mentions that he suffered prodigiously for a long time by the sea sickness, but was greatly, and afterwards habitually, relieved by taking ten drops of vitriolic ether in a spoonful of water. I have also been assured by the commander of a packet constantly passing between Harwich and Helvoetsluys, that he always found this distressing illness greatly relieved in his passengers by a small quantity of red wine heated with spices. The sea sickness seems to be a spasmodic affection of the stomach, produced by the alternate pressure and recess of the contents of that viscous against its lower internal surface, accordingly as the rise and fall of the ship opposes or recedes from the action of gravity. Hence it is relieved by change from the erect to the prone posture, or by removing from the extremity of the vessel to the vicinity of axis of the pitching motion, near the mainmast; and hence also, when the organs have become habituated to a regular vibration of one kind in a ship for several months, the sickness may nevertheless be again generated by a different vibration in a boat. As it is a habit which requires some time to be generated, and comes on gradually, it is not difficult to oppose it by mental effort or diversion; but, to such as have not acquired this facility, it may be acceptable to know that

that the above spirituous stimuli have been found of service in counteracting it. The stimulus of food, taken even against the inclination, has also been frequently found to be beneficial.

VII.

A Comparison between Electrical Machines with a Cylinder, and those which produce their Effect by means of a Circular Plate of Glass. With a Description of a Machine of great Simplicity and Power, invented by DR. MARTINUS VAN MARUM.

IT is a remarkable circumstance that the plate-machine for electricity, first invented and published in this country by Dr. Ingenhoufs, has never been much used here, though it has been well received on the Continent, and almost universally preferred to the machine with a cylinder. This may in some measure be owing to the improvements in our manufacture of blown glass, by which we have been supplied with cylinders of considerable dimensions at a moderate price, instead of the globes which were originally used for this purpose. Some years ago (1787) I improved the cylinder-machine by a contrivance for changing the electricity of the conductor from plus to minus almost instantly, which is described in the *Philosophical Transactions* for 1789. From a comparison of the quantities of electricity accumulated by the friction of a square foot of glass, I was induced to adopt the opinion, that cylinders were preferable to plates in every respect, excepting the great quantity of surface afforded by the latter in machines constructed without regard to expence. The labours of the celebrated Dr. Van Marum, together with some observations of my own, have since that time tended to alter my opinion: for which reason I shall, in the first place, enumerate a few general facts, and then proceed to describe his excellent improvement of my contrivance.

I. Electrical machines were formerly made to revolve with considerable velocity by a multiplying wheel. This has since been rejected in consequence of the strong excitation and increased friction produced by a more advantageous application of the amalgam of zinc and mercury. The machines with a single winch demand the same labour as before to work them. They exhibit much more fire in the form of flashes and sparks. But, as far as my experience shews, the spark from the old machine was denser and more pungent, the excitation more steady, and the time employed in charging somewhat shorter.

II. A cylinder with a single winch requires larger terminations of its metallic parts to prevent the fire from flashing out, than are required either in the old machine, or one of those constructed with a flat plate.

III. It frequently happens that the simple machine will be in a state to throw out ramifications to the table, to the face of the operator, and into the air; though the actual spark is not very dense, nor the power great, when examined by the time required to charge a battery or jar.

IV. From these circumstances it appeared probable that the electric matter in a charged conductor may be thrown into a state of undulation by an irregular supply from the cylinder, and that in this state it will fly off more readily than when supplied by a more uniform stream. Thus, when the cylinder is of an irregular figure, the action of the cushion will be stronger

on the one side than the other, and this irregularity may be increased also from other causes. The irregular supply will therefore be made at more distant intervals by the simple machine, at more frequent reiterations in the machine with the multiplying wheel, and perhaps uniformly from a plate.

V. The effect of this undulation may be shewn from various facts. 1. A small wire of many yards in length, communicating from a ball to the ground, will be rendered luminous through its whole length, by sparks of positive electricity given to the ball; but it will not be at all luminous when the same quantity of electricity is given in a more continued stream by placing the ball in contact with the prime conductor. 2. If a metallic rod be insulated with a ball at one end, of a diameter sufficient to prevent the spontaneous flashing of electricity when it forms part of the prime conductor, and at the other end of the stem be fixed another ball of a proper size to draw the spark; when this last ball is placed in contact with the prime conductor, the other ball will not throw out flashes; but if it be withdrawn so as to receive sparks, the external ball, though certainly no more electrified than before, will throw out a flash every time the spark is emitted. 3. A brass ball of four inches diameter was connected by a metallic stem to the end of the prime conductor of an electrical machine in the positive state, sufficiently vigorous to throw a flash into the air now and then. The metallic stem, which was about six inches long, was then changed for another of the same dimensions of deal wood. In this state the ball threw out continual flashes into the air. The experiment was frequently repeated, and the last result may naturally be supposed to depend on the imperfection or discontinuity of the conducting matter in the wooden stem. 4. A pointed wire was inserted in the positive conductor of Nairne's electrical machine, with the point upwards. It was then covered with a clean Florence flask. The point of the wire occupied the centre of the bottle. Whenever the positive spark was drawn from the conductor, the point of the wire exhibited the luminous negative sign. But when the experiment was repeated on the negative conductor and the spark drawn, the point emitted the positive flash so as to fill the whole capacity of the bottle with ramified light. In these experiments it may be presumed, that the escape at the point was occasioned by undulation. 5. The lateral spark in discharging a jar may be urged as an instance of the same kind.

These observations seem to give the advantage in favour of plate-machines as far as relates to the escape. I shall therefore proceed to describe that of Dr. Van Marum*, and afterwards state from his report, compared with my own experiments, what may be the proportions of electricity collected from each square foot, which passes the cushion in machines of both kinds.

Plate III exhibits a perspective view of the machine, and Plate IV a section, exclusive of the cushions. In the view it may be observed that the cushions are each separately insulated upon pillars of glass, and are applied nearly in the direction of the horizontal diameter of the plate, instead of the vertical diameter as heretofore. The ball diametrically opposite to the handle is the prime conductor, and the semicircular piece with two cylindrical ends serves, in the position of the drawing, to receive the electricity from the plate. By the happy contrivance of altering the position of this semicircular branch from vertical to nearly hori-

* Abridged from the *Seconde Continuation des Expériences faites par le Moyen de la Machine Electrique Tyle-rienne*.

zontal, the cylindrical ends may be placed in contact with the cushions, and the prime conductor instantly exhibits negative electricity. But as it is necessary that the cushions should communicate with the ground when the positive power is wanted, and that they should be insulated when the negative power is required, there is another semicircular branch applied to the opposite side of the plate nearly at right angles to the first. That is to say, when positive electricity is wanted, this second branch denoted by I. I. in the section Fig. 1, Pl. IV, is placed nearly horizontal, and forms a communication from the cushions to the ground through a metallic rod from K behind the mahogany pillar which supports the axis; but when on the contrary the negative power is wanted, and the branch from the prime conductor is placed in contact with the cushions, this other branch from the axis is put into the vertical situation, and carries off the electricity emitted from the plate of glass.

The axis of the plate Bh, Fig. 1, Plate IV, is supported by a single column A, which for that purpose is provided with a bearing-piece K, on which two brass collar-pieces DD, represented more at large and in face in Fig. 3, are fixed, and carry the axis itself. The whole of Fig. 1, is reduced to one-eighth of its real dimensions, unless contracted by the shrinking of the paper after printing; to obviate which, it may be remarked that the diameter of the plate is 3½ English inches. The axis has a counterpoise O, of lead, to prevent too great friction in the collar D nearest the handle. The arc of the conductor EE, which carries the two small receiving conductors FF, is fixed to the axis G, which turns in the ball H. On the other side of the plate is seen the other arc II, of brass wire, half an inch in diameter, fixed to the extremity of the bearing-piece K, so that it may be turned in the same manner as the arc EE. The two receiving conductors FF are six inches long, and two and a half inches in diameter. The double line P represents a copper tube terminating in a ball Q. It moves like a radius upon the stem R of the ball S, which being screwed into the conductor H, serves to confine the arm P in any position which may be required. The diameter of the ball S is only two inches, which, together with certain other less rounded parts of this apparatus, may serve to shew that the considerable electricity from this machine is less disposed to escape than if it had proceeded from a cylinder. The dissipation of electricity along the glass supports is prevented by a kind of cap T, of mahogany, which affords an electrical well or cavity underneath, and likewise effectually covers the metallic caps in to which the glass is cemented. The lower extremity of the cap is guarded in the same manner by a hollow piece or ring V, of mahogany, which covers the metallic socket into which the glass is cemented. The three glass pillars are set in sliding-pieces, as marked on the platform of Plate III, which are 9 inches long.

The rubbers of this machine differ in no essential particular from those described by the inventor in the *Journal de Physique* for February 1791; and the apparatus for applying them is described in the same work for April 1789. Fig. 2. represents a section of this judicious piece of mechanism seen from above, and one-fourth of the real size. A metallic sliding-piece bb, is slid into a correspondent face, on the ball Z, which is one of those fixed on the top of the glass pillars near the circumference of the glass plate in Plate III. To this is affixed the piece dd, which terminates in two hinges gg, that allow the springs ee to move in the plane of the horizon. The pieces gg represent the wood-work of the cushions attached to the extremities of the springs by the hinges hh. The springs are regulated by the bolt and screw ii. The two cushions are thus made to apply to the plate equally through
their

their whole length; the actions on the opposite sides of the plate are accurately the same; and the play of the hinges gg, prevents the plate from being endangered by any strain in the direction of its axis. It is certain that, before this adequate provision was made to secure those essential requisites, it was impracticable to apply the cushions to a plate with the same safety and effect as to cylinders, which possess much strength from their figure. An ingenious workman will probably find little difficulty in constructing these rubbers from this description and drawing; but the most precise information respecting every circumstance and dimensions is to be found in the letters above quoted.

The inner extremities of the cushions are defended by the plates of gum-lac YY, which cover the three sides or edges, and prevent their attracting the electric matter from the ends of the receiving conductor.

That part of the axis which moves between the collars is made of steel. The middle of the non-conducting part of the axis is a cylinder of walnut-tree wood aaaa, baked until its insulating power is equal to that of glass, and then soaked in amber varnish, while the wood still remains hot. The two extremities of this cylinder, which are of a less diameter, are forced, by strong blows, with a mallet, into the stout brass caps b and c, in which they are retained by three iron screws dd. The cylinder aa, and the brass caps are covered with a layer of gum-lac eeee, to preserve the insulating state of the wooden cylinder more perfectly, and to prevent the cap b from throwing flashes to the rubbers. The bottom of the cap b is screwed home on the tapped extremity of the steel axis b. The base of the cap c, which is four inches in diameter, terminates in an axis one inch thick, and two in length; the extremity of which is formed into a screw. The glass plate is put on this projecting part, and secured in its place by a nut of box-wood, forced home by a key, applied in the holes ii. Two rings of felt are applied on each side of the glass, to defend its surface from the contact of the wood and the metal; and the central hole in the glass, which is two inches in diameter, contains a ring of box-wood, which prevents its immediate application to the axis.

As it is necessary that the axis G should be parallel to the axis of the plate, in order that the conductors FF may move parallel to the plate itself, the pillar M is rendered adjustable by three bearing screws RR at the bottom, which re-act against the strong central screw T, and this is drawn downwards by its nut. The conductors FF are also adjustable by the sliding-pieces vv, and the binding-screws ww, which also afford an adjustment to bring the axis of each small conductor parallel to the face of the glass plate. A similar adjustment may be observed at the extremities of the arc II.

Fig. 4. represents a section of the moving part of the branch II, one-half of its real size. A brass plate aa is screwed to the face of the capital K by three iron screws β. To this is screwed another ring δδ, which affords a groove for the moveable ring γγ, into which the arms II are fixed. This is accordingly applied in its place before the ring δδ is fixed.

The wooden part of the rubbers GG, Fig. 2, Plate IV, is covered with thin plates of iron, excepting the surface nearest to the glass. The intention of this is to maintain a more perfect communication between the rubbed part of the cushion and the earth or negative conductor, as the case may be.

The plates of gum-lac YY, are applied to the rubbers, each by means of a thin plate of brass, to which they are affixed by heat. There are two wires riveted in these plates, which are thrust into correspondent holes in the wooden part of the cushion.

The mahogany column A ends in a square $\xi\xi$, upon which the piece K is fitted and firmly applied, by means of the screw and nut exhibited in the section.

To ascertain the power of this machine, Dr. Van Marum relates an experiment made before the Directors of the Teylerian Establishment, and other philosophical gentlemen, in circumstances not very favourable to the apparatus; but to which he gives the preference, on account of the respectability of the assistants. A battery of ninety jars, each containing upwards of a square foot of coated glass, was charged to the highest degree by 150 turns of the plate, so that it discharged itself. The great Teylerian machine with two plates of sixty-five inches diameter in its original state, before Dr. Van Marum's improved rubbers were applied to it, never charged the same battery, in the most favourable circumstances, in less than 66 turns. It follows, therefore, that this small and simple machine exhibited $\frac{66}{150}$ ths, or about $\frac{2}{5}$ ths of the power of that great machine in its first state; and probably, if the circumstances had been alike favourable in each, it would have amounted to one half. The Doctor has grounded a calculation upon these facts; but as he states the rubbed surfaces of these two machines, probably by some mistake in calculation, to be 1243 and 9636 square inches respectively, I shall repeat the calculation in this place.

The diameter of the plate is 31 inches, and the length of the cushion 9 inches. Then $31 \cdot 7854 - 31 - 18 \cdot 7854 = 522$ square inches rubbed by one cushion on one side. And $522 \times 4 = 2088$ square inches rubbed by the four cushions. Again in the great machine, the two plates having a diameter of 65 inches, and eight cushions of $15\frac{1}{2}$ inches long, $65 \cdot 7854 - 65 - 31 \cdot 7854 = 2410.4$. And $2410.4 \times 8 = 19283$ square inches rubbed. But the intensity of the electric power of a machine will be in the compound ratio inversely of the surfaces and number of turns when the charge is the same; Or $150 \times 2088 : 66 \times 19283 :: 1 = \text{the intensity of the larger machine} : 4 = \text{the intensity of the smaller}$.

To have increased the power of steady excitation four-fold, is certainly an astonishing acquisition. This expression, however, of the intensities appears to be less generally useful than that of the ratio of the surface rubbed, to that which is charged. This last expression becomes very simple when the latter quantity is reduced to 1, or unity. Thus, in the two

machines here mentioned, the rubbed surfaces in inches for the battery are $\frac{19283 \times 66}{90 \times 144}$

and $\frac{2088 \times 150}{90 \times 144}$, which are equal to the simple numbers 60.5 and 24.0, which respectively denote the number of inches rubbed to charge one inch of coated glass.

The great machine charged a single foot of glass, by rubbing 66.6 feet; and a battery of 224 feet at the rate of 94.8 feet rubbed per foot. If the gradual decay of excitation be supposed the same in the small machine here described, it must have commenced with an intensity of 17.6. In the Philosophical Transactions, already quoted, I have stated the commencing intensity of a cylinder, excited by the amalgam of zinc, as tried by a jar of $2\frac{1}{2}$ feet to be 18.03 and 19.34. But, from my notes, I find that this jar was charged with less than 15.0 when the hand was constantly pressed against the silk-flap; and also, that this pressure increased the intensity as 49 to 39 in some few trials, not enough varied and repeated. The labour of turning was very great; much more than, from various circumstances, I am inclined to suppose Mr. Van Marum's method requires. From this consideration, as well as from the numbers, and the probability that, on account of the less undulation, the charges by a plate may be higher before they explode than those by a cylinder, and likewise from the

the large surface exposed to friction, I conclude, that the machine described in this paper is at least equal in steady intensity, and much superior in power of charging, to any cylinder-machine which has ever been made.

VIII.

The Process for giving a beautiful White Colour to Raw Silk without Scouring. By M. BAUME.*

[Concluded from page 32.]

TO complete the description of M. Baumé's process for bleaching silk, nothing more remains, than to shew in what manner he recovers the ardent spirit, and ensures the purity of the acids made use of. These circumstances are of essential importance to the art: for the process would be much too expensive if the spirit were lost, and it could not be made to succeed at all if the acid were impure.

The alcohol which has been used in bleaching silk, is acid, and loaded with colouring matter. In this state it cannot be again used. There are two methods of distilling it; which have their respective advantages and inconveniences. By the first, the acid is lost; which is saturated with pot-ash, in order that the distillation may be afterwards performed in a copper alembic. The second is performed by distilling with glass retorts, or an alembic of silver. In either of these vessels, which are not acted upon by the marine acid, the distillation may be performed, and the greater part of the acid recovered. The inventor most generally practised the saturation of the acid from reasons of convenience; but recommends the use of a silver alembic, as being most economical upon the whole, in a manufactory.

A solution of pot-ash is to be poured into the acid spirit and stirred about to promote the saturation. Carbonic acid is disengaged with strong effervescence from the alkali, and the point of saturation is known by the usual test, that the fluid does not redden the tincture of turnsol. The distillation is then to be made in the copper alembic, and the alcohol reserved in proper vessels, as mentioned at the beginning of this Memoir.

If too much alkali should have been added, the liquor remaining in the alembic may be used in another saturation. The alkali in this process being an expensive article, Mr. Baumé endeavoured to supply its place by chalk, quick-lime, and lime which had been flaked by exposure to the air. But he found that the action of the spirit upon the calcareous earth, or perhaps the absence of water, prevented the acid from uniting with that substance. The union does not take place to perfect saturation in less than five or six weeks, even when the alcohol is diluted with upwards of fifty times its bulk of water.

In the second process for distilling without alkali, the acid spirit is distributed into a great number of glass retorts, placed in the sand-bath, on the gallery of a furnace. The first product is scarcely acid; but what follows is more and more so, and must be kept in vessels of glass or stone ware, which become embarrassing on account of their number. The fluid which remains in the retorts has the colour of beer slightly turbid, and contains the greatest part of the marine acid. It must be poured into one or more retorts, and concentrated by

* The Editors of the *Journal de Physique*, to which reference was made at the beginning of this Abstract, omitted to mention how they obtained it. I find in the *Annales de Chimie*, XVII. 156, that it was read at the Public Meeting of the Academy at Paris, April 10, 1793.

heat gradually applied. The first liquor which comes over is slightly red, turbid, and scarcely acid. This is to be thrown away, and the receivers changed. The succeeding product is the colourless marine acid, of an aromatic smell resembling the buds of poplar. The resin of the silk remains in the retort decomposed by the acid. The marine acid thus obtained is weaker than it originally was; which is in fact of little consequence, as it is pure, and may be safely used, either by increasing the dose proportional to its diminished strength, or by concentrating it, if required, in the usual way.

If this distillation be made in a silver alembic, instead of retorts of glass, and a capital and worm of pure tin be annexed, the alcohol will be obtained so slightly acid as scarcely to redden the tincture of turnsol; but it is sufficiently acid to receive injury if preserved in a copper vessel.

If a cucurbit of silver be prepared, of the capacity of three or four quarts, with a glass head, the residues of the first distillation may be treated in this vessel in the same manner as has been directed for glass retorts. M. Baumé affirms that he has practised all these operations with glass retorts and a small silver alembic, with the most perfect success; but that he made use of pot-ash to saturate the marine acid, because he had not a silver vessel of sufficient capacity. From the danger of distilling large quantities of ardent spirit in glass vessels, he is of opinion that no motives of economy are sufficient to justify the risk attending this method. In the use of tin, it is necessary to be careful that it contains no adulteration of lead, because the vapours of marine acid have sufficient power to alter this last metal very considerably.

Upon the first intimation of this new process in France, manufactories were immediately established, to the number of twenty or more, without the concurrence of Mr. Baumé, by persons who consequently were not aware of the apparently minute but very important circumstances necessary to insure its success. In particular, the inventor states that the marine acid of commerce is unfit for this purpose.

This acid was formerly prepared with the marine salt of the saltpetre manufacturers; and even when it is made with good salt, the decomposition is effected with common vitriolic acid which contains nitrous acid. Marine acid mixed with a small quantity of nitrous acid does not prevent the silk from being beautifully whitened: it even accelerates the process considerably, and in the most satisfactory manner. But the alcohol, every time it is used and rectified, becomes charged with the acid and gas of nitre, which assume the characters of the nitrous anodyne liquor. In this state, neither distillations nor repeated rectifications from alkali are sufficient to separate the nitrous matter from the alcohol. Then it is that the success of the operator vanishes, with a degree of rapidity equal to the advances which encouraged his hopes at the commencement. The same disappointments beset M. Baumé at the beginning of his labours; to prevent which, he directs the preparation of the vitriolic and marine acids to the following effect.

The vitriolic acid of commerce is obtained by burning sulphur in chambers of lead, with the addition of saltpetre, either crude or of the second crystallization, and a small portion of flax. This acid is concentrated and rectified in France, at the place of its fabrication, to 66 degrees of Baumé's hydrometer, or specific gravity in the usual form 1.848. It contains sulphur, lead, vitriolated tartar, Glauber's salt, alum, selenite, and particularly the nitrous and marine acid.

To purify it, one hundred pounds of this vitriolic acid is to be mixed in a large bason of copper with the same quantity of river water, and stirred with a wooden spatula. The mixture instantly becomes heated to the boiling-water point, and a great quantity of red vapour is disengaged, which has the smell of aqua-regia, and arises from the nitrous and marine acids. When this mixture is made, it is proper to immerse the bason to a suitable depth in a large vessel of water, to hasten the cooling. As soon as it is sufficiently cooled it is to be drawn off into bottles, and left to become clear during several days. Great part of the sulphur falls down. The author obtained from four to six drams.

A gallery must be provided, on which two rows of iron pots of eleven or twelve inches in diameter are to be properly placed for separate sand-baths, as M. Baumé always practised in the sublimation of sal-ammoniac. By this means the retorts are isolated, and if one breaks, the acid cannot diffuse itself and break the others in its vicinity. An empty retort is then to be placed in each pot, and covered with sand. In this way they are much more convenient to arrange, and are attended with no risk.

The acid is in the next place to be decanted and conveyed into the retorts by a syphon funnel, and the rectification 'proceeded upon until it becomes perfectly white. Towards the end of the operation a small quantity of sulphur sublimes in the neck of the retort. Instead of receivers a small glass cup is placed beneath the aperture of each retort, in order to facilitate the dissipation of the nitrous and marine acids.

When the acid in the retorts is sufficiently cooled, it is poured a second time into the copper bason, and mixed with 100 pounds of river water, as at first, and again concentrated in the retorts till it becomes perfectly clear. Sulphur has been afforded in some instances by the second rectification. The liquor which distills is received in the cups as before, and the acid in the retorts is then sufficiently pure: that is to say, it is purified from all volatile matter. The lead and neutral salts still remain combined with the acid, but fortunately they can in no respect injure the purity of the marine acid.

This concentrated acid exhibits 68 degrees by the hydrometer, or specific gravity 1.836. It still contains a portion of gas, but so small in quantity as not to injure the purity of the marine acid, to which it only gives the property of crystallising when the temperature of the air is near the freezing point.

During the rectification of this acid, what first comes over is mere water, and must be thrown away; but that which succeeds is the aqueous acid. If this be set apart, and concentrated, a considerable quantity of vitriolic acid is obtained of the greatest purity. As it has been carried over in distillation, it contains no foreign matter.

The author attempted, but in vain, to dissipate the nitrous acid from the acid of vitriol by ebullition in an open vessel without concentration. The experiment was made with 50 pounds of common vitriolic acid and 60 of river water. This was kept boiling in the copper bason for four days, water being added from time to time to supply the loss by evaporation. The copper bason, by weighing before and after the operation, had lost by solution no more than ten drams of copper. The acid was blue, but became white as usual during the rectification in the retorts. From this experiment, as the author observes, it is seen not only that the nitrous acid cannot be dissipated by simple ebullition without concentration, but that the action of the vitriolic acid upon copper is extremely slight.

The marine acid is to be disengaged from common salt by the application of this vitriolic acid

acid in the usual manner. But as M. Baumé's experience led him to various simple manipulations and remarks of importance, and more especially as he considers the description of this process as part of the new art of bleaching silk, he has annexed it to his memoir.

The vitriolic acid obtained by the foregoing process being too concentrated, must be diluted in the copper basin as before with river water. It is convenient to add 18 ounces of water to each pound of the acid, because the marine acid is not wanted in a state of high concentration. This mixture ought to give 35 or 36 degrees by Baumé's hydrometer; which last answers to a specific gravity of 1.333. When it is cold it may be preserved in bottles for use.

In the next place, four pounds of marine salt dried, because in that state it pours best, is to be put into a retort of the capacity of five or six French pints, or English quarts. This may be done by means of a paper funnel, or a long-necked funnel of glass, which must enter the body of the retort in order that the neck may remain clean. A number of these must be disposed on a gallery in two opposite rows, with the necks properly enclosed and enveloped in sand as usual.

A bottle or gauge being provided of such a size as by previous experiment is known to hold four pounds of the vitriolic acid before mentioned; this quantity of the acid must be measured into each of the retorts by means of a curved funnel, the tube of which may pass into the body, to prevent the acid being spilled in the neck. If nevertheless a few drops should fall, no inconvenience will follow, as this pure acid is not detrimental to the bleaching process.

The supports for the receivers are then to be placed, and the receivers applied, each being pierced with a small hole. The junctures are to be made good with pasted paper, and the distillation begun. A gradual heat is to be applied until the fluid boils gently. The marine acid which first rises is volatile and expandible*, and requires the small holes of the receiver to be occasionally opened; but after one fourth part of the time of distillation the acid comes over freely, and the vapours cease to be elastic.

This distillation lasts two days; but it is practicable to avoid sitting up the intermediate night. The fire must be so managed that the contents of the retort may be very liquid in the evening: if it begins to thicken, there is reason to apprehend that it may be too hard the next day; in which case the heat will dilate the concrete matter before it liquefies, and break the containing vessel.

Towards the close of the distillation the matter swells up considerably. When this happens, it is proper to empty the receivers, and raise the retorts, that more sand may flow in beneath them. When the matter is dry, and nothing more comes over, the operation is finished.

Each retort affords five pounds of marine acid, of the strength of 14 or 15 degrees; specific gravity 1.114. When the retorts are half cooled, one pound of hot river water is to be poured into each, and the distillation being resumed affords 24 ounces of the same marine acid from each retort.

It is remarkable, that in this process some of the retorts afford the colourless and some the yellow acid; which is an object of no consequence with regard to the bleaching. The author thinks the yellow colour is owing to a portion of sulphur still remaining in the

* It might be of advantage, even in the large way, to adapt a simple pneumatic apparatus to condense the marine acid air in water, as is usual in philosophical processes. N.

vitriolic acid. And if from curiosity the products of several of the retorts be received in eight different parts, it will be seen that in some the acid which passes first is the most concentrated, and gives 20 degrees by the hydrometer; that the products diminish successively in the progress of concentration, till the last exhibits sometimes no more than eight degrees; but that others afford the most concentrated acid at the beginning and end, while that in the middle of the distillation is the weakest. All these products mixed together afford a mean result of 14, 15, or 16 degrees*.

The hard compact saline matter in the retort consists of much Glauber's salt, and a small quantity of undecomposed common salt. M. Baumé's method of extracting it is simple and ingenious. He fills the retort with water, corks it, and inverts it in an open vessel also containing water, over which is fixed a board with holes for receiving the neck of the retort. It is proper that the saline mass should be detached, which soon happens in the filled retort, and suffered to slide down towards the neck before it is placed in the hole of the board. The cork is then taken out, and by that means the water in the retort communicates with that of the open vessel. As the salt dissolves, the brine flows down, and is replaced by purer water from below, which from its less density rises to the uppermost place in the retort. In this way the evacuation is made in two days without trouble, which could not safely be effected in eight or ten days by successive filling and emptying the retorts.

M. Baumé concludes his memoir by describing the method of giving a bright yellow to silk, whether raw or bleached. For this purpose ten gros or drams of nitrous acid are to be mixed with one pound of alcohol, and into this a few ounces of silk are to be immersed, and kept on the water bath at between 30 and 40 degrees of Reaumur, or 100° and 130° of Fahrenheit. The silk acquires a tarnished brown colour, and must be cleared of its acid by washing in several waters, and afterwards scoured with soap in the usual manner. When thus cleaned and dry, it has the appearance of gold threads when seen in the sun's light. Different shades may be given by keeping it a shorter time in the acidulated spirit; all which are equally permanent, and resist washing and every other test. The author proposes them to be used in articles of furniture wrought in designs which require light and shade.

MATHEMATICAL CORRESPONDENCE.

IN order to accommodate such mathematical correspondents as may reside at a distance from the metropolis, and are disposed to contribute to this part of the work, the solutions to the questions proposed in any number of the Journal will be uniformly given the second month after their publication; but it is requested that they may be sent as early as possible, that proper time may be allowed to prepare them for insertion.

QUESTIONS proposed for Solution.

QUESTION III. By ANALYTICUS.

IT is required to determine the odds against the dealer, at the game of whist, having all the thirteen trumps in his own hand.

* For the correspondent specific gravities see p. 39 of this Journal.

QUESTION IV. *By J. B.*

GIVEN the time in which mercury is raised to the boiling point by the heat of a furnace, and the rate of cooling per minute, after it has been removed, to determine the heat of the furnace, or that to which the mercury has been exposed.

SCIENTIFIC NEWS.

THE particular memoirs contained in the four first cahiers of the Journal of the Polytechnic School at Paris are thirteen in number*: two on stereotomy; one on fortification; six on chemistry; three on general physics; and one on the application to the arts.

The memoirs of stereotomy treat: 1. On the determination of tints in designs, by several pupils of the school; and 2. On the curve lines of the surface of the ellipsoid, by Monge.

The solution of the interesting problem of the determination of tints is deduced from the lessons which Monge has given upon perspective. This solution depends on the fundamental principle, that all bodies reflect the white rays; and that the quantity of rays of this colour, reflected from each point of a body, depends on its polish, and the angles formed by the incident and reflected rays which come to the eye; that the more white rays are reflected, the more luminous the body will appear; and on the contrary, the less of these rays are reflected, the more obscure it will be.

All polished bodies present a white spot or line; the line is visible upon cylinders, cones, &c.; the point, upon surfaces of double curvature.

When bodies are perfectly polished, a white point or determinate surface is perceived; but when they are obscure, a successive degradation of tints is seen, which depends on the form of the body.

From the point, the line, or the most enlightened surface, it is possible to trace a succession of curves of equal tint: these are the curves of which the authors of this memoir have in the first place determined the equation, and afterwards sought to ascertain the law of the degradation of the tints.

If it be assumed that any tint laid on white paper will possess an intensity dependent on the quantity of colour spread, and the proportion of white points left uncovered, the authors of the memoir have sought to determine whether the law of the application of tints upon each other ought to depend on their degradation: by this means they have succeeded in giving the theory of washed tints, properly so called.

The solution of this problem, so interesting to those who cultivate the art of design, and are capable of pursuing the calculation it demands, proves the extended knowledge to which the pupils have arrived, and the progress they daily make, under their Instructor.

The memoir on the curve lines of the surface of the ellipsoid, which Monge has given in the second cahier of the Journal Polytechnique, contains an application of the property of curve surfaces with regard to their curvature, to the configuration of stones for arches.

The joints of arch-work ought to be made agreeable to a number of conditions; the chief of which are: "1. They must be everywhere perpendicular to the arch,

* This Analysis is translated from the Journal des Savans, p. 127.

in order that the angles of two contiguous key-stones being respectively equal, they may alike resist disunion by the action they exercise upon each other. 2. They must be perpendicular between the stones, for the same reason. 3. They must be generated by the motion of a right line; for the surfaces generated in this manner are alone susceptible of being accurately wrought; and it is necessary that the joints of the contiguous stones should be perfectly well executed, because very slight irregularities would produce a rupture of the arch."

All these conditions may be obtained by dividing the curved surfaces of a vault by lines drawn from the one to the other of the two curves; and formed by the movement of a point of the surface which meets a series of normals placed in the same direction. The distance between these lines must be a finite quantity, dependent on the nature of the materials.

"The operations of artists having been constantly directed to this general solution, they have obtained it only for the most easy cases of cylindric and conical surfaces, and such as are generated by the revolution of a plane. But with regard to curved surfaces of which they know not the lines of curvature, they almost generally excluded them from the construction of arches, even when circumstances urgently demanded them; and it is principally to this that we are to attribute the bad effect generally produced in architecture by leaning arches, or portions of arch-work (*les morceaux de trait de coupe des pierres*), because, in order to render this practicable, a surface of curvature is chosen for the arch which is not always such as the nature of things demands."

The example chosen by Monge for the application of his principles, is apparently the most happy he could have selected in the present situation of the French government, in which architects are busied in the construction of halls destined for the sittings of the Legislative Council. Now the form most favourable to the distribution of the members of a deliberative assembly, and the situation of the orator, is the elliptic curve commonly called an oval. From the projection of these curves, traced on the designs at the end of this memoir, it may be seen what agreeable and elegant forms are thus produced; and how eminently the architect may avail himself of the principles here established, even for the objects of decoration. Students in architecture will find this memoir highly deserving of their perusal. It will more and more convince them of the immense resources afforded by the study of stereotomy for the solidity of construction, and even for beauty of new forms, afforded by this theory to the system of ornament.

The art of securing those who defend a rampart from the stroke of balls and shot is known in French by the term *défilement*. Say, assistant professor in the Polytechnic School, has given a memoir on this subject in the fourth cahier of the Journal of that institution.

He shews in the first place, that the outline of the *défilement* is the same as that of a shadow afforded by a row of luminous points situated in the space from which the defenders of a fortification may be fired on.

He divides his ground into two parts: the exterior space in which the attacking party may be placed, and the interior space which is to be defended.

He examines the art of securing a work (*de filer*) in two different cases: 1. That in which the parapet is determined. 2. That in which the ground plan is given, but not absolutely

olutely the relief or profile. To these two cases he adds certain rules for tracing fortifications, deduced singly from considerations relative to their desilement.

These three questions are preceded by some preliminary notions; and by the method in which Say has treated his subject, we have acquired in this memoir a more complete treatise than any which has yet appeared on desilements. He has the merit of having fixed with clearness and precision, a series of principles which hitherto have been only transmitted in a fugitive and as it were traditional manner, in the School de Mézières, appropriated to the instruction of the *élèves du génie*, and destined again to receive them, according to the message long ago sent from the Government to the Legislative Body.

The six memoirs of chemistry are: 1. Description and use of an eudiometer of sulphate of pot-ash (liver of sulphur), by Guyton. 2. Observations on the eudiometric properties of phosphorus, by Berthollet. 3. Analysis of the calcedony of Creusot, by Guyton. 4. Experiments on the fusibility of earths, and their habitudes with saline fluxes, together with the solvent action they exert on each other, by Guyton. 5. Experiments on the formation of the colouring prussic principle, by Bonjour. 6. The properties of the sulphureous acid, and its combination with earths and alkaline bases, by Fourcroy and Vauquelin.

[To be continued.]

NEW PUBLICATIONS.

Travels in Hungary, with a short Account of Vienna in the Year 1793. By Robert Townson, LL. D. F. R. S. Edin. &c. &c. Illustrated with a map and sixteen other copper-plates. 4to. 506 pages including the Index. Printed for Robinfon. Price 1*l.* 1*s.*

To those who attend chiefly to diplomatical politics, or the balance of power, the kingdom of Hungary will probably afford an object of subordinate value; but on the larger, more extended and important considerations of internal government, political economy, and the state of man with regard to science and manners in the progress of civilization, it will prove highly interesting. Mr. Townson has published the present work from the corrected notes of a five-months' tour; in which these and other objects of utility and entertainment have engaged his attention. As I hope shortly to give a fuller account of this work, the present notice is intended only to announce the publication of a valuable and entertaining book.

The *Histoire Naturelle* of Valmont de Bomare, rangée par ordre de matieres par L. Blondelin of the University of Bâle, ornamented with coloured plates engraved by J. J. de Méchel, was announced for publication in the middle of February last, in the *Decade Philosophique*, &c. It will be printed at Bâle; and the Quadrupeds were then ready for the press. It will amount to about 150 sheets, or 6 or 7 volumes in octavo, with at least the same number of plates. One number, containing seven sheets and eight plates, will appear every second decade, or one volume of three numbers every two months. The price four livres per number, with the figures plain; or six, if coloured. The subscribers to the first 500 copies will have the advantage of a deduction of one-fourth. The plates may be had at the price of two livres, plain; or six, coloured.

Subscribers pay in advance for one number or volume; and the rest, on delivery, to Cit.

Fuchs, bookseller, rue des Mathurins, hotel de Clany, à Paris; and at Bâle, by Cit. L. Blondéu (perhaps Blondelin as above), agrégé à l'Université.

Refutation de la Théorie Pneumatique, &c. A Refutation of the Pneumatic Theory, or the New Doctrine of the Modern Chemists, presented Article by Article, in a Series of Replies to the Principles collected and published by Cit. Fourcroy in his *Philosophie Chimique*: to which is prefixed, a Complementary Supplement to the Theory exhibited in a Work entitled *Recherches sur les Causes des Principaux Faits Physiques*; or, Researches into the Causes of the Principal Physical Facts, to which this forms a necessary Continuation. By J. B. Lamarck, of the National Institute of France. Octavo, 481 pages. Published at Paris, by the Author, at the Museum of Natural History; and by Agasse, rue des Poitevins. L'an 4.

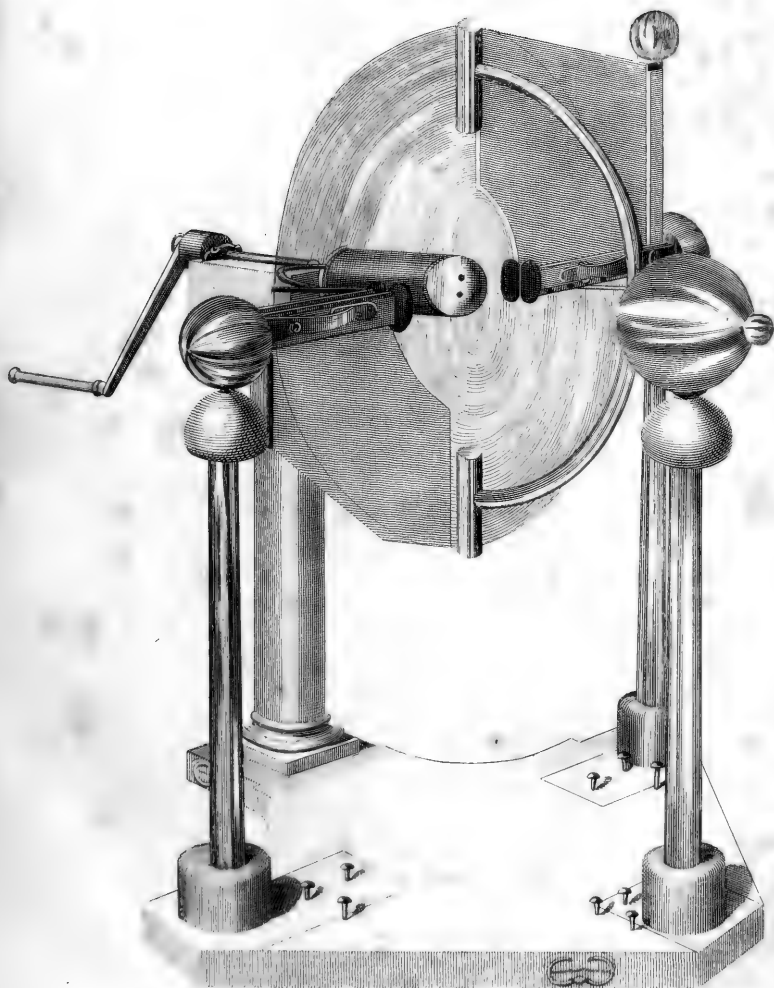
I have not yet procured the *Recherches* here mentioned; but, when in possession of both, it will become an object of careful enquiry, whether an abstract of the leading points of difference in this author's theory and that which he attempts to refute may be of value to the scientific world.

Specimens of British Minerals selected from the Cabinet of Philip Rashleigh, of Menabilly, in the county of Cornwall, Esq. M. P. F. R. S. and F. A. S. with general descriptions of each article: 4to—56 pages—33 plates coloured. Nicol and White. Price 2l. 12s. 6d. This magnificent work displays with great effect the specimens, chiefly consisting of tin, copper and lead ores, with calcareous spars and quartz. The descriptions are intended to complete the knowledge, in part and very strikingly, conveyed by the plates. Little is said in general concerning analysis, or the component parts.

Cit. Prony, of the French National Institute, has published the second part of the *Nouvelle Architecture Hydraulique*. This part contains the description of steam engines. It is sold by Firmin Didot, No. 116, rue de Thionville. Quarto, 240 pages, with 40 plates. Price 40 liv.

M. Lalande, in the *Journal des Sçavans* page 125, gives the following account of this work: "Since the appearance of the first part of the *Architecture Hydraulique* in 1790, the public has waited with impatience for the continuation. This part is a complete Treatise on Steam Engines. It contains new experiments on the expansive power of heat; an account of pumps which have the piston acted on by the steam alternately on both sides (*pompes à double effet*), and those which are simple; instructions for disposing the latter to act in the manner of the former; the theory of the right-lined motion of the piston by a combination of circular movements; and a method of interpolation applicable to phenomena which depend on elastic fluids. The analytic part of this work is no less curious than the mechanical and experimental parts. No one but a geometer could have treated these subjects in a manner so perspicuous for the learned; nor could a less able engineer have rendered them so satisfactory to practical men."

Citizen Garnier, Professor of Mathematics, has added several useful explanations of parts of the first volume of Citizen Prony, which render this work still more complete.





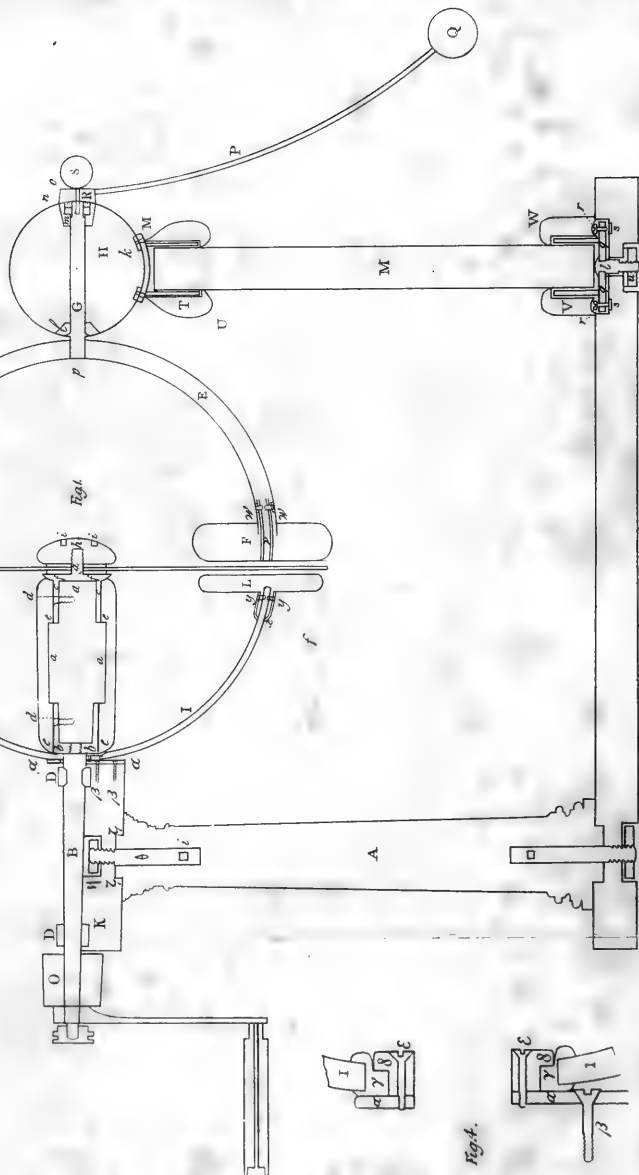
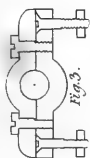
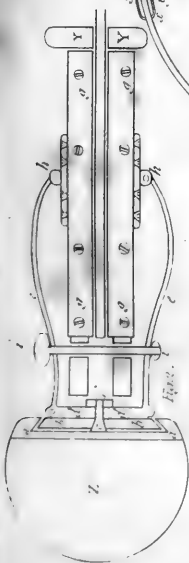
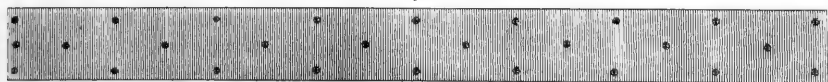


Fig. 4.



Fig. 1.



Flexure of metallic bars by heat.

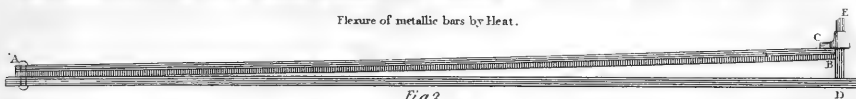
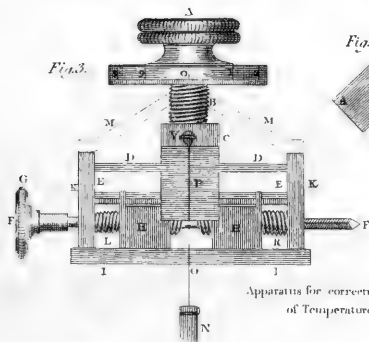


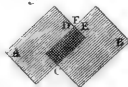
Fig. 2.

Fig. 3.



Apparatus for correcting the effects of change of Temperature in Pendulums.

Fig. 4.



Expansion Balance.

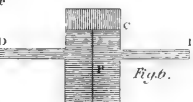
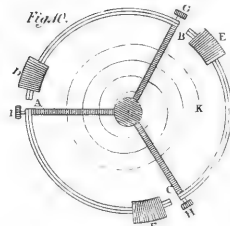


Fig. 6.



Fig. 7.

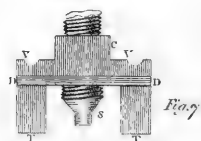


Fig. 8.

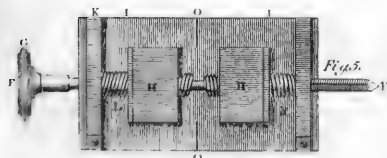


Fig. 9.

Compound bar



Fig. 10.



Fig. 11.



A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

JUNE 1797.

ARTICLE I.

A Letter from M. de HUMBOLDT to M. PICTET, on the Magnetic Polarity of a Mountain of Serpentine.*

AT the beginning of the eighteenth century the attention of natural philosophers was entirely fixed on the phenomena of magnetism. The progress which has since been made in the theory of electricity, and the preponderance which chemistry has acquired over all the other branches of natural history, have diminished the interest with which enquiries into the nature of the magnetic fluid ought to have been pursued. It is true that your celebrated countrymen Messrs. de Saussure and Prevôt have given vigour to this pursuit by discoveries worthy of their sagacity: the first, by inventing an instrument capable of measuring the comparative intensity of the magnetic forces in different regions of the globe; and the other, by reducing the laws of polarity to the simple laws of attraction. But these discoveries have not afforded inducement sufficient to lead philosophers into a path so honourably explored. The most valuable work on the origin of magnetic forces has been neglected, together with the calculations of the ingenious Coulomb, and his experiments with the balance of Torfion.

Having traversed with the compass in my hand great part of the mountains of Europe, I became convinced that declinations caused by masses of iron in beds or in veins are infinitely less frequent than naturalists affirm. The observations which Messrs. de Saussure and Trembley have made on the summit of Cramont†, appear to me the more curious, as

* I received this communication in manuscript from the Right Hon. Sir Joseph Banks, Bart. P. R. S. &c. It is written in French, under the title of "Lettre quatrième de M. de Humboldt à M. Picet, sur la Polarité Magnétique d'une Montagne de Serpentine." The same liberal promoter of science has favoured me with a specimen of this rock, with permission to make experiments upon it. A few observations on this specimen are added at the end of this memoir. N.

† Voyage dans les Alpes, T. I. p. 375.—T. II. p. 343.

it stands alone, and presents to our knowledge a very extended image of the dimensions of magnetic spheres. It is among the Alps of Sweden and Norway, those northern regions which nature has enriched with an enormous deposit of iron less oxidized than in our country, that we were entitled to expect similar phenomena.

I hasten to communicate to you a discovery I made in the month of November, and which appears to me of considerable importance in the progress of geology. You are acquainted, Sir, with the laws and the harmony which I have observed in the direction and inclination of the primitive strata, from the banks of the Mediterranean to those of the Baltic Sea. You have even condescended, jointly with our friend Dolomieu, to express an interest with regard to this laborious undertaking; which, in more skilful hands than mine, would, I am well assured, throw great light on the construction of the globe. I traversed the chain of mountains of the High Palatinate and the margraviate of Bayreuth; and I found, in the bottom of the Fichtelgebirge, between Munichberg and Goldcronach, an isolated hill, which rises to the elevation of fifty toises above the surrounding plain. Its height above the level of the sea may be estimated at two hundred and eighty, or three hundred, toises. This hill extends in length from west to east, and forms a pyramid extremely obtuse. The rocks which crown the summit or ridge are composed of serpentine of considerable purity, which, by its colour and foliated fracture, approaches in various parts to the chloritischiefer of Werner (*schistsus chlorite*). This serpentine is divided into strata rather distinct, of which the inclination to the north-west presents an angle between 60 and 65 degrees. It reposes on a foliated granite, mixed with hornblende; a mixture which we distinguish by the name of syenite. I approached this serpentine with the compass, in order to determine more accurately the angle it formed with the meridian. The magnetic needle was in a state of continual agitation. I advanced two steps farther, and beheld that the north pole was entirely turned to the south. I called two friends, Messrs. Godeking and Kilinger, who assisted me in my geological pursuits; and we were alike penetrated with that joy which the contemplation of interesting phenomena produces in the minds of thinking men. I shall not detain your attention by a full recital of our observations; but shall merely present the results, to which I may hereafter make additions, if my occupations should not lead me from this part of Germany.

The action of this mountain of serpentine upon the magnet shews itself in a very curious manner. The uncovered rocks which are seen on the northern slope, and those on the declivity towards the south, have poles directly opposite. The former exhibit only south poles, and the latter north poles. The whole mass of foliated serpentine does not therefore possess a single magnetical axis, but presents an infinity of different axes perfectly parallel to each other. This parallelism also agrees with the magnetic axis of the globe, though the poles of the serpentine are inverted; so that the northern pole of the hill is opposed to the south pole of the earth. The east and western slopes present what in the theory of magnetism would be called points of indifference. The magnet does not at this part appear to be in any respect affected, though the substance of the rock differs in no external character from the other parts. It is the same on the south side of the summit*. I have observed not only that the magnetic axes are not disposed in the same horizontal plane; but I

* I suspect an error of the copyist, as the words "Il en est de même du côté meridional de la sommité" contradict the general description immediately preceding. N.

have likewise remarked, that two points, of which the action is very strong, are joined by rocks which do not exert the least attraction. The chemical analysis of these compounds affords the same results; and it would be no less difficult to discover any difference of aggregation between them, than between iron which has received the touch, and other iron which had never acquired the magnetic power.

On this occasion a question presents itself which cannot be resolved in less than half a century. The tables founded on the observations of Picard, La Hire, Maraldi, Cassini, and Le Monnier, shew that the needle has declined since 1660 towards the west; and that this declination continues to increase, though the oscillations caused by the heats of the south, and the temperature of the seasons, often produce a retrograde course. If the magnetic axis of our mountain were astronomically determined by the culmination of the stars, whether its direction would remain the same until the year 1850, or whether its south-pole would turn towards the west, in connection with the variation of the magnetic needle? From our profound ignorance of the causes of the declination, as well as of most geological phenomena, it is not in our power to resolve so complicated a problem.

Other observations equally interesting may be made on the identity of magnetic forces. I have discovered a mass of rocks which affect the needle at the distance of twenty-two feet. With an apparatus similar to the magnetometer of M. de Saussure, we might observe whether the intensity of the forces of magnetic action remains the same in winter and in summer; whether it be stronger in the morning, at noon, at the solstices, during the aurora borealis, or in an atmosphere loaded with electric fluid? I suppose that these same rocks might act on the needle sometimes at 16, and sometimes at 28 feet distance.

It has been observed, that metals exposed to the air gradually imbibe the magnetic fluid. A slight oxidation of the iron seems to favour this effect. I have myself observed, that in a magnetic bed of iron those parts only which were in contact with the air affected the needle. This phenomenon is considered as the effect of atmospheric electricity. I am aware that lightning converts a bar of iron into a magnet; that the discharge of the Leyden vial sometimes increases the intensity of magnetic forces; but I do not see why the atmospheric electricity should act simply on the external surface of a bed of magnetic iron, which is a good conductor of the electric fluid. Does not the oxygen of the atmosphere rather act a part in this operation? Without wandering in the sphere of probabilities, I have chosen to adhere to enquiries respecting facts. I have observed the rocks which were covered with turf, from which I detached pieces that had not been in contact with the air. I found that the magnetic force was constantly the same.

The mountains of the Harz present a granite rock called the *schnarcher*, which is elevated in the form of a tower, or broken pyramid. This granite likewise affects the needle; but it acts only in the mass, and in a single band or perpendicular vein. Detached pieces shew no action upon the needle. It is to Mr. de Trebra, celebrated for his researches concerning the internal parts of mountains, that we are indebted for this important discovery.

Some philosophers pretend that the *schnarcher* contain in their bowels a mass of magnetic iron; others presume that a stroke of lightning has caused the magnetic vein in these mountains.

The nature of the rocks which I have the honour to present to your notice in this paper does not admit of similar explanations. The serpentine not only acts in a mass, in its natural

tural situation, but all the pieces, when broken, to infinity, still exhibit two very distinct poles. Pieces of five inches diameter act on the needle at the distance of half a foot. The examination of the magnetic axes affords an object of curious enquiry. They are mostly found in a direction parallel to that of the foliated grain; nevertheless, I have found some which cross it perpendicularly. Fragments extremely small, of the magnitude of 0.01 of a cubic line, shew a very strong polarity in proportion to their masses. You see them turn very suddenly when the poles of the weakest magnet are successively presented to them. It is a very striking phenomenon, that a stone possessed of so high a degree of polarity should exhibit no attraction for iron which is not magnetised. I have never observed the smallest particle of filings of iron adhere to the serpentine; but the serpentine, reduced to powder, attaches itself very readily to the magnet.

You will enquire with impatience, if it be well proved that my serpentine is not mixed with magnetic iron; whether this mixture may not be sufficiently intimate to enter into the composition of each particle of the rock? I can assure you, that I have made the most assiduous enquiries in this respect. All my experiments were made in conjunction with Mr. Godeking, whose knowledge and abilities are a sufficient assurance against error; but we were decidedly convinced, that if the magnetic force cannot adhere to the earthy substances which form the base of the serpentine, it can be attributed only to the oxide of iron with which it is coloured. These are our reasons: The rock has no mixture of metallic substances. It presents only here and there a few fragments of talc or amianthus; but neither pyrites, nor schoerl, nor octahedrons of magnetic iron. When reduced to very fine powder, it resembles pounded chalk. The microscope discovers only earthy parts, of a clear whitish green. The specific gravity of this serpentine is very small. I find it only from 1.901 to 2.04 assuming water to be 1.0. There are not consequently any minerals but pumice-stone, mountain-leather, and some varieties of the opal, which do not equal our serpentine in density. The chemical experiments we have hitherto made, prove that it contains, like the jade or lapis ollaris, oxidised iron; but not iron capable of attraction by the magnet. The solutions in muriatic acid, mixed with the nitric acid, are yellow, and not green like those which the micaceous iron, and all the ores which contain pure or metallic iron, afford.

Here then is a very striking phenomenon, namely, the polarity of super-oxygenated iron. We learn by the valuable experiments of my celebrated countrymen Klaproth and Wenzel, that pure nickel and cobalt are attracted by the magnet; we know that iron slightly oxidised (the black oxide) is also affected; but how great the difference between this state of oxidation, and that of the iron which colours the serpentine, various calcareous stones, and perhaps even certain vegetable matters! What difference between a substance which acts alike on the two extremities of the needle, and a stone of which the smallest portions exercise a spontaneous polarity! Let us pursue the path of observation; let us collect indubitable facts. By this method the theories of natural philosophy will be established on solid and durable foundations.

Observations on the Stone which was forwarded to Sir JOSEPH BANKS with the preceding Memoir.

DESCRIPTION.—IT is of a bluish opake black colour, every where interspersed with minute particles of a yellowish rather silky white, and of no regular figure. No appearance of symmetry or crystallization presents itself, except on one side, where a rough indication

of laminae is seen. This is in the direction of the magnetic poles. The tenacity is very considerable, as it was not broken but by a violent blow on a round flint. Hardness between 6 and 7 of Kirwan; that is to say, it yields to the knife and file, but greatly injures those instruments. Powder white, or greenish-white. Fracture coarse, earthy. Fragments rather angular. No smell, nor adhesion to the tongue; but when two pieces are struck together its smell is the same as is given in like circumstances by rock crystal. Under the hammer it gives fire, but rarely with the steel. Specific gravity 2.91. Does not imbibe water. When touched with nitrous acid, a very slight effervescence took place, which was general and distinct, but scarcely visible without the magnifier. When strongly urged with the blow-pipe, it acquired an irregular light-brown colour, and was much less tenacious, but suffered no other change. Acid of borax and microcosmic salt dissolve it with little effervescence, and very slowly. The glass of the former is clear green, the latter clear white.

Its whole weight was 8238 grains; and that of the piece broken off for experiment, without the contact or use of iron, was 614 grains. It weighed 403 grains in water, and consequently lost 211 grains. Hence $\frac{614}{211} = 2.91$, or the specific gravity to water assumed as 1.00. This differs from the results found by Mr. Humboldt.

The fracture was made across the line of magnetical direction. The surfaces which had been thus separated, exhibited opposite poles, in the same manner as when any other natural magnet is broken. The smaller piece was then broken into many fragments, and in part pulverised. All the pieces were possessed of polarity. A small five-bar horse-shoe magnet took up the smaller fragments, and the powder, though weakly, as they were easily shaken off. I could not satisfactorily ascertain that any part of the stone did either attract the finest iron filings, or influence their arrangements when laid upon paper.

A very delicate magnetic needle, $1\frac{1}{2}$ inches long, moving on an agate socket, was suffered to dispose itself in the magnetic meridian. The larger piece was presented due east from the centre, and then moved upon its own centre, to find the positions of the greatest attraction and repulsion. At the distance of 18 inches from the middle of the stone, the needle was perceptibly affected; but its greatest deviation did not exceed ten minutes of a degree. At 12 inches the deviation was one degree, and at six inches it was about 14 degrees. By application of the stone close to the compass-box, the needle was led into any position at pleasure.

It was found on comparison with various pieces of natural magnet, that its directive force or effect on the needle is much weaker than that of any of the pieces. But whether a natural magnet as weak in directive power as this stone would be equally inactive with regard to iron filings, does not appear from the facts I have hitherto observed.

II.

An Account of some Experiments upon Coloured Shadows. By Lieutenant General Sir BENJAMIN THOMPSON, Count of Rumford, F. R. S. In a Letter to Sir JOSEPH BANKS, Bart. P. R. S.

DEAR SIR,

SINCE my last letter, being employed in the prosecution of my experiments upon light, I was struck with a very beautiful, and what to me appeared to be a new appearance. De-

firous of comparing the intensity of the light of a clear sky by day with that of a common wax-candle, I darkened my room, and letting the day-light from the north, coming through a hole near the top of the window-shutter, fall at an angle of about 70° upon a sheet of very fine white paper, I placed a burning wax-candle in such a position that its rays fell upon the same paper, and, as near as I could guess, in the line of reflection of the rays of day-light from without; when, interposing a cylinder of wood, about half an inch in diameter, before the centre of the paper, and at the distance of about two inches from its surface, I was much surprised to find that the two shadows projected by the cylinder upon the paper, instead of being merely shades without colour, as I expected, the one of them, that which, corresponding with the beam of day-light, was illuminated by the candle, was yellow; while the other, corresponding to the light of the candle, and consequently illuminated by the light of the heavens, was of the most beautiful blue that it is possible to imagine. This appearance, which was not only unexpected, but was really in itself in the highest degree striking and beautiful, I found upon repeated trials, and after varying the experiment in every way I could think of, to be so perfectly permanent, that it is absolutely impossible to produce two shadows at the same time, from the same body, the one answering to a beam of day-light and the other to the light of a candle or lamp, without these shadows being coloured, the one yellow, and the other blue.

The experiment may very easily be made at any time by day, and almost in any place, and even by a person not in the least degree versed in experimental researches. Nothing more is necessary for that purpose than to take a burning candle into a darkened room, in the day-time, and open one of the window-shutters a little, about half or three-quarters of an inch for instance; when, the candle being placed upon a table or stand, or given to an assistant to hold, in such a situation that the rays from the candle may meet those of day-light from without at an angle of about 40° at the surface of a sheet of white paper, held in a proper position to receive them, any solid opaque body, a cylinder, or even a finger, held before the paper, at the distance of two or three inches, will project two shadows upon the paper, the one blue and the other yellow.

If the candle be brought nearer to the paper, the blue shadow will become of a deeper hue, and the yellow shadow will gradually grow fainter; but if it be removed farther off, the yellow shadow will become of a deeper colour, and the blue shadow will become fainter; and the candle remaining stationary in the same place, the same varieties in the strength of the tints of the coloured shadows may be produced merely by opening the window-shutter a little more or less, and rendering the illumination of the paper, by the light from without, stronger or weaker. By either of these means, the coloured shadows may be made to pass through all the gradations of shade, from the deepest to the lightest, and *vice versa*; and it is not a little amusing to see shadows thus glowing with all the brilliancy of the purest and most intense prismatic colours, then passing suddenly through all the varieties of shade, preserving in all the most perfect purity of tint, growing stronger and fainter, and vanishing and returning, at command.

With respect to the causes of the colours of these shadows, there is no doubt but they arise from the different qualities of the light by which they are illuminated; but how they are produced, does not appear to me so evident. That the shadow corresponding to the beam of day-light which is illuminated by the yellow light of a candle, should be of a yellowish hue, is not surprising; but why is the shadow corresponding to the light of the

candle, and which is illuminated by no other light than the apparently white light of the heavens, blue? I at first thought that it might arise from the blueness of the sky; but finding that the broad day-light reflected from the roof of a neighbouring house covered with the whitest new-fallen snow produced the same blue colour, and, if possible, of a still more beautiful tint, I was obliged to abandon that opinion.

To ascertain with some degree of precision the real colour of the light emitted by a candle, I placed a lighted wax-candle, well trimmed, in the open air, at mid-day, at a time when the ground was deeply covered with new-fallen snow, and the heavens were overspread with white clouds; when the flame of the candle, far from being white, as it appears to be when viewed by night, was evidently of a very decided yellow colour, not even approaching to whiteness. The flame of an Argand's lamp, exposed at the same time in the open air, appeared to be of the same yellow hue. But the most striking manner of shewing the yellow hue of the light emitted by lamps and candles, is by exposing them in the direct rays of a bright meridian sun. In that situation the flame of an Argand's lamp, burning with its greatest brilliancy, appears in the form of a dead yellow semi-transparent smoke. How transcendently pure and inconceivably bright the rays of the sun are when compared to the light of any of our artificial illuminators, may be gathered from the result of this experiment.

It appearing to me very probable that the difference in the whiteness of the two kinds of light which were the subjects of the foregoing experiments, might somehow or other be the occasion of the different colours of the shadows, I attempted to produce the same effects by employing two artificial lights of different colours; and in this I succeeded completely.

In a room previously darkened, the light from two burning wax-candles being made to fall upon the white paper, at a proper angle, in order to form two distinct shadows of the cylinder, these shadows were found not to be in the least coloured; but upon interposing a pane of yellow glass, approaching to a faint orange colour, before one of the candles, one of the shadows immediately became yellow, and the other blue. When two Argand's lamps were made use of, instead of the candles, the result was the same: the shadows were constantly and very deeply coloured, the one yellow, approaching to orange, and the other blue, approaching to green. I imagined that the greenish cast of this blue colour was owing either to the want of whiteness of the one light, or to the orange hue of the other, which it acquired from the glass.

When equal panes of the same yellow glass were interposed before both the lights, the white paper took an orange hue; but the shadows were to all appearance without the least tinge of colour; but two panes of the yellow glass being afterwards interposed before one of the lights, while only one pane remained before the other, the colours of the shadows immediately returned.

The result of these experiments having confirmed my suspicions that the colours of the shadows arose from the different degrees of whiteness of the two lights, I now endeavoured, by bringing day-light to be of the same yellow tinge with candle-light by the interposition of sheets of coloured glass, to prevent the shadows being coloured when day-light and candle-light were together the subjects of the experiment; and in this I succeeded. I was even able to reverse the colours of the shadows, by causing the day-light to be of a deeper yellow

yellow than the candle-light. In the course of these experiments I observed that different shades of yellow given to the day-light produced very different and often quite unexpected effects: thus one sheet of the yellow glass interposed before the beam of day-light changed the yellow shadow to a lively violet colour, and the blue shadow to a light green; two sheets of the same glass nearly destroyed the colours of both the shadows; and three sheets changed the shadow which was originally yellow to blue, and that which was blue to a purplish yellow colour.

When the beam of day-light was made to pass through a sheet of blue glass, the colours of the shadows, the yellow as well as the blue, were improved and rendered in the highest degree clear and brilliant; but when the blue glass was placed before the candle, the colours of the shadows were very much impaired.

In order to see what would be the consequence of rendering the candle-light of a still deeper yellow, I interposed before it a sheet of yellow or rather orange-coloured glass; when a very unexpected and most beautiful appearance took place. The colour of the yellow shadow was changed to orange, the blue shadow remained unchanged, and the whole surface of the paper appeared to be tinged of a most beautiful violet colour, approaching to a light crimson or pink; almost exactly the same hue as I have often observed the distant snowy mountains and valleys of the Alps to take about sun-set. Is it not more than probable that this hue is in both cases produced by nearly the same combinations of coloured light? In the one case it is the white snow illuminated at the same time by the purest light of the heavens, and by the deep yellow rays from the west; and in the other it is the white paper illuminated by broad day-light, and by the rays from a burning candle, rendered still more yellow by being transmitted through the yellow glass. The beautiful violet colour which spreads itself over the surface of the paper will appear to the greatest advantage if the pane of orange-coloured glass be held in such a manner before the candle, that only a part of the paper, half of it, for instance, be affected by it, the other half remaining white.

To make these experiments with more convenience, the paper, which may be about eight or ten inches square, should be pasted or glued down upon a flat piece of board, furnished with a ball and socket upon the hinder side of it, and mounted upon a stand, and the cylinder should be fastened to a small arm of wood or of metal, projecting forward from the bottom of the board for that purpose. A small stand, capable of being made higher or lower as the occasion requires, should likewise be provided for supporting the candle; and if the board with the paper fastened upon it be surrounded with a broad black frame, the experiments will be so much the more striking and beautiful. For still greater convenience, I have added two other stands for holding the coloured glass through which the light is occasionally made to pass in its way to the white surface upon which the shadows are projected. It will be hardly necessary to add, that, in order to the experiments appearing to the greatest advantage, all light which is not absolutely necessary to the experiment must be carefully shut out.

Having fitted up a little apparatus, according to the above directions, merely for the purpose of prosecuting these enquiries respecting the coloured shadows, I proceeded to make a great variety of experiments, some with pointed views, and others quite at random, and merely in hopes of making some accidental discovery that might lead to a knowledge of the causes of appearances which still seemed to me to be enveloped in much obscurity and uncertainty.

Having

Having found that the shadows corresponding to two like wax candles were coloured, the one blue and the other yellow, by interposing a sheet of yellow glass before one of them; I now tried what the effect would be when blue glass was made use of instead of yellow, and I found it to be the same; the shadows were still coloured, the one blue and the other yellow, with the difference, however, that the colours of the shadows were reversed, that which with the yellow glass was before yellow being now blue, and that which was blue being yellow.

I afterwards tried a glass of a bright amethyst colour, and was surprised to find that the shadows still continued to be coloured blue and yellow. The yellow, it is true, had a dirty purple cast; but the blue, though a little inclining to green, was nevertheless a clean, bright, decided colour.

Having no other coloured glass at hand to push these particular enquiries farther, I now removed the candles, and, opening two holes in the upper parts of the shutters of two neighbouring windows, I let into the room, from above, two beams of light from different parts of the heavens; and placing the instrument in such a manner that two distinct shadows were projected by the cylinder upon the paper, I was entertained by a succession of very amusing appearances. The shadows were tinged with an infinite variety of the most unexpected, and often most beautiful colours, which, continually varying, sometimes slowly and sometimes with inconceivable rapidity, absolutely fascinated the eyes, and, commanding the most eager attention, afforded an enjoyment as new as it was bewitching. It was a windy day, with flying clouds; and it seemed as if every cloud that passed brought with it another complete succession of varying hues and most harmonious tints. If any colour could be said to predominate, it was purple; but all the varieties of browns, and almost all the other colours I ever remembered to have seen, appeared in their turns; and there were even colours which seemed to me to be perfectly new.

Reflecting upon the great variety of colours observed in these last experiments, many of which did not appear to have the least relation to the apparent colours of the light by which they were produced, I began to suspect that the colours of the shadows might in many cases, notwithstanding their apparent brilliancy, be merely an optical deception, owing to contrast, or to some effect of the other neighbouring colours upon the eye. To determine this fact by a direct experiment, I proceeded in the following manner: Having, by making use of a flat ruler, instead of the cylinder, contrived to render the shadows much broader, I shut out of the room every ray of day-light, and prepared to make the experiment with two Argand's lamps, well trimmed, and which were both made to burn with the greatest possible brilliancy; and having assured myself that the light they emitted was precisely of the same colour, by the shadows being perfectly colourless which were projected upon the white paper, I directed a tube about 12 inches long, and near an inch in diameter, lined with black paper, against the centre of one of the broad shadows; and looking through this tube with one eye, while the other was closed, I kept my attention fixed upon the shadow, while an assistant repeatedly interposed a sheet of yellow glass before the lamp, whose light corresponded to the shadow I observed, and as often removed it. The result of the experiment was very striking, and fully confirmed my suspicions with respect to the fallacy of many of the appearances in the foregoing experiments. So far from being able to observe any change in the shadow upon which my eye was fixed, I was not able

even to tell when the yellow glass was before the lamp, and when it was not; and though the assistant often exclaimed at the striking brilliancy and beauty of the blue colour of the very shadow I was observing, I could not discover in it the least appearance of any colour at all. But as soon as I removed my eye from the tube, and contemplated the shadow with all its neighbouring accompaniments, the other shadows rendered really yellow by the effect of the yellow glass, and the white paper, which had likewise from the same cause acquired a yellowish hue, the shadow in question appeared to me, as it did to my assistant, of a beautiful blue colour. I afterwards repeated the same experiment with the apparently blue shadow produced in the experiment with day-light and candle-light, and with exactly the same result.

How far these experiments may enable us to account for the apparent blue colour of the sky, and the great variety of colours which frequently adorn the clouds, as also what other useful observations may be drawn from them, I leave to philosophers, opticians, and painters to determine. In the mean time, I believe it is a new discovery; at least it is undoubtedly a very extraordinary fact, that the eyes are not always to be believed, even with respect to the presence or absence of colours.

I cannot finish this letter without mentioning one circumstance which struck me very forcibly in all these experiments upon coloured shadows, and that is, the most perfect harmony which always appeared to subsist between the colours, whatever they were, of the two shadows; and this harmony seemed to me to be full as perfect and pleasing when the shadows were of different tints of brown, as when one of them was blue and the other yellow. In short, the harmony of these colours was in all cases not only very striking, but the appearances were altogether quite enchanting; and I never found any body to whom I shewed these experiments, whose eyes were not fascinated with their bewitching beauties. It is however more than probable, that a great part of the pleasure which these experiments afforded to the spectators, arose from the continual changes of colour, tint, and shade with which the eye was amused, and the attention kept awake. We are used to seeing colours fixed and unalterable, hard as the solid bodies from which they come, and just as motionless; consequently dead, uninteresting, and tiresome to the eye; but, in these experiments, all is motion, life and beauty.

It appears to me very probable, that a further prosecution of these experiments upon coloured shadows may not only lead to a knowledge of the real nature of the harmony of colours, or the peculiar circumstances upon which that harmony depends; but that it may also enable us to construct instruments for producing that harmony for the entertainment of the eyes, in a manner similar to that in which the ears are entertained by musical sounds. I know that attempts have already been made for that purpose; but when I consider the means employed, I am not surprised that they did not succeed. Where the flowing tide, the varying swell, the crescendo is wanting, colours must ever remain hard, cold, and inanimate masses.

I am very sorry that my more serious occupations do not at present permit me to pursue these most entertaining enquiries. Perhaps at some future period I may find leisure to resume them*.

I am, &c.

Munich, March 1, 1793.

* Otto Guericke, Buffon, Mazzeas, Beguelin, and many other philosophers, have paid attention to the coloured shadows of bodies which form the subject of this interesting letter. An abridgement of their observations
and

III.

A Memoir upon the Discovery of America. By M. OTTO.

[Concluded from page 77.]

AFTER having performed several other interesting voyages, the Chevalier Behem died at Lisbon in July 1506, regretted by every one, but leaving behind him no other work than the globe which we have just been speaking of. It is made from the writings of Ptolemy, Pliny, Strabo, and especially from the account of Mark Paul, the Venetian, a celebrated traveller of the thirteenth century; and of John Mandeville, an Englishman, who, about the middle of the fourteenth century, published an account of a journey of thirty-three years in Africa and Asia. He has also added the important discoveries made by himself on the coasts of Africa and America.

From these circumstantial accounts, little known to modern writers, we must conclude that Martin Behenira, of whom Garcilasso makes mention, is the same Chevalier Behem, upon being the place of whose birth Nuremberg prides itself so much. It is probable that, as soon as he was knighted in Portugal, he thought it necessary to give a Portuguese termination to his name, to make it more sonorous and more conformable to the idiom of the country. Garcilasso, deceived by this resemblance of sound, has made him a Spaniard, in order to deprive Christopher Columbus of the honour of having procured to his country so great an advantage. And what ought to confirm us in this opinion is, that we neither find in Mariana, nor any other Spanish historian, the name of this Martin Behenira, who was certainly a man of too much importance not to have had a distinguished place in history. Besides, the Spanish pride would have been flattered in giving to a native those laurels with which it crowned Christopher Columbus.

It is then very unlikely that this navigator was treated as an enthusiast, when he offered to the Court of Portugal to make discoveries in the west. The search after unknown countries was at that time the reigning passion of this Court; and even if the Chevalier Behem had not offered the interesting ideas which he had procured, the novelty of the project would undoubtedly have engaged King John to support the views of Columbus: but it appears that this prince declined it, because all his thoughts were turned at that time to the coast of Africa, and the new passage to the Indies, from whence he procured great riches; whilst the southern coast of Brazil, and the territories of the Patagonians, seen by Behem, offered to him only barren lands inhabited by unconquerable savages. The refusal of John II., very far from weakening the testimony of Behem's discoveries, is then

and deductions may be seen in Priestley's Optics, p. 436. Count Rumford is the first, as far as I know, who has shewn that the effect depends not immediately on the nature of the light, but principally upon the manner in which the organs of sight, or perhaps the organs of thought (if in truth there be any difference here between them), are affected by the successive actions they undergo. The colours called accidental, which are rendered permanent for a time after the impression of bright objects upon the eye, and the effects of less forcible impressions, for which the last quoted work, p. 631, and the authors there referred to, may be consulted;—the harmony and discord of colours, probably arising from the pleasure or disgust afforded by the admixture of an accidental colour with a new sensation, or real colour;—the phenomena of dazzling, which is of the same nature as this last combination;—and the general arrangement and inferences to be found in the Zoonomia of Darwin, all bear evident relation to the facts exhibited by Count Rumford, and open a wide field for curious research. N.

rather a proof of the knowledge which this politic prince had already procured of the existence of a new continent: and it was only in 1501, that is to say, three years after the voyage of Vasco de Gama to the Indies, that Emanuel thought proper to take advantage of the discoveries of Behem, by sending Alvarez Cabral to Brazil; a measure which was perhaps rather owing to the jealousy which has always existed between Portugal and Spain, than to a desire of making advantageous establishments, for which the Indies were much more proper than this part of America.

If any doubts yet remain respecting the important discovery made by the Chevalier Behem, it is particularly the authority of Dr. Robertson which attacks the testimony of the different authors we have transcribed. This learned writer treats the history of Behem as a fiction of some German authors, who had an inclination to attribute to one of their countrymen a discovery which has produced so great a revolution in the commerce of Europe. But he acknowledges, nevertheless, after Herrera, that Behem had settled at the island of Fayal; that he was the intimate friend of Christopher Columbus; and that Magellan had a globe made by Behem, by the help of which he undertook his voyage to the South Sea; a circumstance which proves much in favour of our hypothesis. He relates also, that in 1492 this astronomer paid a visit to his family at Nuremberg, and left there a map drawn by himself, of which Dr. Forster procured him a copy, and which, in his opinion, partakes of the imperfection of the cosmographical knowledge of the 15th century: that he found in it, indeed, under the name of the island of St. Brandon, land which appears to be the present coast of Guiana, and lies in the latitude of Cape Verd; but that there is reason to believe that this fabulous island, which is found in many ancient maps, merits no more attention than the childish legend of St. Brandon himself. Although Dr. Robertson does not appear disposed to grant to Behem the honour of having discovered the new continent, we find the means of refuting him in his own History. He allows that Behem was very intimate with Christopher Columbus; that he was the greatest geographer of his time, and scholar of the celebrated John Müller, or Regiomontanus; that he had discovered, in 1483, the kingdom of Congo upon the coast of Africa; that he made a globe which Magellan made use of; that he drew a map at Nuremberg, containing the particulars of his discoveries; and that he placed in this chart land which is found to be in the latitude of Guiana. Dr. Robertson asserts, without any proof, that this land was but a fabulous island. We may suppose, upon the same foundation, that the Chevalier Behem, engaged in an expedition to the kingdom of Congo, was driven by the winds to Fernambouc, and from thence by the currents very common in those latitudes towards the coast of Guiana; and that he took for an island the first land which he discovered. The course which Christopher Columbus afterwards steered makes this supposition still more probable; for if he knew only of the coast of Brazil, which they believe to have been discovered by Behem, he would have laid his course rather to the south-west. The expedition to Congo took place in 1483: it is then possible that at his return Behem proposed a voyage to the coasts of Brazil and Patagonia; and that he requested the assistance of his sovereign, which we have mentioned above. It is certain that we cannot have too much deference for the opinion of so eminent a writer as Robertson; but this learned man not having it in his power to consult the original German documents which we have quoted, we may be allowed to form a different opinion without being too presumptuous.

But should it be asked, Why we take from Christopher Columbus the reputation which all Europe has to this day allowed him?—why search into the archives of an imperial city for the causes of an event which took place in the most western extremity of Europe?—why the enemies of Christopher Columbus, who were numerous, did not take advantage of the pretended Chevalier Behem to lessen his consequence at the Spanish Court?—why Portugal, jealous of the discovery of the New World, has not protested against the assertions of the Spaniards?—why Behem, who died only in 1506, had not left to posterity any writing to confirm to himself so important a discovery?

To answer all these questions, I shall submit to the impartial reader the following remarks:

1. Before Columbus, the great merit of a navigator consisted rather in conceiving the possibility of the existence of a new continent, than in searching for lands in a region where he was sure to find them. If it is then certain that Behem had conceived this bold idea before Columbus; the fame of the latter must be considerably diminished.

2. The historical proofs which we have given above leaving us no doubt of the fact, we have only to explain the moral causes of the silence of the Spanish and Portuguese authors, of the enemies of Columbus, and of Behem himself.

3. It is well known that, previous to the reign of Charles V. there was little communication between the learned men of different nations. Writers were scarce, excepting some monks, who have related, well or ill, the events which came to their knowledge, in chronicles which are no longer read; or they had but little notion of what passed in foreign countries. Gazettes and Journals were unknown, and the Learned were obliged to travel, to inform themselves of the progress of their neighbours. Italy was the centre of the arts, and what are called sciences, at that time. The frequent journeys of the German emperors to Rome gave them an opportunity of knowing persons of merit, and of placing them in the different universities of the empire. It is to this circumstance that we ought to attribute the great progress which the Germans made, particularly in mathematics, from the fourteenth to the sixteenth century; during which time they had the best geographers, the best historians, and the most enlightened politicians. They were particularly attentive to what passed in Europe; and the multiplied connections of different princes with foreign powers assisted them greatly in collecting, in their archives, the original pieces of the most important events of Europe. It is to this spirit of criticism and enquiry that we are indebted for the reformation of Luther; and we cannot deny that, particularly in the fifteenth century, there was more historical and political knowledge in Germany than in all the rest of Europe, Italy excepted. It is not then astonishing that we should find, in the archives of one of the most ancient imperial cities, the particulars of an expedition planned upon the banks of the Tagus, by a German, a man of great repute in his own country, and whose every action became very interesting.

4. It was different in Portugal, where the whole nation, except the king, was plunged in the most profound ignorance. Every one was either merchant, sailor or soldier; and if this nation has made the most important discoveries, we must ascribe them rather to avarice than to a desire of knowledge. They were satisfied with scraping together gold in every quarter of the known world, whilst the German and the Italian took up the pen to transmit to posterity the remembrance of their riches and cruelties. The Spaniards were

not

not much more informed before Charles V. introduced at Madrid the learned men of Flanders and Germany. It is then very possible that the Chevalier Behem made very interesting discoveries in geography in 1485, without the public being acquainted with them. If he had brought back from his expedition gold or diamonds, the news would have been spread in a few weeks; but simple geographical knowledge was not of a nature to interest men of this turn of mind.

5. The long stay which Christopher Columbus made at Madeira makes his interview with Behem more than probable. It is impossible that he should have neglected seeing a man so interesting, and who could give him every kind of information for the execution of the plan he had formed. The mariners who accompanied the Chevalier Behem might also have spread reports at Madeira and the Azores concerning the discovery which they had been witnesses of. What ought to confirm us in this is, that Mariana says himself (book xxvii. chap. iii.) that a certain vessel going to Africa was thrown by a gale of wind upon certain unknown lands; and that the sailors, at their return to Madeira, had communicated to Christopher Columbus the circumstances of their voyage. All authors agree that this learned man had some information respecting the western shores; but they speak in a very vague manner. The expedition of the Chevalier Behem explains this mystery.

6. This astronomer could not be jealous of the discoveries of Columbus, because the last had been farther north, and that in a time when they did not know the whole extent of the New World; and when geographical knowledge was extremely bounded, it might be believed that the country discovered by Columbus had no connection with that discovered by Behem.

It appears however certain, that Behem discovered this continent before Columbus; and that this question, which is only curious in Europe, becomes interesting to the American patriot. The Grecians have carefully preserved the fabulous history of their first founders, and have raised altars to them: Why are not Behem, Christopher Columbus, and Vesputius deserving of statues in the public squares of American cities? These precious monuments would transmit to posterity the gratitude which these benefactors of mankind should inspire. Without knowing it, they have laid the foundation of the happiness of many millions of inhabitants; and Sesostris, Phul, Cyrus, Theseus, and Romulus, the founders of the greatest empires, will be forgotten before the services rendered by these illustrious navigators can be effaced from the memory of man.

IV.

Description of a Gravimeter, or Instrument for measuring the Specific Gravity of Solids and Fluids.
By Citizen GURTON*.

EVER since the art of chemistry, by an approach to the accurate sciences, has shewn that the phenomena of combinations produced or destroyed are not the result of occult qualities, but of a rupture of equilibrium determined by forces which afford the hope of admeasurement by computation, philosophers have been aware of the necessity of conducting their experiments with precision, that they might take account of all the circumstances

* Read to the National Institute on the 31th Germinal, in the 4th year of the Republic. It is inserted in the twenty-first volume of the *Annales de Chimie*. This translation is nearly verbal.

which

which might impede or favour this motion. The specific gravity of bodies must necessarily constitute part of those observations: for it serves not only to indicate the nature of bodies under examination, but likewise affords information respecting their purity, and the state of their aggregation, condensation, or rarefaction, which are also the immediate causes of divulsion or repose. It is therefore of importance that the instruments for measuring this property of bodies should be brought to the utmost perfection they are capable of receiving, and that they should be rendered as convenient as possible for habitual use.

Of all the hydrometers hitherto known, that of Fahrenheit is acknowledged to be the most accurate. The well known principle of this instrument is, that it ascertains the weight of a constant bulk of the several fluids under examination. Such hydrometers as are constructed to measure the density by the degree of immersion, may serve, in some manufactories, to give an approximation sufficiently near for the purpose required. But without considering the inequality of the stem, the tedious work of graduating by observation, and the uncertainty of estimate from one division to another, it may be remarked, that they are not capable of correction for the different temperatures. In a word, they are not fit for the hand of the philosopher.

The form which Nicholson gave some years ago to the hydrometer of Fahrenheit *, rendered it proper to measure the density of solids. At present it is very much used. It gives, with considerable accuracy, the ratio of the specific gravity to the sixth decimal, water being taken as unity. It is susceptible of correction for the variations of temperature, and the impurity of the water which it is sometimes more convenient to use, as may be seen in the article *Areometer*, in the chemical part of the *Encyclopedie Methodique*. It does not appear that any better instrument need be wished for in this respect.

But this hydrometer has hitherto been constructed in metal only; so that it could not be applied either to salts or acids. It is known likewise that areometers constructed on the principles of Fahrenheit, for spirituous, saline, and acid liquors, require to be varied in magnitude, form, and quantity of ballast. In the one kind, the lower weight must be at a great distance from the buoyant part, to maintain the vertical situation; in the other, it is brought nearer, to operate on smaller masses of the fluid. Those instruments which are intended for alcohol must be light, and those applied to determine the density of concentrated acids must be heavy. The mass and dimensions of every instrument of this nature must be adjusted in such a manner that the additional weight may operate not only as a load, but as ballast. It cannot be applied in the upper basin without deranging the vertical situation. From all which circumstances it happens, that a collection of these instruments would be required, to answer every purpose of experiment. As a remedy for part of these inconveniences, it was ingeniously proposed that the areometer should terminate beneath in a hook, to which were suspended at pleasure certain glass balls filled with mercury,

* From Lowthorpe's Abridgement of the Philosophical Transactions, I. 604, or Boyle's Works, in quarto, London, 1772, IV. 204, it appears that the hydrometer was first invented by Boyle, and described under the name of a new Essay Instrument. It had a graduated stem, and, by means of a stirrup or clip underneath, it was applied, as perfectly as a graduated instrument could be, to ascertain the specific gravities of solids as well as fluids. Fahrenheit first applied a dish for weights at the top, to ascertain the specific gravities of fluids only; as may be seen in Read and Gray's Abridgement of the Philosophical Transactions, Vol. VI. Part I. p. 294. My instrument, referred to by Citizen Guyton, is a combination of both. It is described in the 2d volume of the Manchester Memoirs, and is represented in Plate VI. Fig. 1. N.

and constituting different ballasts; but this did not answer in every respect. In all the instruments of this kind which I have had occasion to examine, no attempt had been made, or at least the object had not been accomplished, to render the point of intersection of the stem with the fluid, common to all the ballasts. Whence it followed, that, upon changing the ballast, it was likewise necessary to change the slip of glass which carries the mark within the upper stem.

I have thought it possible, by following the principles of Fahrenheit and executing the instrument of Nicholson in glass with a slight addition, to render it more generally useful and commodious, without diminishing its accuracy in any respect. I was well aware of the prejudice which is naturally entertained against polychrest, or universal instruments, most of which are rendered of no use from the attempt to extend their application too far; but at the same time I was convinced that it would be a real advantage to every philosophical observer, to require only one single measure for determining the density of all bodies, whether solid or liquid. This is the object I propose to accomplish. It will be seen how far I have succeeded. I must observe, on this occasion, that the name hydrometer (*pese-liqueur*) as well as that of areometer, is scarcely applicable to an instrument possessing these qualities: for these terms suppose that the liquid is always the thing weighed; whereas, with regard to solids, the liquid is the term of comparison which is known, and to which the unknown weight is referred. I propose therefore to make use of the name gravimeter, which will easily be understood, and applied with propriety in every case.

This instrument, being executed, as already remarked, in glass, is of a cylindric form, being that which requires the smallest quantity of the fluid, and is on that account preferable, except so far as it is necessary to deviate for the security of a perpendicular position.

Like the instrument of Nicholson, it carries two basons; the one superior, at the extremity of a thin stem; towards the middle of which the fixed point of immersion is marked. The other lower bason terminates in a point; it contains the ballast, and is attached to the cylinder by two branches. The moveable suspension by means of a hook has the inconvenience of shortening the lever which is to secure the vertical position.

The cylinder is 22 millimeters (0.71 inches) in diameter; and 21 centimeters (6.85 inches) in length. It carries in the upper bason an additional constant weight of five grammes. These dimensions might be increased, so as to render it capable of receiving a much more considerable weight; but it will hereafter be shewn, that this is unnecessary.

I have added a piece which I call the diver (*plongeur*), because in fact it is placed in the lower bason when used, and consequently is entirely immersed in the fluid*. It is a bulb of glass, loaded with a sufficient quantity of mercury, in order that its total weight may be equal to the constant additional weight, added to the weight of the volume of water displaced by this piece.

It will readily be understood, that the weight being determined at the same temperature at which the instrument was originally adjusted, it will sink to the same mark on the stem, whether it be loaded with a constant additional weight in the upper bason, or whether the effect of this weight be produced by the additional piece in the lower dish.

From this explanation there will be no difficulty in deducing how this instrument may be adapted to every case of practice.

* As we have no word in the English language which can with propriety be used as the translation of the word *plongeur*, I shall take the liberty to call this weight the additional piece. N.

It may be used, 1. for solids. It is the hydrometer of Nicholson, from which it differs in no respect. The only condition will be, as in his instrument, that the absolute weight of the body to be examined shall be rather less than the constant additional weight, which in this instrument is five grammes (115 grains).

2. For liquids of less specific gravity than water, the instrument, without the additional weight above, weighs about two decagrammes (459 grains) in the dimensions before laid down. It would be easy to limit its weight to the utmost accuracy. We have therefore the range of one-fifth of buoyancy, and consequently the means of ascertaining all the intermediate densities from water to the most highly rectified spirit of wine, which is known to bear in this respect the ratio of eight to ten with regard to water.

3. When liquids of greater specific gravity than water are to be tried, the constant weight being applied below, by means of the additional piece, which weighs about six grammes (138 grains), the instruments can receive in the upper basin more than four times the usual additional weight, without losing the equilibrium of its vertical position. In this state it is capable of shewing the specific gravity of the most concentrated acids.

4. It possesses another property common to the instrument of Nicholson, namely, that it may be used as a balance to determine the absolute weight of such bodies as do not exceed its additional load.

5. Lastly, the purity of the water being known, it will indicate the degrees of rarefaction and condensation in proportion to its own bulk.

I have little to say respecting the construction of this instrument. Every workman in glass who shall once see it, will be able to make it without difficulty. The additional piece for the lower basin will require some attention to make it perfectly agree with the constant upper weight, as to the immersion of the instrument. But this object may, by careful adjustment, be ascertained with the utmost certainty and accuracy.

The bulb of glass is for this purpose drawn out to a fine point; a sufficient quantity of mercury is then introduced to sink it, and the aperture closed with a morsel of wax. The bulb being then placed in the lower basin of the instrument, the upper basin is to be loaded until the mark on the stem becomes accurately coincident with the surface of the water. The sum of the weights added above is precisely equal to that of the quantity of mercury necessary to be added to that in the glass bulb; which done, nothing more is necessary than to seal the point by fusion, taking care not to change its bulk.

Though this instrument is, taking delicate in form, it has no other imperfection than the natural brittleness of the material, which must necessarily be used for experiments with saline and acid liquors. For six months past I have made very frequent use of one of these instruments in the Polytechnic School, without any other inconvenience than that one of the branches of the lower basin was accidentally broken.

Nothing more remains but to render it portable. I apprehend that this object is sufficiently secured by means of a case in which all the delicate parts are secured from pressure, and the heavier parts supported in such a manner as to resist the excess of motion they are capable of acquiring by virtue of their mass. This last circumstance is frequently overlooked by such workmen as are employed in the package of instruments; whence it necessarily follows, that some strain or fracture must be produced when matters of very unequal density are exposed to receive a common impulse.

The method of securing this instrument in its case will be better understood from the figure, than from the most extended verbal description.

ADDITION to the foregoing MEMOIR.

THE constant use I have made of the gravimeter since I presented the description to the National Institute, has led me to make some slight changes, by which its construction is rendered more easy, and the effects of brittleness in one of its principal parts are removed.

It has also appeared to be of some utility to annex to this memoir the formula, by means of which, the gravimeter being once well adjusted, we are enabled, by a very simple calculation, to find the specific gravity of any substance whatever in proportion to that of distilled water at the temperature of 12.5 degrees of the decimal thermometer, and 757.7 millimeters of pressure, without the use either of distilled water, or the thermometer or barometer. The astonishment which I have sometimes remarked when the problem has been thus announced, induces me to think that the solution here added will be acceptable, instead of the reference to the article *Areometer* of the Dictionary of Chemistry, in the *Encyclopédie Methodique*.

I shall likewise add an explanation of the figures which represent the instrument and its case, together with examples of the application of the gravimeter to tables of specific gravity.

Description of a Solid Additional Piece for the Lower Basin.

THE condition of hermetically sealing the additional piece without changing its volume necessarily requires that the part near the aperture should be very thin. Hence it has sometimes happened that the point was broken without any external blow, but merely in consequence of the motion of the mercury contained within. A similar glass bulb might indeed be added; but this constitutes but a small part of the remedy. For it is necessary to adjust the instrument again to preserve the property of measuring the specific gravity of the denser fluids, and it has been found that this operation was not exempt from difficulties when the desired degree of precision was to be sought.

I have remedied this inconvenience by substituting in the place of the glass bulb loaded with mercury a small mass of solid glass, as the stopper of a bottle, which is first brought to the proper form by grinding, and afterwards carefully diminished, until, when placed in the lower basin of the instrument, its immersion in distilled water at the required degrees of temperature and pressure shall be exactly the same as when the instrument is floated in the same liquid with its constant additional weight in the upper basin only.

By this means there is a certainty of acquiring the utmost degree of precision at first trial; because the whole process is reduced to the mere adjustment of a weight.

Concerning the Application of the Gravimeter, to find the Specific Gravity of any Substance whatever, without requiring Distilled Water, or the Thermometer or Barometer, or any subsequent Correction.*

THE gravimeter being supposed to be well regulated, let x represent the specific gravity sought; b the additional weight necessary to sink the instrument to the mark in the unknown fluid;

* There is a correction overlooked by the celebrated author of this paper, and by almost every writer on this subject. It arises from the expansion or contraction of the instrument itself, and of the solid under examination.

fluid; c the weight which placed with the solid in the upper basin immerses the instrument to the mark; d the additional weight to produce the same effect when the body is in the lower basin; Π the specific gravity of distilled water, at the temperature of 12.5 degrees of the decimal thermometer, and the pressure of 757.7 millimeters = 1; Π' the specific gravity of the water made use of.

The following formula gives the solution*: $x = \frac{(b-c) \Pi'}{d-c}$

The value of Π' is first to be found, which is greater than unity when the water made use of is heavier than distilled water, and in the contrary case is a fraction.

Let P represent the weight of the gravimeter without any additional weight \dagger ; V the constant volume of the immersed part; a the additional constant weight in the upper basin, or that which immerses it to the mark in distilled water Π ; and we shall have $P + a = V \Pi$; whence $V = \frac{P+a}{\Pi}$

Again b represents the weight more or less than a , which must be substituted to produce the same immersion in another liquor different from distilled water.

\ddagger We shall therefore have $\Pi' = \frac{P+b}{V} = \frac{P+b}{P+a}$.

The value of Π' being found, every thing else is known; nothing more being necessary than to substitute this value in the formula.

I am persuaded that philosophers will immediately perceive the advantages of this method. Distilled water was wanting; but this praxis renders it unnecessary. Even if distilled water were at hand, it would seldom happen that the times of the standard temperature and pressure would agree with those of the experiment; and when an artificial temperature is produced, it is subject to vary during the course of the experiment. All these difficulties are removed; and even when distilled water is at hand, I prefer, more especially in summer, water containing a small portion of neutral salt. Two motives justify this preference:

1. *mination.* I am not prepared to state the method of applying this correction; and shall only remark, in this place, that the mere change of dimensions from temperature in a piece of steel (independent of the greater change in the whole instrument) will alter the fourth figure at every third degree of Fahrenheit. For pyrometrical data see *Philos. Journal*, I. 58. N.

2. For the sake of those who are unacquainted with symbols, I shall here give the rule in words at length: From the weight in the upper dish, when the instrument is properly immersed in the unknown fluid, take the weight which is placed with the body in the same scale at the like adjustment. The remainder is the absolute weight of the solid. Multiply this by the specific gravity of the fluid, and reserve the product. From the additional weight when the body is placed in the lower basin, take the additional weight when it was placed in the upper. The remainder will be the loss of weight by immersion. Divide the reserved product by the loss by immersion, and the quotient will be the specific gravity of the solid with regard to distilled water at the standard temperature and pressure. N.

\dagger In the upper dish. For as the lower additional piece acts only by its residual weight, and this must vary with the fluid, it is clear that it must be considered as part of the instrument, and enter into the value of P whenever it is used. In fact, the gravimeter with this piece is an instrument perfectly distinct from that in which it is not used, and the adjustment may then be made as well (and perhaps more easily) by a constant small weight in the upper dish, as by the very delicate process of the author. N.

\ddagger Or in words: To the weight of the gravimeter add the weight required in the upper basin to sink it in the unknown fluid. Again, to the weight of the gravimeter add the weight required in the same manner to sink it in distilled water. Divide the first sum by the latter, and the quotient will be the specific gravity of the fluid in question. N.

1. It is more convenient to add a few milligrams to the constant additional weight, than to compose a less by a series of sub-multiples; 2. By making use of a liquid, at the temperature of the surrounding air, it is evidently less exposed to sudden variations. These circumstances are more favourable to accuracy in the result.

Explanation of the Figures.

Fig. 2, Plate VI.—The gravimeter.

- a. The lower bason.
- b. The upper bason.
- c. The point of immersion marked on a thin piece of glass in the inside of the stem.

Fig. 3. The piece called *plongeur*, which is placed in the lower bason when experiments are made on fluids of greater density than water.

Fig. 4. The gravimeter in the cylindric vessel filled with water, in which it floats, immersed to the mark c, by means of the additional constant weight d.

It is convenient to choose a vessel of such a depth that the instrument may be at liberty to float at the level of the mark, or even beneath it, without its being possible that the bottom of the upper bason should ever descend to the surface of the water.

Fig. 5. The gravimeter in its case.

- A. The cylindrical part of the instrument lodged in a groove in the case, and secured above by the two projections e e which leave the stem at liberty. It is secured at the middle by the brass button f, and is pressed below by a piece of cork g, which rests on the fixed block h.
- i. Sliding piece and screw to support the ballast-piece, and prevent the branches from being endangered by any internal movement of the mercury.
- k. The additional ballast-piece, or *plongeur* in its separate cell.
- l. The constant additional weight placed in a cell cut in the solid wood, and cleared out at the sides, so that it may be conveniently taken up when wanted.
- m. The inner surface of the cover hollowed out at n to receive without friction the projecting part of the upper bason. A paper is pasted on the inner surface of the cover, to shew the weight of the gravimeter with or without the additional ballast-piece, and the volume of water it displaces in either case, which are often required to be accurately known.

On the useful Application of the Gravimeter to the Results of Tables of Specific Gravity.

IT is frequently necessary, in philosophical as well as commercial transactions, to determine the proportions of a mixture of two liquids, or an alloy of two metals; and it has been for some time known, that in order to render this operation easy and certain, recourse must be had to tables drawn up from observation with the assistance of an instrument capable of giving the specific gravity to at least three decimals. The gravimeter will perfectly answer this purpose. To shew this more effectually, I shall apply it to the mixture of alcohol and water, and the alloy of tin and lead. These two compositions are precisely what in a commercial point of view most frequently require to be examined in this respect. And I am convinced that an exhibition of the most accurate results of experiments of such extent and minuteness, cannot fail to prove acceptable in this place.

A TABLE

A TABLE of the Specific Gravities of the Mixtures of Alcohol and Water, with the Proportion of these Fluids at the Temperature of 60 Degrees of the Scale of Fahrenheit, 124.44 of Reaumur, 155.5 of the Decimal Thermometer.

Note. I have placed in this table the ratios published by Citizen Chauffier, from the Memoirs of Citizen Gouvenain, at the article ALCOHOL of the Encyclopédie Methodique, and those given in the tables of Mr. Gilpin, which are recommended by the philosophers of Germany. Journ. Phys. de M. Gren, 1796, Tom. III. p. 128.

Centesimal parts of the mixture *.		Specific Gravities.	
		According to Chauffier.	According to Gilpin (last table. N.)
Alcohol	100	0.7980	0.825
	95	0.8165	0.83887
	90	0.8340	0.85244
	85	0.8485	0.86414
	80	0.8620	0.87606
	75	0.87525	0.88762
	70	0.8880	0.89883
	65	0.9005	0.90941
	60	0.9120	0.91981
	55	0.9230	0.92961
	50	0.9334	0.93882
	45	0.94265	0.94726
	40	0.9514	0.95493
	35	0.95865	0.96158
	30	0.96535	0.96736
	25	0.97035	0.97239
	20	0.97605	0.97723
	15	0.9815	0.98213
	10	0.9866	0.98737
	5	0.99335	0.99327
	0	0.99835	1.00000

We shall not be surpris'd at the difference between these two tables, if we attend to the circumstance that the alcohol used by Chauffier was more highly rectified, its specific gravity having been only 0.798 at the same temperature at which the spirit used by Gilpin gave 0.825. But it is not easy to conceive that this difference could produce so considerable a change in the penetration or diminution of volume of the mixtures; that indicated by Gilpin at equal parts of spirit and water being 0.025, whereas, according to Chauffier, it is 0.0454†.

TABLE

* By measure, the water being added as the spirit is diminished, so as to keep the volume constantly = 100.

† For want of that part of the New Encyclopédie which contains the tables of Chauffier, I am not qualified to give an account of the degree of accuracy they may possess. The tables of Gilpin are to be found in the second part of the Philosophical Transactions for 1794. They are comprehended in one hundred quarto pages, and express—for every single degree of Fahrenheit, from 30° to 80° inclusive;—and for all mixtures of ardent spirit and water, formed by the addition of 1, 2, 3, &c. parts by weight of water, as far as 100, to the constant quantity 100 of spirit, at 0.825 specific gravity when at 60;—and for all mixtures formed in like manner by adding

TABLE of the Correspondencies of the Specific Gravities of Alloys of Tin and Lead, with the respective Proportions of these Metals.

In constructing this table, I have made use of the observations published by M. Berghslierna in the Memoirs of the Academy of Stockholm for 1780, and reprinted in the Manuel Systematique of M. Gren (§ 3195). But as he grounded all his ratios on the variations of absolute weight under equal volumes, and made use of foreign weights, I have been obliged to change the expression for the more simple and usual proportion to distilled water, taken as unity.

At every fifth line I have added the mathematical specific gravity, or that which is determined by calculation, in order to shew the changes of bulk which result from the combination, and in this particular case diminish the density instead of increasing it.

The specific gravity of pure lead to water is as 11.603 to 1.

The specific gravity of pure tin is 7.2914 to 1.

Compound Metal.		Mathematical Specific Gravity.	Real Specific Gravity.	Compound Metal.		Mathematical Specific Gravity.	Real Specific Gravity.
Tin.	Lead.			Tin.	Lead.		
99	1	7.3300	7.3252	89	11		7.6468
98	2		7.3552	88	12		7.6787
97	3		7.3871	87	13		7.7106
96	4		7.4189	86	14		7.7425
95	5		7.4508	85	15	7.8717	7.7744
94	6	7.4848	7.4828	84	16		7.8063
93	7		7.5146	83	17		7.8382
92	8		7.5511	82	18		7.8701
91	9		7.5835	81	19		7.9020
90	10	7.6782	7.6149	80	20	8.0652	7.9339

adding 1, 2, 3, &c. parts by weight of the spirit, to the constant quantity 100 of water. (1) The specific gravity to five places of figures; (2) the proportion of water by measure to the spirit taken as 100; (3) the actual bulk of this last mixture; (4) the diminution of bulk by mixture; (5) the quantity of spirit per cent.; and (6) a decimal multiplier, which being applied to the bulk of the mixture gives the measure of the pure spirit in a mixture of that density. Hence it may be inferred how extensively useful this table must prove to all practical philosophers and manufacturers. With regard to its accuracy, I must refer to the ample reports of Dr. (now Sir Charles) Blagden, in the Transactions for 1790, and shall only observe that the experiments were made with the balance of the President of the Royal Society; of the admirable mechanism and sensibility of which, some account, though imperfect, is given in the Journal de Rozier, Vol. XXXIII. and that the term or standard spirit .815 was arbitrarily chosen from motives of convenience and accuracy.

The spirit was obtained from malt. The greatest strength obtained by adding hot caustic alkali to spirit and then carefully distilling, gave a specific gravity of 0.813 at 60 degrees. Perhaps the circumstances of M. Gouvenier's experiments may shew by what means he succeeded in arriving at 0.7980 at the same temperature. No useful purpose would be answered by conjecture in this place. From the last line in the table here given by Citizen Guyton, the specific gravity of mere water is such as to indicate a temperature of 75 degrees instead of 60 in Chauviller's column. In which case Gilpin's standard spirit would have a specific gravity of 0.8178. With regard to the considerable difference in the penetration or diminution of bulk, it remains to be shewn whether it depends upon any part of the processes by which the alcohol was obtained and concentrated, or, as is more probable, upon some mistake in the calculations or methods of expressing the quantities. N.

Compound

Compound Metal.		Mathematical Specific Gravity.	Real Specific Gravity.	Compound Metal.		Mathematical Specific Gravity	Real Specific Gravity.
Tin.	Lead.			Tin.	Lead.		
79	21		7.9658	39	61		9.4727
78	22		7.9977	38	62		9.4355
77	23		8.0296	37	63		9.4788
76	24		8.0615	36	64		9.5221
75	25	8.2586	8.0934	35	65	9.8074	9.5676
74	26		8.1253	34	66		9.6132
73	27		8.1572	33	67		9.6565
72	28		8.1891	32	68		9.7021
71	29		8.2210	31	69		9.7454
70	30	8.4520	8.2529	30	70	10.0010	9.7887
69	31		8.2848	29	71		9.8297
68	32		8.3167	28	72		9.8730
67	33		8.3486	27	73		9.9163
66	34		8.3828	26	74		9.9573
65	35	8.6455	8.4170	25	75	10.1945	9.9983
64	36		8.4511	24	76		10.0416
63	37		8.4853	23	77		10.0871
62	38		8.5195	22	78		10.1350
61	39		8.5537	21	79		10.1806
60	40	8.8397	8.5879	20	80	10.3881	10.2261
59	41		8.6228	19	81		10.2717
58	42		8.6562	18	82		10.3173
57	43		8.6904	17	83		10.3629
56	44		8.7246	16	84		10.4084
55	45	9.0333	8.7588	15	85	10.5799	10.4586
54	46		8.7929	14	86		10.5062
53	47		8.8271	13	87		10.5543
52	48		8.8613	12	88		10.6021
51	49		8.8955	11	89		10.6500
50	50	9.2258	8.9319	10	90	10.7734	10.7001
49	51		8.9729	9	91		10.7479
48	52		9.0139	8	92		10.7958
47	53		9.0550	7	93		10.8414
46	54		9.0960	6	94		10.8869
45	55	9.4203	9.1373	5	95	10.9668	10.9354
44	56		9.1552	4	96		10.9781
43	57		9.2190	3	97		11.0236
42	58		9.2600	2	98		11.0692
41	59		9.3033	1	99		11.1148
40	60	9.6139	9.3466	0	100	11.1216	11.1603

V.

Descriptions of the Improved Air Pumps of PRINCE and CUTHBERTSON; with Observations.

THE important uses of the Air Pump are too many and too well known to require enumeration. It is an engine, not only calculated to demonstrate many of the leading principles in natural philosophy, but indispensable for the performance of some of the most accu-

rate experiments in the Pneumatic Chemistry. The air is exhausted out of a receiver by this instrument, by an action nearly similar to that of the common sucking pump, as it is called. In the common pump there is a bucket, which by leathering is made to slide, water-tight, up and down the pipe; but instead of a bottom, it is provided with a valve or flap opening upwards. By this contrivance the bucket may be moved downwards through a column of water without producing any progressive motion in the fluid, because the valve then opens and affords a free passage; and, on the contrary, when it is moved upwards, it must also move the whole of the column above the flap or valve, which is then shut. Another valve, also opening upwards, is fixed in the pipe below the ordinary range of the bucket, and serves to prevent the return of any fluid which may lie between the two valves during the descent of the bucket itself.

In working this common and very useful engine, it is first necessary to pour a small quantity of water into the pipe above the bucket; after which, the handle being moved produces the following effect: As the bucket descends, the space between the two valves becomes diminished, and the air contained in that space lifts the upper valve, and in part rises with noise through the water; after which, as the bucket rises, and again enlarges the space, the remaining air expands by its spring, which is thus weakened. The air in the pipe beneath the lower valve will therefore act more strongly upwards by its entire spring than the air above does downwards. It will open the valve, and rise up into the upper space; at the same time that the pressure of the external air, upon the water in the well, will drive part of the water after it into the lower part of the pipe. By continuing the work for a few strokes the included air is thus entirely drawn out and succeeded by water, provided the height from the surface of the fluid in the well to the upper valve be not so great as to include a column of upwards of thirty-three feet. For such a column would be sufficiently heavy to counterpoise the whole pressure of the atmosphere, and would accordingly prevent any farther ascent.

If this last-mentioned case were to exist, or if the lower aperture of the pipe were closed, it may without difficulty be understood that in theory, that is to say, with a perfect instrument, the air beneath the lower valve would become more and more rarefied to a certain point as the process was continued. And the progress and extent of this rarefaction would be computable from the known ratios of the greatest and least spaces between the valves when in their extreme positions, together with the assumed conditions that the air, however greatly rarefied, would diffuse itself equally through the containing space, and that the lower valve shall afford no resistance to the power applied to open it when the bucket is elevated. It will also follow, that the same rarefaction might be produced in any vessel containing air, and communicating only with the pipe of the pump beneath the lower valve.

Thus it is seen that the common sucking pump is, in fact, an air pump before it operates as a pump for water; and that nothing more is required to constitute the philosophical air pump than convenience of size, accuracy of workmanship, and, that the resistance at the valves be either diminished or removed.

It would be to little purpose to enter into any discussion concerning the original construction of the air pump, as invented by Otto Guericke, and afterwards improved and most usefully employed by our eminent countryman Robert Boyle. It is generally allowed at present, that such air pumps as operated by means of stop-cocks instead of the lower valve, are

not durable, but soon become leaky. If ever these pumps, of which engravings are to be seen in the works of Muschenbroek and S'Gravensande, were used in England, which I think was not the case, they must have been long superseded by the cheaper and more simple contrivance of valves formed by tying a strip of bladder over a small hole through which the air is allowed to pass in one direction only. Mr. Smeaton was of opinion, that the operation of the air-pump is rendered defective from the small surface of pressure at which the air acts against the bladder of the lower valve, as well as from the pressure of the external air, which is supposed to prevent the upper valve from opening in the last stages of the process. To remedy these imperfections, he made a number of hexagonal holes instead of the one small hole in the valve; and by passing the rod of the piston through a collar of leathers at the top of the barrel, which was closed, with the exception of a valve opening outwards, he prevented the external air from acting upon the valve of the piston in its descent*. Mr. Abraham Brooke of Norwich, by experiments with the common air-pump, has rendered it at least doubtful, whether, in pumps equally well made, these be improvements at all†. But, previous to the time this last author wrote, there existed two other air-pumps, containing improvements both in principle and workmanship sufficient to annihilate any controversy respecting the merits of the common pump, and that of Smeaton. These were made by the Rev. Mr. Prince, of Salem in North America, and Mr. John Cuthbertson of Amsterdam. The first is described in the only volume of Memoirs published by the American Academy, and is scarcely known in the philosophical world. The other is described in a publication‡ by the inventor; of which the circulation must, in the ordinary course of things, be considerably limited. I have thought it my duty to present both to the world in this place.

Mr. Prince's paper contains a considerable portion of matter relative to the air-pump of Smeaton, partly descriptive of the improvements made by that ingenious mechanic, and partly by way of comparison with his own. For the sake of brevity, I have omitted these, and other matters of less consequence; but in the essential parts of the account I have kept as nearly as possible to the words of the author.

From the account of Mr. Smeaton's success in facilitating the opening of the valves at the bottom of the barrel and in the piston, Mr. Prince supposed, if those valves were entirely removed, and the remaining air in the barrel could be more perfectly expelled, the rarefaction might be carried still further. Upon this plan he constructed his pump. He removed the lower valve, and opened the bottom of the barrel into a cistern on which it was placed, and which had a free communication with the receiver. For the valve on the plate at the top of the barrel (which is constructed like Smeaton's) makes it unnecessary, as he remarks, that there should be any at the bottom in order to rarefy the air in the receiver.

The cistern is deep enough to allow the piston to descend into it below the bottom of the barrel. Suppose then the piston to be solid, that is, without a valve in it; when it enters the barrel and rises to the top plate, which is made air-tight with a collar of leathers, &c.

* Philosophical Transactions, XLVII.

† Miscellaneous Experiments and Remarks on Electricity, &c. by A. Brooke. Norwich, 1789. P. 123, and elsewhere.

‡ Description of an improved Air Pump. By John Cuthbertson. Octavo, 41 pages. London; no date. This artist is at present settled in London.

like Smeaton's, it forces out all the air above it; and, as the air cannot return into the barrel on account of the valve on the top plate, when the piston descends, there will be a vacuum formed between that and the plate, every thing being supposed perfect. But in working the pump, the piston is not allowed to descend entirely into the cistern so far as to leave the bottom of the barrel open; because, as the cistern, for another purpose, is made larger than the bore of the barrel, this might make the piston-rod work unsteadily in the collar of leathers, and cause it to leak; but it descends below a hole in the side of the barrel near the bottom, which opens a free communication between the barrel, cistern, and receiver. Through this hole the air rushes from the cistern into the exhausted barrel, when the piston has dropped below it; and by its next ascent this air is forced out as the other was before. If now, the capacity of the receiver, cistern, pipes, &c. below the bottom of the barrel, taken together, be equal to the capacity of the barrel, half the remaining air will be expelled by every stroke.

But as the working a pump of this kind with a solid piston would be laborious, on account of the resistance it would meet with in its descent from the air beneath (though this would be lessened by every stroke as the air became more rarefied), the inventor, to remedy this inconvenience, pierced three holes in the piston at equal distances from each other; and a circular piece of bladder, which is tied over the top of the piston, to make the joint more perfect with the top plate, and to defend them from injury when the piston is brought up against it, forms a kind of valves over the holes, which open easily enough to prevent any labour in working the pump, as it allows the air to pass through the piston when it descends. But the air does not necessarily depend upon a passage through the piston in order to get into the barrel: for when the air becomes so weak from its rarefaction that it cannot open this valve, it will still get into the barrel when the communication is opened by the hole at the bottom. This piston, therefore, will descend as easily as any other; and these valves do not impede the rarefaction, since it is of no consequence, as to this, whether they open or not. By this construction the valves, which Mr. Smeaton only made to open with more ease, are rendered unnecessary in rarefying the air, and that at the bottom of the barrel, which is the most difficult to be made and kept in order, is entirely removed; the valve on the top plate being the only one necessary in rarefying the air.

But as in a single-barrelled pump of this construction, where there is no valve at the bottom to prevent the air, which follows the piston in its ascent, from returning into the receiver in its descent, a fluctuation would be produced which might prove detrimental in some experiments, this pump is made with two barrels, which rarefy the air at every stroke of the winch. In this construction, the capacity of the two barrels taken together below the pistons is always the same; for while one is descending the other ascends, and what is taken from the one is added to the other.

Having thus set aside the valves which in some measure prevented the air from entering the barrel above the piston, he next attempted to expel the air more perfectly out of the barrel than Mr. Smeaton had done, by making a better vacuum between the piston and the top plate, which would allow more of the air to expand itself into the barrel from the receiver.

Upon Mr. Smeaton's plan, our author also contrived to connect the valves on the top plates with the receiver occasionally, by means of a pipe and cock, by the turning of which the machine

machine may be made to exhaust or condense at pleasure. This is done in the following manner: There is a cross piece laid over the valves extending from one barrel to the other, which has a duct through it connected with a small pipe standing between the barrels; through this pipe the air passes into a duct at the bottom piece leading to the cock. In this piece is likewise the duct leading from the cistern to the cock; and with this cock also is connected the pipe leading to the receiver. The key is pierced with two holes, in such a manner, that one of them will connect the pipe coming from the receiver with the duct in the bottom piece leading to the cistern, or with the other leading to the valves, as may be required for exhausting or condensing. The other hole through the key will open, occasionally, to the atmosphere either of these ducts round the cock: so that, having the direction of the air which passes through the valves under the command of this cock, the pump may exhaust or condense at pleasure; for when the key connects the pipe from the receiver and the duct leading to the cisterns together, the pump will exhaust; and when it connects the pipe with the duct leading to the valves, it will condense, as the other hole in the key at the same time opens to the atmosphere the duct leading to the cisterns, by which passage the air enters the barrel from the atmosphere, is forced out at the valves, and through the pipe and cock into the receiver. In this part of the machine which is contrived for condensation, the inventor has, by an additional part, endeavoured to get the air more perfectly out of the barrel.

We have seen, says he, that Mr. Smeaton, by making the piston of his pump fit more exactly to the bottom of the barrel, and by shutting up the top to prevent the pressure of the atmosphere on the piston valve, was able to get more of the air above it than could be effected in the common pump: but still the difficulty, though so far removed, remains in the top of the barrel; for, as the piston cannot be made to fit so exactly to the top plate, but that there will be some lodgement for air, it is impossible to expel it entirely. More perhaps might be expelled if the valve on the top could be made to open more easily, by removing the weight of air from it; for the atmosphere pressing on this valve will prevent its opening freely, in the same manner as, when pressing on the piston valve, it obstructs the opening of that in the common pump.

The difficulty which Mr. Smeaton removed from the piston valves, Mr. Prince endeavoured to remove from the valve on the top plate; that this valve, having the pressure of the atmosphere taken off, might open with the same ease as the piston valve does in his pump. To effect this, there is connected with the duct on the bottom piece which conveys the air from the valves to the cock, a small pump of the same construction as the large one; having the barrel opening into a cistern, the piston rod moving through a collar of leathers, and a valve near the top, through which the air is forced into the atmosphere. This piston is solid: because the diameter being only half an inch, does not make it work hard. This pump, which is of one barrel only, he calls the valve pump; its chief use being to rarefy the air above the valves, or remove the weight of the atmosphere from off them. To use this pump, it is necessary the key of the cock should be pierced differently from that of Mr. Smeaton's; for, as the pipes round his are placed at equal distances, when the one from the bottom of the barrel is connected with that from the receiver to exhaust it, the other from the valve on the top plate is opened to the atmosphere by the other passage through the cock. But in order to rarefy the air above the valve in my pump, it is necessary this last passage should be shut

up when the valve pump is used. Instead, therefore, of placing the three ducts at equal distances round the cock, I have divided the whole into five equal parts; leaving the distance of one fifth part between the ducts leading from the cistern and the valves to the cock, and two fifths between each of these and the one leading from the cock to the receiver. By this adjustment, when the communication is open between the receiver and the valves for condensation, the other hole through the cock opens the cisterns to the atmosphere; but when the communication is made between the cisterns and the receiver for exhaustion, a solid part of the key comes against the duct leading to the valves, and shuts it up, and the air which is forced out of the barrel passes through the atmosphere into the valve pump; for the valve of the small pump may be kept open while the great one is worked.

Upon this construction also we are able to make the pump with two barrels like the common pump, which cannot be done conveniently where the lower valve is retained; because it would be difficult to make the piston in one barrel come exactly to the bottom, at the same time that the piston in the other touched as exactly at the top; it would at least require a nicety in the workmanship which would be troublesome to execute.

In this pump the pistons do not move the whole length of the barrels: there is a horizontal section made in them a little more than half way from the bottom where the top plates are inserted. See Fig. 6. Pl. VI. By this means the pump is made more convenient and simple, as the head of it is brought down upon the top of the barrels in the same manner as in the common air pump. The barrels also stand upon the same plane with the receiver plate, and this plane is raised high enough to admit the common gauge of thirty-two or three inches, to stand under it without any inconvenience in working the pump, as the winch moves through a less portion of an arch at each stroke, than it would if the pistons moved the whole length of the barrels.

There is also placed, between the barrels in this pump, on the cross piece over the valves, a gauge to measure the degree of condensation, having a free communication with the valves, cock, &c. This gauge is so constructed that it will also serve to measure the rarefaction above the valves when the air is worked off by the valve pump. It consists of a pedestal, which forms a cistern for the mercury, a hollow brass pillar, and glass tube hermetically sealed at one end, which moves up and down in the pillar through a collar of leathers. The die of the pedestal is made of glass, as well to hold the quicksilver as to expose its surface to view, that it may be seen when the open end of the tube is put down into it or raised out of it. The body of the pillar is partly cut away to expose the tube to view in the same manner.

If the pump be used as a condenser, the degree of condensation is shewn by a scale marked on one edge of the pillar: if it be used as an exhauster, the degree of the rarefaction of the air above the valves is shewn by a scale marked on the other edge of the pillar.

This gauge will also serve to shew when the valves have done playing, either with the weight of the atmosphere on them or taken off. If we want to know when they cease opening with the weight of the atmosphere on them, draw the piston of the valve pump into its barrel, to prevent any air escaping through that valve: in this situation work the great pump again; and if any air passes through the valves into the pipe, the gauge will rise by condensation. This condensed air must then be let out by opening the communication at the

cock with the outward air. By repeating this till the gauge rises no longer, we may know the valves will open no more while the weight of the atmosphere lies on them; and the rarefaction in the receiver can be carried no further. When the weight of the atmosphere is to be removed, after conducting as in the former experiment, raise the open end of the tube above the surface of the mercury, and then work the valve pump, and the air will be rarefied over the valves and in the tube to the same degree (we may see when the valve of this pump has done playing, by unscrewing the cap that covers it). The open end of the tube is then to be immersed into the mercury, and the great pump worked. The air which passes through the valves will then raise the gauge by condensation; and thus by alternately raising and depressing the tube, and working the two pumps in their turns, we may carry the rarefaction of the air in the receiver as far as the power of the pump will go. If one of Mr. Smeaton's pear gauges be used in the receiver, as he directs, the difference of the rarefaction in the two experiments may be known. And as the air above the valves may be rarefied to different degrees, it may be known, by the two gauges, what proportion the rarefaction above the valves bears to the degree of excess in the receiver. This condensing gauge can be taken off, and a button screwed into the hole in its stead, in any case wherein a greater degree of condensation is required than the glass will bear. When a glass receiver is used, this gauge may be placed within it, where it will measure any degree of condensation the receiver will bear without danger to the gauge; or the capacity of any receiver may be measured by this gauge before it is removed from its place, by shewing how many strokes of the winch will throw one atmosphere into the receiver; then turning the cock to prevent any air escaping, change the gauge for the button. When this is done, the degree of condensation may be further measured by the number of strokes.

As in cases where great condensation is required, there must be a great deal of labour, and a great strain on the teeth of the wheel and piston rods, on account of the great diameter of the pistons*; to remedy this, Mr. Prince has fitted a condenser of a smaller bore than the barrel of the great pump to the cistern of the valve pump, to be screwed on occasionally, by which the condensation may be finished, instead of the great pump. Or, to save the work and expence of this condenser, the valve pump, if made a little larger, may be easily fitted for the same purpose, by having a plate made to screw into the bottom of the cylinder occasionally, with a valve on it opening into the cistern: a hole must also be made to be opened on the same occasion near the top of the cylinder, to let air in below the piston when this is drawn up above it.

The common gauge, which is generally placed under the receiver plate in this pump, is placed in the front, that it may be seen by the person who is working the pump, and that the place may be left free for other uses.

The plate is so fixed to the pipe leading to the cock, that it may be taken off at pleasure, and used as a siphon; or any tube or apparatus may be fixed to it to perform some experiments without removing it, which will save trouble and make less apparatus necessary.

* In this pump the pistons are two inches diameter; so that there will be about forty-eight pounds added to the resistance in opening the valves for every atmosphere thrown into the receiver. P.

It is certainly an injury to an exhausting pump to use it for condensing. This operation ought always to be performed with a long syringe of small bore. Such an instrument would cost much less than the condensing part of an air pump, and will at the same time have much more power. N.

The head of this pump is not divided as the common one is, to dislodge the teeth of the wheel from the piston rods when the pump is to be taken apart, but is made whole, except a small piece in the back, where the wheel is let in; which makes it much more convenient to remove the head, or place it on the barrels. The wheel is freed from the piston rods when required, by pushing it into the back part of the head; and when it is drawn into its place and connected with them again, a button is screwed into the socket of the axis behind, to keep it in its place. This makes the head less troublesome to remove; but its chief use is to dislodge the piston rods from the wheel, that they may be put down into the cisterns when the pump is not in use, where they will stand uncompressed, and retain their elasticity better than if kept in the barrels. In these cisterns they may also stand covered with oil, if necessary, as they are large enough to admit of it.

The principal joints of the pump are sunk in sockets, that the leathers which close them may be covered with oil to prevent leaking*.

For convenience, the lower part of the pump is fitted with drawers to contain the apparatus. A door opens behind one range to a place reserved the whole height, to get at the under part of the receiver plate, and fix apparatus to it for some experiments. In this place stand the long tubes, and such tall glasses belonging to the apparatus as will not go into the drawers. The barrels, &c. of the pump are covered with a case or head, which keeps them from dust and accidents when the pump is not in use. The apparatus is secured between sliders, &c. in the drawers, so that the whole machine may be easily removed in one body, without danger.

As there was no glass manufactory in Mr. Prince's vicinity, he sent to Europe for his apparatus; but unluckily the gauges, with some other parts, had not been forwarded to him when he wrote his account. He had only a small tube of two-tenths inch bore, which he used as a common gauge; but this was not sufficient to determine the power of the pump.

All that he could say of the instrument was, that he found it much more convenient to use than one of the common sort; that it would exhaust a receiver much sooner, and keep in order much longer, for being made without valves, which must depend on the spring of the air to open them. When a common pump in his possession had been fitted up with valves, leathers, &c. at the same time with this, the valves of the common pump have become too dry and stiff to use, while this pump has continued in good order. He attributes this, in part, to the moisture which the valves on the top plates receive from the pistons every time the pump is used; the pistons being always kept moistened with oil in the cisterns, where they stand when the pump is not in use; and in part to the power which the pistons have over these valves, by condensing the air against them.

Fig. 6, Plate VI, represents a perpendicular section of one of the barrels, the two cisterns, condensing gauge, &c. where AB represents the barrel; CD is the cistern on which it stands; a a a the leathered joint, sunk into a socket and buried in oil; EF is the piston; the cylindrical rod passing through a collar of leathers, GG, in the box HI. K shews the place of the valve on the top plate K, covered by the cross piece MM, into which the pipe OO

† This, I find, is very effectual; having never known one of the joints secured in this way to leak, though the pump has stood for a long time; whereas a portable pump which I have, made by Mr. Nairne, London, has leaked, and repeatedly been refitted with new oiled leathers in the same time. P.

is soldered, that conveys the air from the valves to the duct going under the valve pump, as may be seen in Figure 8; o is part of the said duct; p is the joint sunk into a socket in the cross piece Pl', which connects the cisterns, and has a duct through it leading to them. Into this duct open the ducts q and r, the first leading to the gauge in front of the pump, the other to the cock and receiver.

The other barrel is left out of the figure to shew some of the parts more distinctly, except Q, which is the top of the barrel retained and brought down out of its place to shew the top plate that shuts up the barrel, separated from the box which contains the collar of leathers. S shews one of the holes in the plate over which the valve lies, and which is covered by R in the cross piece. VV is the piston shewing the valve open on the top, which is to prevent labour when the pump condenses. WX is the cistern, in which is more distinctly seen the shoulder for the leather which closes the point between this and the barrel, and also the socket in which the oil lies over the leather. YZ is the condensing gauge, with the orifice of the tube raised above the surface of the quicksilver. ee is the collar of leathers, through which the glass tube moves. i is a small pipe coming up through the quicksilver to make a communication between the valves and the gauge.

Fig. 7. is a view of the upper surface of the top plate which closes the barrel, being soldered into it, shewing the place of the valve over the three small holes, one of which only can be seen at S in Fig. 3.

Fig. 8. is a perpendicular section of the bottom piece, pipes, valve pump, cock, &c. at right angles with the other section, Fig. 6. AB is the pipe between the barrels, as represented in Fig. 6. The button o is here screwed into the top instead of the gauge. CD is the valve pump and its cistern, e the place of the valve under the cap. EF the cock, shewing the duct through it leading to the atmosphere. GH the pipe leading from it to the stem of the receiver plate, in which is the cock I, to shut up the duct when the plate is used as a transferer. KK is the plate. L, a piece to shut up the hole, into which tubes &c. are occasionally screwed to perform experiments without removing the plate. The dotted line at O shews the place of the screw which presses the plate against the pipe: PQ the pipe and common gauge standing in front of the pump.

Fig. 9. is a horizontal section of the cock, and pieces containing the ducts leading from it to the receiver, the cisterns, and the valves on the top of the barrels. AB the duct connecting the cisterns together. CD the duct leading from the cisterns to the cock. GH the duct leading from the cock through the pipe AB (Fig. 8.) to the valves. DE the duct through the cock, which occasionally connects the two last mentioned ducts with the duct EF, leading from the cock to the receiver. I, the duct in the cock leading to the atmosphere, which, when connected with the duct at D, lets the air into the cisterns and barrels for condensation; the other duct through the cock at the same time connecting H and E. This duct also, when connected with E, restores the equilibrium in the receiver. KL is part of the duct leading from the cisterns to the gauge. The dotted circles shew the places of the pipe and valve pump on the piece, and r the place where the air enters the valve pump from the duct GH, and is thrown into the atmosphere when the pump exhausts.

Fig. 10. shews the under surface of the boxes which contain the collars of leathers with the cross piece which connects them together, having a duct through it, as represented by the dotted

dotted line, through which the air passes from the valves to the pipe. This Fig. is designed chiefly to shew the places in which the valves play, as at I.

Fig. 11. is a side view of the pump, shewing the situation of the valve pump and handle of the cock; where A is the pump, and B the handle.

Fig. 12, Plate VII. is a perspective view of Cuthbertson's pump, with its two principal gauges screwed into their places: these need not be used together, except in cases where the utmost exactness is required; for in common experiments either of the two may be taken away, and a stop screw put into its place. When the pear gauge is used, a small round plate, large enough for the receiver to stand upon, must first be screwed into a hole at A; but when this gauge is not used, this hole must be closed with a stop screw. When all three gauges are made use of, and the receiver is exhausted, the stop screw B, at the bottom of the pump, must be unscrewed, to admit the air into the receiver; but when the gauges are not all used, the stop screw at A, or either of the other two which are in the place of the gauges, may be unscrewed for this purpose.

In Figure 1, CD represents one of the barrels of the pump, F the collar of leathers, G a hollow cylindrical vessel to contain oil; R is also an oil vessel, which receives the oil that is driven with the air through the hole a a, when the piston is drawn upwards; and when this is full, the oil is carried over with the air along the tube T into the oil vessel G. c c is a wire which is driven upwards, from the hole a a, by the passage of the air; and as soon as this has escaped, falls down again by its own weight, shuts up the hole, and prevents any air from returning by that way into the barrel: at d d are fixed two pieces of brass, to keep the wire c c in such a direction as may preserve the hole air-tight. H is a cylindrical wire which carries the piston I, and is made hollow to receive a long wire q q that opens and closes the hole L, which forms the communication with the receiver standing on the plate. m is part of a pipe, one end of which is screwed into L, and the other into the centre of the receiver plate. M is a stop screw, serving only to close that hole. OP is a small steel screw, one end of which is screwed into the wire q q, that opens and shuts the hole L; and upon the other end O is screwed a nut, which, stopping in the smallest part of the hole, prevents the wire from being lifted or carried too high. This wire and screw are more clearly shewn in Figure 2 and in Figure 6; they slide through a collar of leathers r r, Figure 2 and Figure 5, in the middle piece of the piston. Figures 4 and 5 are the two main parts which compose the piston; and when the pieces in Figures 3 and 6 are added to it, the whole is represented by Figure 2. Figure 5 is a piece of brass, turned in a conical form, with a shoulder or ledge at the bottom; a long female screw is cut in it, about two thirds of its length; and the remaining part of the hole, in which there is no screw, is about the same diameter as the screw part, except a thin plate at the end, which is of a width exactly equal to the thickness of q q. That part of the inside of the conical piece of brass, in which no thread is cut, is filled with oiled leathers with holes in them, through which q q can slide air-tight; there is also a male screw with a hole in it, which is fitted to q q, and serves to press down the leathers r r. In Figure 4, a a a is the outside of the piston, the inside of which is turned exactly to fit the outside of Figure 5. b b are round leathers, about 60 in number; c c is a circular plate of brass, of the size of the leathers; and d d is a screw, which serves to press them down as tight as is necessary. The male screw at the end of Figure 3, is made to fit the female screw in Figure 5: now if Figure 6 be pushed into Figure 5, this into Figure 4, and Figure 3 screwed

screwed into the end of Figure 5, these will compose the whole piston, as represented by Figure 2. H in Figure 1 represents the same part as H in Figure 2, and is that to which the rack is fixed. If this therefore be drawn upwards, it will make Figure 5 shut close into Figure 4, and drive out the air above it; and when it is pushed downwards, it will open as far as the shoulders a a Figure 4 will permit, and suffer the air to pass through. A A Figure 7, is the receiver plate; B B is a long square piece of glass screwed to the undermost side of the plate, through which a hole is drilled, corresponding with that in the centre of the receiver plate, and with the three female screws b b c.

Suppose the piston to be at the bottom of the barrel, and a receiver to stand upon the plate, the inside of the barrel, from the top of the piston to a, is full of air, and the piston shut; when drawn upward, by the hollow cylindrical wire H, it will in its course drive the air before it, through the hole a a, into the oil-vessel R, and out into the atmosphere by the tube T. The piston will then be at the top of the barrel at a, and the wire q q will stand nearly as it is represented in the figure, just raised from the hole L, and prevented rising higher by means of the nut o. While the piston is moved upwards, the air will expand in the receiver, and be driven along the bent tube m into the inside of the barrel. Thus the barrel will be filled with air, which, as the piston rises, will be rarefied, in proportion as the capacity of the receiver pipes and barrel is to the capacity of the barrel alone. When the piston is moved downwards again by H, it will force the conical part, Figure 5, out of the hollow part, Figure 4, as far as the shoulders a a: Figure 2 will rest upon a a Figure 4, which will then be so far open as to permit the air to pass freely through it, while at the same time the end of q q is forced against the top of the hole, and closes it in order to prevent any air from returning into the receiver. Thus the piston, while moved downwards, suffers the air to pass out between the sides of Figure 4 and 5, and when it is at the bottom of the barrel will have the column of air above it; consequently, when drawn upward, it will shut and drive out this air, and by opening the hole L give a free passage to more air from the receiver. This process being continued, the air will be exhausted out of the receiver as far as its expansive power will permit; for in this machine there are no valves, as in the common air pumps, to be forced open by the air in the receiver, which, when its elasticity is diminished, it becomes unable to effect; but every thing is contrived to open by the motion of the piston, and there is nothing to prevent the air from expanding to its utmost degree.

In exhausting with this machine, no other directions need be observed, but such as are common to all air pumps; nor is any peculiar care required to preserve it in order, except that the oil vessel G be always kept about half full of oil. When it has stood a considerable time without being used, it will be proper to draw a table-spoonful or two through it, by pouring it into the hole in the middle of the receiver plate when the piston is at the bottom of the barrel; then, by moving the winch backward and forward, to raise and depress the piston, the oil will be drawn through all the parts of the machine; and what is more than is necessary in the inside will be forced out through the tube T into the oil vessel G. Near the top of the cylindrical wire H is a square hole, which is intended to let in some of the oil from the vessel G, that the oiled leathers through which the wire q q slides, may always be sufficiently supplied with it.

When the pump is required to condense, either at the same time that it exhausts, or separately, the piece which contains the bent tube T must be taken away, and Figure 8 put into

its place, and fastened by the screws which had before confined that piece. Figure 8 is drawn in the plate as it is made for a double-barrelled pump; but for a single barrel only one piece is used, represented by *b a a*, the double piece being cut off at the dotted line *a a*. In this piece is a female screw, to receive the end of a long brass tube; to which a bladder, if sufficient for the experiment, must be tied; or else a glass, properly confined for this purpose, must be screwed to it. Then the air, which is exhausted out of a receiver standing on the plate, will be forced into the bladder, or glass connected with the brass tube. But if the pump be double-barrelled, the apparatus, as represented by Figure 8, must be used, and the long brass tube screwed into the female screw at *C*.

Figures 9 and 10 represent the two gauges; namely, the syphon gauge, and the barometer or long gauge. When these are used, Figure 9 must be screwed into the female screw *c b*, or into that at the other end *c* Figure 7, and Figure 10 into the female screw *a b* Figure 7.

If it be used as a single air pump, either to exhaust or condense, the screw *K*, which fastens the rack to the cylindrical wire *H*, must be taken out; then turning the winch till this wire is depressed as low as possible, the machine will be rendered fit to exhaust as a single air pump: and if it be required to condense, the directions given in the last paragraph but one, with regard to the bent tube *T*, and Fig. 8, must be observed.

Mr. Cuthbertson's treatise contains a relation of various experiments made with this air pump, which shew its great power of exhausting, and are in other respects entitled to the consideration of philosophers. With the double syphon gauge, and also with the long gauge, compared with an attached barometer, in which the mercury had been repeatedly boiled, the difference between the heights of the mercurial columns proved to be no more than one fortieth of an inch, the barometer then standing at 30 inches. This gives an exhaustion of 1200 times. On some occasions, when the air was in a very dry state, he observed the difference to be as low as one hundredth of an inch, which indicates more than double the rarefaction.

On a review of these improvements of the air pump, I perceive in each instrument so happy a combination of philosophical acuteness and mechanical skill, that it is with a degree of diffidence that I venture to speculate on their respective merits and blemishes. There is no provision to open the upper fixed valve of Prince's greater barrel, except the difference between the pressures of the elastic fluid on each side of the strip of bladder; and this may reasonably be inferred to limit the power of his small pump. In Cuthbertson's pump, the same valve is exposed to the action of the atmosphere, together with that of a column of oil in the oil vessel. The mischief in either instrument is probably trifling; but in both the valve might have been opened mechanically. If this were done, the small pump of Prince might perhaps be unnecessary in most states of the atmosphere. With regard to the lower valves, Cuthbertson, by an admirable display of talents as a workman, has insured their action. Prince, on the other hand, has, by the process of reasoning, so far improved the instrument that no valves are wanted. In this respect he has the advantage of simplicity and cheapness, with equal effect. The mechanical combination of Cuthbertson's pump reduces the operation to one simple act of the handle: but Prince's engine requires some manipulation with regard to the play of the small pump; though this might have been remedied by a more skilful disposition of the first mover.

The most perfect scheme for an air pump, taking advantage of the labours of these judicious operators, seems to be that in which two pistons of the construction of Prince should work in one barrel; one piston being fixed at the lower end of the rod, and the other at the middle. The lower piston must come clear out of the barrel when down, and work air-tight through a diaphragm at an equal distance from the effective ends of the barrel. In the diaphragm must be a metallic valve of the form of Cuthbertson's lower valve, but with a short tail beneath, that it may be mechanically opened when the piston comes up. Above the diaphragm, must work the other piston similar to the first; but as it cannot quit the barrel when down, a small portion of the barrel must be enlarged just above the diaphragm, so that the leathers may be clear in that position. Lastly, the top of the barrel must be closed and fitted with a valve and oil vessel, according to the excellent contrivance of Cuthbertson.

If we suppose the workmanship of such a pump to leave the space between the diaphragm and lower piston, when up, equal to one-thousandth part of the space passed through by the stroke of that piston, the rarefaction produced by this part of the engine will in theory bear the same proportion to that of the external air. And the same supposition applied to the upper piston would increase the effect one thousand times more. Whence the rarefaction would be one million times. How far the practical effect might fall short of this from the imperfections of workmanship, or the nature of the air, which in high rarefactions, may not diffuse itself equally through the containing spaces, or from other yet unobserved circumstances, cannot be deduced from mere reasoning without experiment.

VI.

Useful Notices respecting various Objects.—A Method of preventing Heat in Grinding—Concerning Gold, Silver, and other Metals reduced into very thin Leaves by the Hammer—Globules for Microscopes—On the Plumb-Line, and Spirit-Level.

I. On the Art of Grinding.

A CURIOUS fact was mentioned to me, some years ago, respecting grinding, which promises to be of some use in the arts. Daily experience, as well as philosophical experiment, shews us that heat is produced or developed by friction. The fact of sparks flying from a dry grindstone when a piece of iron or steel is applied to its surface during the rotation, has been seen by every one. The heat produced during this process is such that the steel very soon becomes ignited, and hard tools are very frequently softened and spoiled, for want of care during the grinding. When a cylindrical stone is partly immersed in a trough of water, the rotation must be moderate and the work slow, otherwise the water would soon be thrown off by the centrifugal force; and when this fluid is applied by a cock from above, the quantity is too small to preserve the requisite low temperature. It is even found, that the point of a hard tool, ground under a considerable mass of water, will be softened if it be not held so as to meet the stream; sparks being frequently afforded even under the water. My informant assured me, that fine cutlery is ground in Germany on a cylinder of a peculiar kind of pottery instead of stone, upon the face of which pul-

verified hone is occasionally applied by means of tallow. The peculiar advantage of this kind of pottery was stated to be, that it never heats, however rapid the motion.

This object seemed to deserve an experimental investigation. The three bodies subjected to experiment were pottery, pulverised flint, and tallow. The effect, namely that it could support violent and rapid friction without increase of temperature, appeared at first scarcely credible. The mind was rather disposed to reject the evidence, than investigate the cause. No indication was given respecting the nature of the pottery. It did not seem probable that any peculiarity in the siliceous sand should produce this desirable effect; but an easy process of reasoning might have pointed out the effect of the tallow, which is indeed curious. I cannot however assume the merit of investigating the subject *a priori*; for my apparatus was ready for experiment, and the fact spoke for itself before I had systematised the notions which occurred to me.

The pottery grindstone was not easily attainable. I therefore procured a Newcastle grindstone of a fine grit and ten inches in diameter; and also a block of mahogany to be used with emery on its face. Both the stone and the wooden block were mounted on an axis to be occasionally applied between the centres of a strong lathe. In this situation both were turned truly cylindrical, and of the same diameter. The face of the wood was grooved obliquely in opposite directions, to afford a lodgement for the emery. The face of the stone was left smooth, and there was a trough of a proper size applied beneath the stone to hold water. The grindstone was then used with water, and the wooden cylinder was faced with emery and oil. The instrument ground was a file, out of which it was proposed to grind all the teeth. The rotation was produced by the mechanism of the lathe; the velocity being such as to turn the grinding apparatus about five revolutions in a second. The stone operated but slowly, and the water from the trough was soon exhausted, with inconvenience to the workman, who could scarcely be defended from it but by slackening the velocity. The emery cylinder cut rather faster. But notwithstanding the friction was made to operate successively and by quick changes on the whole surface of the file, it soon became too much heated to be held with any convenience; and when a cloth was used to defend the hand, the work not only became awkward, but the heat increased to such a degree that the oil began to be decomposed, and emitted an empyreumatic smell. The stone was then suffered to dry, and the file tried upon its face. It almost immediately became blue, and soon afterwards red-hot. Both the cylinders were then covered with tallow, by applying the end of a candle to each while revolving, and emery was sprinkled upon the cylinder of wood. The same tool was then applied to the grindstone in rapid motion. At the first instant the friction was scarcely perceptible; but very speedily afterwards the zone of tallow pressed by the tool became fused, and the stone cut very fast. The tool was scarcely at all heated for a long time; and when it began to feel warm, its temperature was immediately lowered by removing it to a new zone of the cylinder. The same effect took place when the experiment was repeated with the wooden cylinder.

It is not difficult to explain this by the modern doctrine of heat. When oil was used upon the wooden cylinder, the heat developed by the friction was employed in raising the temperature of the tool and of the fluid oil. But when tallow was substituted instead of the oil, the greatest part of the heat was employed in fusing this consistent body. From the increased capacity of the tallow, when melted, this heat was absorbed, and became latent,
instead

instead of being employed to raise the temperature : and whenever, by continuing the process, the tallow already melted began to grow hot, together with the tool, it was easy to reduce the temperature again by employing the heat on another zone of consistent tallow. I used these two cylinders with much satisfaction, in a considerable quantity of work.

In this stage of experiment, I concluded that the cylinder of pottery mentioned to be used in Germany was either of no particular utility beyond that of a common grindstone, or that the report might be inaccurate in this respect. But it happened that the small stone here mentioned was laid aside for about three years. At the end of this term it was again brought into use, and it was found that the tallow had undergone some change, either from the stone itself, or the action of the external air, which enabled it to defend the grit much more effectually than at first. It seemed to have become less fusible. I think it probable that this might not have happened with a pottery cylinder, or at least that it would have been more easy to clean and restore the surface to its first state.

2. *Concerning Gold, Silver, and other Metals reduced into very thin Leaves by the Hammer.*

IT is generally thought by chemists, and others, that leaf gold consists of the metal in a high state of purity. It is never pure, because pure gold is too ductile to be worked between the gold-beaters' skin. The newest skins will work the finest gold, and make the thinnest leaf, because they are the smoothest. Old skins, being rough or foul, require coarser gold. The finer the gold, the more ductile ; inasmuch that pure gold, when driven out by the hammer, is too soft to force itself over the irregularities, but would pass round them, and by that means become divided into narrow slips. The finest gold for this purpose has three grains of alloy in the ounce, and the coarsest twelve grains. In general the alloy is six grains, or one-eightieth part. That which is called pale gold contains three pennyweights of silver in the ounce. The alloy of leaf gold is silver, or copper, or both, and the colour is produced of various tints accordingly. Two ounces and two pennyweights of gold is delivered by the master to the workman, who, if extraordinarily skilful, returns two thousand leaves, or eighty books, of gold, together with one ounce and six pennyweights of waste cuttings. Hence one book weighs 4.8 grains ; and as the leaves measure 3.3 inches in the side, the thickness of the leaf is one two hundred and eighty-two thousandth part of an inch.

Silver leaf is said to be pure silver. It is extensible in this way when compared with gold, rather more than in the proportion of the specific gravities. Some leaf silver which I tried was thicker than the gold in the proportion of seven to four. So that the weight of metal covering equal surfaces approached to equality.

The yellow metal called Dutch gold is fine brass. It is said to be made from copper-plates, by cementation with calamine, without subsequent fusion. Its thickness, compared with that of leaf gold, proved as 19 to 4, and under equal surfaces it is considerably more than twice as heavy as the gold.

The Dutch silver appears to be tin, not only from its habitudes with re-agents, but from the consideration that there is no other cheap white metal of sufficient ductility. It is somewhat more than ten times as thick as gold leaf, and about two and a half times as heavy under equal surfaces.

The thinnest tin-foil in small sheets for silvering looking-glasses, is about one-thousandth of an inch thick, or near three hundred times the thickness of gold leaf. It usually contains lead.

3. *Globules for Microscopes.*

AT the beginning of the present century, the simple microscope was very much used. Among other advantages, it possesses the very desirable requisites of simplicity and cheapness. In particular it is an instrument not difficult to be constructed by such ingenious philosophers as by narrow circumstances and remote situations are obliged to have recourse to their own skill and ingenuity for experimental implements. The history of natural philosophy abounds with instances of eminent men who come under this description. To these at least it will be of importance to know a ready method of forming very bright spherules of glass for microscopic uses. The usual method has been to draw out a fine thread of the soft white glass called crystal, and to convert the extremity of this into a spherule by melting it at the flame of a candle. But this glass contains lead, which is disposed to become opaque by partial reduction, unless the management be very carefully attended to. I find that the hard glass used for windows seldom fails to afford excellent spherules. This glass is of a clear bright green colour when seen edgewise. A thin piece was cut from the edge of a pane of glass less than one-tenth of an inch broad. This was held perpendicularly by the upper end, and the flame of a candle was directed upon it by the blow-pipe at the distance of about an inch from the lower end. The glass became soft, and the lower piece descended by its own weight to the distance of about two feet, where it remained suspended by a thin thread of glass about one five-hundredth of an inch in diameter. A part of this thread was applied endways to the lower blue part of the flame of the candle without the use of the blow-pipe. The extremity immediately became white-hot, and formed a globule. The glass was then gradually and regularly thrust towards the flame, but never into it, until the globule was sufficiently large. A number of these were made, and, being afterwards examined by viewing their focal images with a deep magnifier, proved very bright, perfect, and round.

4. *On the Plumb-Line and Spirit-Level.*

THE spirit-level is an instrument substituted instead of the plumb-line for determining the level. It seldom happens that wire can be procured so fine as the one-thousandth of an inch in diameter. Upon examining this with the microscope, it scarcely ever proves truly cylindrical. It may with reason be supposed that wire, from its curl upon the bobbin, and its elasticity, may not, even when loaded with a considerable weight, form an accurate right line. As the radius of a circle is equal to 206265 seconds of measure nearly, the diameter of such a wire would cover a space on the line equal to 206 seconds on an instrument of one inch radius, or 17 seconds if the radius were 12 inches. How far this quantity, in the bisection of two dots, of near half a minute broad, may be affected by the causes of error here mentioned, must be ascertained, in each particular instance, by careful experiments. But these considerations, joined to the circumstance that the spirit-level is somewhat more portable and easy of application, have tended to bring this last very delicate instrument into estimation with astronomers.

The

The spirit-level consists of a tube of glass nearly filled with the purest ardent spirit, or with highly rectified ether. In order that this instrument may answer the intended purpose, it is necessary that the inner surface should not be cylindrical or straight on that side which is placed uppermost when in use. Suppose this tube to be laid nearly in the horizontal position; the spirit will occupy the lower space, and the empty part above will exhibit the appearance of a bubble, which will run to the higher end. It follows therefore, supposing the tube to be slightly curved, and the convexity uppermost, that the bubble will be near the middle when the tube is level. In the portable spirit-level, the tube is properly set in brass, and fixed by means of screws in a small brass trough, the bottom of which is ground very straight. The screws are useful to place the bubble in such a position that the lower surface of the trough may be parallel to a tangent supposed to be applied to the middle point of the curve of the level. The adjustment is effected without much difficulty, by placing the level on an adjustable plane, and then reversing it. If the bubble stand accurately in the same position between two marks made on the tube in both situations of the level, it follows that neither end of the plane nor of the lower surface of the frame of the level is elevated; or in other words, that every surface to which the level may be applied, and on which the bubble stands in the position here mentioned, is horizontal.

This easy praxis may be effected in various ways, according to the nature and figure of the instrument of which the position is to be determined: but the accuracy of the result will depend upon the sensibility of the level; that is to say, the space passed over by the bubble for every minute or second of the quadrant, and the certainty with which, under circumstances precisely similar, it shall arrive at the same position. In the best levels the curve must be circular; for in such the bubble will move with more activity, settle itself with more certainty, and describe equal spaces by equal changes of inclination. An ordinary good spirit level will exhibit a movement of upwards of half an inch for each minute of inclination, and alter the position of its bubble by a change of five seconds, or less. In such a tube the radius of the curve will be about 150 feet. But extraordinary levels are much more delicate. De la Lande* speaks of a level filled with ether, the bubble of which passed over fourteen inches by equal spaces of one-tenth of an inch for every second. The radius of this curve was consequently 1719 feet, or near one-third of a mile.

The tubes of spirit-levels are selected by trial. If a long piece of tube be nearly filled with ardent spirit, and corked at the ends, the run of the bubble may be tried with a suitable instrument called the level-trier, throughout the whole length on all sides. By this means it may be known whether, and in what parts, it may be desirable to divide the tube for the purposes of filling and closing. It is remarkable that these tubes in general prove either good throughout, or good for nothing; for it seldom happens, where one good level can be taken, that the remainder is unserviceable. A respectable mathematical instrument-maker assures me, that he finds it a good practice to go to the glass-house and cause the tubes to be drawn without suffering them to be turned round. The usual practice in drawing tubes is, that two workmen extend a piece of blown glass, while hot, by drawing the ends asunder; while a third cools the thinner parts, by blowing the air against it with a hat. To prevent the glass from bending downwards by its own weight, the workmen are in

* In his *Astronomie*; not now at hand. I speak from memory; but have no doubt of the numbers, nor of the fact.

the habit of turning the iron pipes they hold in their hands, both the same way, as they walk backwards. This is the practice which, by producing undulations and irregularities in the tube, is supposed to render them unfit for spirit-levels. I have remarked, that such tubes as have veins, or extended bubbles, running lengthways very straight through the substance, have afforded good levels.

But the most regular and accurate levels are obtained by grinding the inside of the tube. I do not know that this operation requires any particular management capable of being described in words; though there is no doubt but in this, as well as in every other mechanical process, facility and skill will be acquired by practice. A cylindrical piece of wood is turned so as to go easily through the portion of tube intended to be ground. It is then worked in the tube with water and fine emery in the usual way. As soon as the polish has by this means disappeared on one side, the tube is cleaned, filled, and tried; and accordingly as its figure proves to be more or less straight or curved, the grinding is either repeated or discontinued. Some operators polish the inside again after grinding; but this has not been found to increase their sensibility.

From the great delicacy of the spirit-level, compared with the few observations here presented on the plumb-line, the former instrument may appear greatly to deserve the preference. Astronomers are not however agreed on this point. When a spirit-level is adjusted by reversing, at a certain temperature, and both ends of the bubble marked, it may be allowed that the instrument may be successfully applied to use. But if the temperature be raised, the spirit will expand, and of course the bubble will become shorter. Whence it appears necessary that a division and adjusting-piece should be applied, from experiment, to ascertain the true station of the bubble at different temperatures; and even this application seems scarcely adequate to supply the place of repeated adjustments. The variation of the bubble will differ according to the quantity of spirit contained in the tube. In two good levels, of nearly the same magnitude and figure, I found it amount to one-fifth of an inch for every ten degrees of Fahrenheit. The bubble therefore may be one inch longer in winter than in summer, which in these individual levels amounts to near one-third of the summer length. The curvature of a spirit-level will also vary from unequal temperature; such, for example, as may arise from one end of the tube being touched or breathed upon, while the other end is left at the original temperature. The error from each of these causes may amount to several minutes, as is easily shewn by trial; but I do not find that the presence or absence of sunshine causes any perceptible difference. It is probable that the rays may not speedily alter the temperature, on account of the transparency. And with regard to these three last sources of error, it must be allowed that they are easy to be avoided, and indeed not likely to be present in the operations of accurate observers.

MATHEMATICAL CORRESPONDENCE.

QUESTION I. Answered by the Proposer.

LET a be the length of the whole line, and x, y, z, v, w , &c. the parts into which the half of it is to be divided. Then $\frac{a}{x} = \frac{a-x}{y} = \frac{a-x-y}{z} = \frac{a-x-y-z}{v}$ &c. by the question: Or $y = \frac{x}{a}(a-x)$, $z = \frac{x}{a}(a-x-y) = \frac{x}{a}(\overline{a-x} - \frac{x}{a} \times \overline{a-x}) = \frac{x}{a}(\overline{a-x} \times 1 - \frac{x}{a}) = \frac{x}{a}(\overline{a-x} \times \frac{a-x}{a}) = \frac{x}{a^2}(a-x)^2$, $v = \frac{x}{a}(a-x-y-z) = \frac{x}{a}(\overline{a-x} - \frac{x}{a} \times \overline{a-x} - \frac{x}{a^2} \times \overline{a-x}^2) = \frac{x}{a} \times \overline{a-x} (1 - \frac{x}{a} - \frac{x}{a^2} \times \overline{a-x}) = \frac{x}{a} \times \overline{a-x} (\frac{a-x}{a} - \frac{x}{a^2} \times \overline{a-x}) = \frac{x}{a} \times \overline{a-x} (\frac{a \times \overline{a-x} - x \times \overline{a-x}}{a^2}) = \frac{x}{a^3} \times \overline{a-x}^2 \times (a-x) = \frac{x}{a^3} \times \overline{a-x}^3$, $w = \frac{x}{a}(a-x-y-z-v) = \frac{x}{a}(a-x - \frac{x}{a} \times \overline{a-x} - \frac{x}{a^2} \times \overline{a-x}^2 - \frac{x}{a^3} \times \overline{a-x}^3) = \frac{x}{a^4} \times \overline{a-x}^4$, &c. But $x + y + z + v + w$, &c. = $\frac{a}{2}$, by the question. Whence, also, $x + \frac{x}{a}(a-x) + \frac{x}{a^2}(a-x)^2 + \frac{x}{a^3}(a-x)^3 + \frac{x}{a^4}(a-x)^4$ &c. to n terms = $\frac{a}{2}$; which last being a geometrical series, whose first term is x , and the ratio $\frac{a-x}{a}$, its sum, by the common rule, is $\frac{x(\frac{a-x}{a}^n - 1)}{\frac{a-x}{a} - 1} = \frac{a}{2}$, from whence x is found = $a \times (1 - \frac{1}{2^{\frac{1}{n}}})$; and consequently the several divisions of the half line are $a \times (1 - \frac{1}{2^{\frac{1}{n}}})$, $\frac{a}{2^{\frac{1}{n}}}(1 - \frac{1}{2^{\frac{1}{n}}})$, $\frac{a}{2^{\frac{2}{n}}}(1 - \frac{1}{2^{\frac{1}{n}}})$, $\frac{a}{2^{\frac{3}{n}}}(1 - \frac{1}{2^{\frac{1}{n}}})$, &c. where n is any number whatever.

QUESTION II. Answered by the Proposer. See Fig. 12. Plate VI.

LET $AF = x$, $AC = r$, c = celerity, or velocity in the circumference, and F = variable force in the sine EF .

Then $CE : FC :: EG : EK$ by *fin. Δ*, or $r : r - x :: c : \frac{c}{r}(r - x) = v$ = velocity in FE . But, by the laws of variable forces, $F\dot{x} = \dot{v} = -\frac{c}{r}\dot{x}$, and $\dot{x} = \frac{\text{flux. } EF}{v} = \frac{\text{flux. } \sqrt{2rx - x^2}}{\frac{c}{r}(r - x)} = \frac{rx - x\dot{x}}{\sqrt{2rx - x^2}} \times \frac{r}{c(r - x)}$; whence $\frac{F(r - x)\dot{x}}{\sqrt{2rx - x^2}} \times \frac{r}{c(r - x)} = -\frac{c}{r}\dot{x}$,

or $\frac{Fr}{c\sqrt{2rx-x^2}} = -\frac{r}{r}$, and consequently $F = -\frac{c^2}{r^2} \sqrt{2rx-x^2} = -\frac{c^2}{r} \sqrt{\frac{2rx-x^2}{r^2}}$.

But when the body, in revolving, comes to D, x then becomes $= r$; in which case, $F = \frac{c^2}{r}$, or F is as $\frac{c^2}{r}$, or as the square of the velocity divided by the radius.

NEW MATHEMATICAL QUESTIONS.

QUESTION V. By W. SIMPSON.

A BOTTLE which held exactly 5 oz. 106 gr. of distilled water, was capable of containing $5\frac{1}{2}$ oz. and 170 gr. of a solution of common salt in distilled water: Required the weight of the salt held in solution, its specific gravity being 2.8?

QUESTION VI. By J. B.

THE mean annual temperature of any two latitudes, in the same hemisphere, being given, to determine the mean annual temperature of any other latitude in that hemisphere?

To Mr. NICHOLSON, Editor of "*The Journal of Natural Philosophy, Chemistry, and the Arts.*"

SIR,

I DO not know whether you will consider the following as of sufficient importance to merit a place in your valuable publication;—if so, I shall perhaps occasionally, in future, trouble you with other attempts of a similar nature.

Being about four years ago engaged in some mathematical speculations, in the course of which the resolution of pretty high equations became frequently necessary, I was induced to set about the investigation of an easy arithmetical rule for the solution of an adfectèd quadratic, in order to shorten the business of approximation.—It is, indeed, you know, on account of the prolixity of the ordinary methods by completing the square or exterminating the second term, that Dr. Halley's method of obtaining the approximate value of the root, by taking such terms of the assumed equation as involve the square of the converging quantity, is now seldom used; such only as contain its first power being usually admitted into the calculation. The mode which I adopted for this purpose will be best understood by a statement of the principles from which it is deduced.

Let $x^2 + ax$ be $= b$. Then will the affirmative value of x be $= \frac{1}{2} (\sqrt{a^2 + 4b} - a) =$
 $\frac{1}{2} \times \left(\frac{4b}{2a} - \frac{4^2b^2}{2 \cdot 4 \cdot a^3} + \frac{1 \cdot 3 \cdot 4^3b^3}{2 \cdot 4 \cdot 6 \cdot a^5} - \frac{1 \cdot 3 \cdot 5 \cdot 4^4b^4}{2 \cdot 4 \cdot 6 \cdot 8 \cdot a^7} + \frac{1 \cdot 3 \cdot 5 \cdot 7 \cdot 4^5b^5}{2 \cdot 4 \cdot 6 \cdot 8 \cdot 10 \cdot a^9} \right.$ &c.
 $= \frac{b}{a} - \frac{b^2}{a^3} + \frac{3b^3}{a^5} - \frac{5b^4}{a^7} + \frac{14b^5}{a^9}$ &c. But in order to obtain this latter series,

we have only to divide b by a , adding to the original divisor a , every time of repeating the division, double the quotient already found, together with the term which such new division shall produce, as in the common operation for extracting the square root, as follows:

$a +$

$$\begin{aligned}
 & a + \frac{b}{a} \Big) b \left(\frac{b^2}{a} - \frac{b^2}{a^3} + \frac{2b^3}{a^5} - \frac{5b^4}{a^7} + \frac{14b^5}{a^9} - \frac{42b^6}{a^{11}} \&c. \right. \\
 & \qquad \qquad \qquad \left. b + \frac{b^2}{a^2} \right. \\
 & a + \frac{2b}{a} - \frac{b^2}{a^3} \Big) \div - \frac{b^2}{a^2} \\
 & \qquad \qquad \qquad - \frac{b^2}{a^2} - \frac{2b^3}{a^4} + \frac{b^4}{a^6} \\
 & a + \frac{2b}{a} - \frac{2b^2}{a^3} + \frac{2b^3}{a^5} \Big) \div + \frac{2b^3}{a^4} - \frac{b^4}{a^6} \\
 & \qquad \qquad \qquad + \frac{2b^3}{a^4} + \frac{4b^4}{a^6} - \frac{4b^5}{a^8} \dots \&c. \\
 & a + \frac{2b}{a} - \frac{2b^2}{a^3} + \frac{4b^3}{a^5} - \frac{5b^4}{a^7} \Big) \div - \frac{5b^4}{a^6} + \frac{4b^5}{a^8} \dots \&c. \\
 & \qquad \qquad \qquad - \frac{5b^4}{a^6} - \frac{10b^5}{a^8} \dots \&c. \\
 & a + \frac{2b}{a} - \frac{2b^2}{a^3} + \frac{4b^3}{a^5} - \frac{10b^4}{a^7} + \frac{14b^5}{a^9} \Big) \div + \frac{14b^5}{a^8} \dots \&c. \\
 & \qquad \qquad \qquad + \frac{14b^5}{a^8} \dots \&c. \\
 & \qquad \qquad \qquad \div
 \end{aligned}$$

The variations which a change of signs in the original equation will produce are too obvious to need description.

Hence we get the following rules for the arithmetical solution of the three forms of quadratics, which may be reduced to two-cases.

CASE I. $x^2 \pm ax = b$.

RULE.—Place the numerical values of a and b as divisor and dividend, as in common division. —Having found the first figure of the quotient, add it in its proper place (which is the same as that of such figure of the dividend b as stands over the units place of the first product of the increased divisor into such quotient figure, when placed for subtraction) to the original divisor a ; multiply the sum by such added figure, and subtract the product from the dividend, as in common division. Then, if the next figure of the quotient to be found will fall under any of the figures of the first increased divisor, so as not to increase the number of its digits when added to it, bring down the next figure of the dividend, as in common division; but, if such next figure of the quotient will, when added in its proper place, increase the number of figures in the divisor, bring down the two next figures, as in the extraction of the square root. —Find such next figure of the quotient, and add both the figures of the quotient to the first increased divisor for a new divisor (as in extracting the square root); multiply such last increased divisor by the last found figure; subtract the product; bring down another figure or figures of the dividend, and proceed as before: always adding to the last divisor the two last figures of the quotient, including the quotient-figure obtained by each division, for a new divisor, as in extracting the square root; and so on till the operation be finished.

The quotient so obtained is a value of x , whose sign is the same as that of the second term of the original equation:—for if it's other value add to such quotient the original divisor a , and prefix to the sum the contrary sign.

EXAMPLE 1.

Given . . . $x^2 + 5613x = 2579080$. Quare x .

$$\begin{array}{r}
 a = \dots 5613 \\
 + 42 \\
 \hline
 \text{1st Divisor} = 6013 \dots) 2579080 \quad (+ 427. = x \\
 + 42 \quad \quad \quad 24052'' \quad \quad \quad 5613. \\
 \hline
 \text{2d D.} \dots = 6433 \dots) 17388. \quad - 6040. = x. \\
 + 27 \quad \quad \quad 12866. \\
 \hline
 \text{3d D.} \dots = 6460 \dots) 45220 \\
 \hline
 \quad \quad \quad 45220 \\
 \hline
 \quad \quad \quad .0.
 \end{array}$$

EXAMPLE 2.

Given $x^2 + 13.24687x = 123.45678932$. Quare x .

$$\begin{array}{r}
 a \dots\dots = 13.24687. \\
 + 6 \\
 \hline
 \text{1st Divisor} = 19.24687. \quad) 123.45678932 \quad (+ 6.3120464569 \dots \&c. = x \\
 + 63 \quad \quad \quad 115.48122'' \quad \quad \quad 13.24687 \\
 \hline
 \text{2d D.} \dots = 2554687. \quad 7975569 \quad - 19.5589164569 \dots \&c. = x. \\
 + 31 \quad \quad \quad 3115083 \\
 \hline
 \text{3d D.} \dots = 2585687 \quad 5293962 \\
 + 12 \quad \quad \quad 1201880. \\
 \hline
 \quad \quad \quad 167045. \\
 \text{4th D.} \dots = 2586887. \quad 11820. \\
 \quad \quad \quad 1472. \\
 \quad \quad \quad 20 \quad \quad \quad 179. \\
 \quad \quad \quad \quad \quad \quad 24. \\
 \text{5th D.} \dots = 2587087 \quad \quad \quad 2. \\
 \quad \quad \quad \dots\dots\dots
 \end{array}$$

EXAMPLE 3.

Given $x^2 - 45x = 522.47695432$. Quare x .

$$\begin{array}{r}
 a \dots\dots = 45 \\
 + 9 \\
 \hline
 \text{1st Divisor} \dots = 54 \quad) 522.47695432 \quad (- 9.573773622 \dots \&c. = x \\
 + 95 \quad \quad \quad 486 \quad \quad \quad 45 \\
 \hline
 \text{2d D.} \dots = 635 \quad 3647 \quad + 54.573773622 \dots \&c. = x \\
 + 57 \quad \quad \quad 47269 \\
 \hline
 \quad \quad \quad 242054 \\
 \text{3d D.} \dots = 6407. \quad 4962532. \\
 + 73 \quad \quad \quad 472263. \\
 \hline
 \quad \quad \quad 23236. \\
 \text{4th D.} \dots = 64143 \quad 3992. \\
 + 37 \quad \quad \quad 144. \\
 \hline
 \quad \quad \quad 16. \\
 \text{5th D.} \dots = 641467. \quad \quad \quad 3. \\
 \quad \quad \quad \dots\dots\dots
 \end{array}$$

CASE II. $x^2 - ax = -b$.

RULE.—Proceed as before, except that instead of adding the two last figures of the quotient to each divisor, as in the former Case, they must in this be subtracted from it, and the remainder will be the new divisor for each repetition of the division.

The quotient is the lesser value of the root, which, subtracted from the original divisor a , will give the other value, both of them being affirmative. If b be greater than the square of $\frac{1}{2}a$, the solution is however impossible.

EXAMPLE.

Given $x^2 - 19.23456x = -83.57214567$. Quære x .

a	$= 19.23456$.		
	$- 6$		
1st Divisor . .	$= 13.23456$.) 83.57214567 .	($+ 6.630654987$. &c. = x .
	$- 66$	79.40736 .	19.23456 .
2d D.	$= 663456$.	4164785	$+ 12.603905012$. &c. = x .
	$- 63$	1840496	
		3912870 .	
3d D.	$= 600456$.	328494 .	
	$- 30$	29796 .	
		5900 .	
4th D.	$= 597456$.	524 .	
	$- 06$	46 .	
		5 .	
5th D.	$= 597396$		

It is unnecessary to remark, that the contractions ordinarily made use of in the extraction of the square root are equally applicable here; and they are accordingly applied in all the examples except the first.

I am inclined to believe that such as use this process will find it attended with some advantages in point of expedition, especially where a considerable number of places is required in the root—and being mathematically correct, it may of course be extended to any length.

I am, Sir, your most obedient Servant,

9th May 1797. J. F.—:—:—R.

SCIENTIFIC NEWS.

Account of Memoirs published by the Polytechnic School at Paris.

[Continued from page 95.]

IT has long been known, that the sulphure of potash attracts the oxygene in the atmosphere, and separates it from the azote with which it is combined. Attempts were consequently made to construct eudiometers with this sulphure in imitation of Scheele; but none of these

gave the result with the requisite speed and accuracy. Guyton discovered, that by heating the sulphure the combination is made instantly. He therefore advises to take a small retort filled with water, to place therein the small piece of sulphure, then to introduce the air and heat with a taper to that place on which the sulphure rests. The absorption of the oxygene is immediately made, and the desired result ascertained.

By this invention, which contributes to the accuracy and value of an instrument greatly useful in meteorological observations, Guyton has rendered a real service to philosophers.

Gottling had maintained, in a memoir published in Gren's Journal, that phosphorus burns in azotic gas, forms an acid by this combustion, and does not burn in oxygenous gas. Berthollet made several experiments on the combination of phosphorus with different gases. He found, 1. That the phosphorus does not combine with oxygene at the ordinary temperature, but requires a more elevated temperature to produce combustion. 2. That phosphorus is soluble in the azotic gas; and that, by means of this solution, it burns with oxygenous gas at the ordinary temperature, and appears luminous in the dark.

By means of these experiments, Berthollet explained the extraordinary phenomena which Gottling, Lemps, and Lampadius, had observed; and he imagined that phosphorus might be used to measure the quantity of oxygene gas contained in the air of the atmosphere, or any other gas containing azote.

This new eudiometer has an advantage over other instruments of the same nature, by shewing the precise instant of the total absorption of oxygene, which happens when no more white vapour is seen in the day light, nor light in an obscure place; but it has likewise the inconvenience of requiring the presence of a quantity of azote sufficient to dissolve the phosphorus readily before its combination with oxygene.

In the analysis of the calcedony of Creusot, of which the constituent parts proved to be, filix 86.08, iron 7.68, alumine 4.11, lime 1.16, loss 1.0, Guyton aimed less to ascertain the proportions of ingredients in a stone so variable, than to present to the pupils of the school an example of the analysis of stones. In this respect his memoir is one of the most instructive in the collection.

A great number of chemists, with Pott at their head, have laboured at the solution of the interesting problem of the fusibility of earths, upon which they have made many scientific experiments. To these Guyton has added a great number of his own. He first tried the fusibilities of the earths with oxygene gas, and afterwards the effect of carbonate of soda, calcined borax, pulverized ammoniacal phosphate of soda, and the red oxide of lead. He afterwards mixed the earths two and two with the preceding fluxes, and at last terminated by the action of the earths upon each other, and that of calcareous phosphate on each of them. The series of facts must be consulted in the memoir itself, as their connection does not allow of detached extracts.

Clouet had published in a memoir, that the prussic colouring principle might be obtained by passing ammoniacal gas through ignited charcoal. The experiments related by Bonjour, in the third Cahier of the Journal of the Polytechnic School, consist merely of the processes he made use of, and the trials he made in the presence of Clouet to verify the phenomenon he had announced, and which, after a most careful examination, proved to be in every respect agreeable to the account which was published.

The memoir of Fourcroy and Vauquelin on the sulphureous acid, consists in an examination or history of its action on every known substance. They, in the first place, examined this action

upon	{ caloric oxygene gas,		
next on the acids	{ sulphuric nitric muriatic	with the alkalis	{ potash soda ammoniac,
afterwards on the combustibles	{ hydrogen phosphorus phosphorated hydrogen gas sulphurated hydrogen gas carbon	with the earths	{ alumine lime barytes magnesia.

In every case wherein the sulphureous acid formed a salt by combination, these chemists have described the form of its crystals, its fusibility, the affinities and proportion of constituent parts.

This memoir presents a complete table of the binary combinations of the sulphureous acid, and renders it very desirable that the authors should continue, for the advancement of science, to publish similar accounts of a great number of substances, on which our notions are yet very incomplete.

The three memoirs of general physics: 1. Of the congelation of mercury, by Hassenfratz, Bonjour, and Weite. 2. The laws of dilatation in elastic fluids, by Prony. 3. The influence of snow and rain on vegetation, by Hassenfratz.

It has long been known, that mercury was rendered solid at Petersburg by cold 24 degrees of Reaumur below zero, and that in this state it becomes malleable. The experiment was repeated at London by Cavendish, by producing the same degree of artificial cold; but this result had never been obtained in France.

The memoir of Hassenfratz on the congelation of mercury contains only a repetition of this experiment, in which the proportion of caloric, absorbed during the congelation of the metal, is determined, which had not before been done by any philosopher. This experiment has afforded a very curious result. In water the caloric, absorbed during the transition from the solid to the fluid state, is sufficient to have raised its temperature 60 degrees Reaumur; and in mercury the quantity to produce the same effect would have been sufficient to raise the temperature 64 degrees.

In order to ascertain the principle and the laws to which a great number of phenomena are referable, it is necessary to make a series of reiterated experiments on the same facts. This is attended with two kinds of difficulties, namely, the time they require, and the imperfection of the instrument.

Nevertheless, when the existence of a law in the progress of any phenomenon is ascertained, it is always possible, from a small number of experiments, to discover by analysis an equation which may express the law. This is the object of the memoir which Prony has printed in the second Cahier of the Journal of the Polytechnic School. It is divided into two parts. In the first he investigates a method of interpolation applicable to the phenomena which depend on elastic fluids; in the second he applies this method to ascertain the law of

of dilatibility in these fluids. For this application he avails himself of the experiments made by Prieur, and published by Guyton, on the dilatibility of oxygene gas, azote, hydrogenous, nitrous, carbonic acid, and ammoniacal gases; and lastly, on the expansive force of the vapour of water and that of alcohol.

This memoir contains tables deduced from the equations afforded by the methods made use of by the author, and the results obtained by Prieur. They are likewise expressed by engraved curves.

It was formerly observed, that snow preserves the vegetative power, and that rain accelerates the growth of plants more than artificial waterings. Hassenfratz has enquired, in his memoir, into the cause of these comparative effects. He shews, from several experiments, that the preserving power of snow arises from two circumstances—its imperfect conducting power, and its oxygenation. This philosopher has proved, that snow is water oxygenated and converted into the solid form. He has also shewn, that rain is more oxygenated than any other water, and that a large portion of its good effects is to be attributed to this circumstance.

The memoir on the arts is written by Chaussier. Its object is to describe the composition of a liquor proper to be substituted instead of wine lees in fulling, to obtain the greatest effect with the most facility and economy.

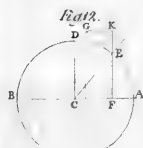
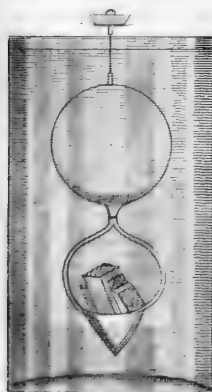
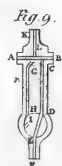
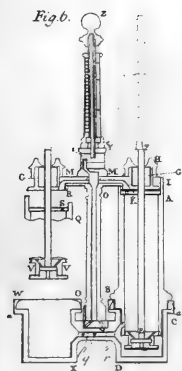
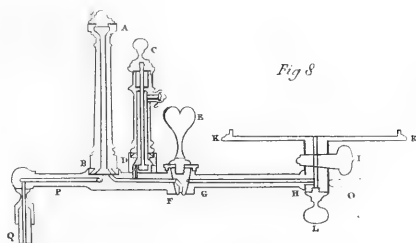
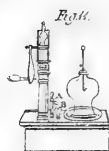
The liquor indicated by Chaussier is a small quantity of sulphuric acid. This acid bath, which experience has assured him may be used with invariable success, likewise affords the advantage of obtaining a very white colour in hats, because the felt does not become coloured in the working.

Extract of a Letter from Professor LORENZ DE CRELL.

MR. KLAPROTH* is continuing his chemical analysis, and will shortly publish a second volume of them. He has found that the newly discovered *Titanium* is by no means so rare as might be supposed from its not having been known till lately. He has detected it in a singular kind of mineral found at Aschaffenberg by Prince Gallitzin, and in some other minerals, as well as in menakanite, in which it is mixed with iron.—Mr. Westrumb's method of obtaining ardent spirits from different sorts of grain, proves to be excellent, and succeeds well on a large scale as a branch of trade.—Sulphate of barytes does not require more than an equal quantity of well aerated alkali for its decomposition both in the humid and dry way. Muriate of barytes is obtained perfectly pure by boiling it in ten times its weight of highly rectified alcohol vini, which dissolves nothing but this muriate.—To separate hepatic gas and carbonic acid gas when united, employ acidulated acetite of silver or mercury. Either of these metallic salts will absorb the hepatic gas, and leave the carbonic acid gas untouched.

* This article was forwarded to the learned editor of the English translation of Crell's Journal; from whom I received it in a letter, containing very encouraging expressions of approval and good wishes for the success of the present undertaking.

AIR PUMP by the REV.^d JOHN PRINCE



Gravimeter

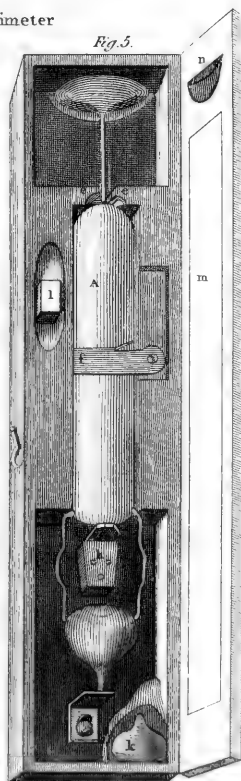
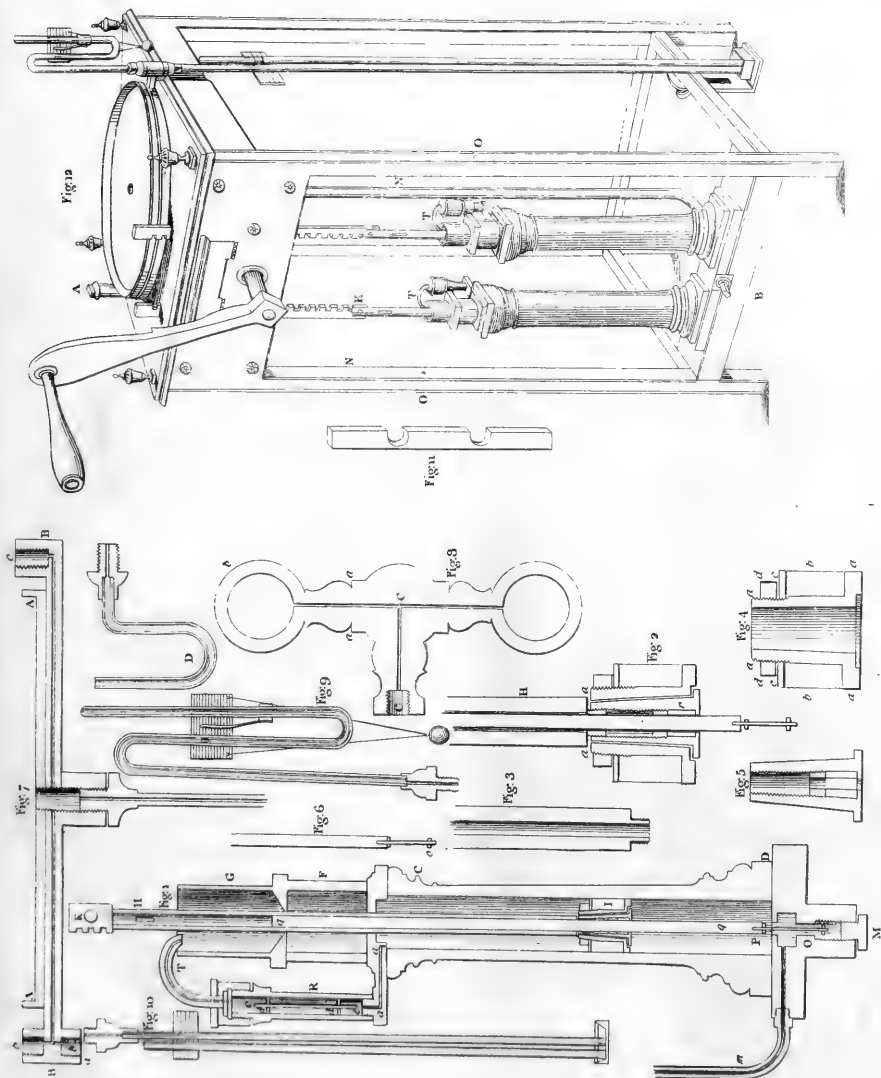


Fig. 3.







A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

JULY 1797.

ARTICLE I.

*Observations on Horizontal Refractions which affect the Appearance of Terrestrial Objects, and the Dip or Depresson of the Horizon of the Sea. By JOSEPH HUDDART, Esq. F. R. S.**

THE variation and uncertainty of the dip in different states of the air, taken at the same altitude above the level of the sea, was the reason of my turning my thoughts to this subject, as it renders the latitude observed incorrect, by giving an erroneous zenith distance of a celestial object.

I have often observed, that lowlands, and the extremity of headlands, or points forming an acute angle with the horizon of the sea, and viewed from a distance beyond it, appear elevated above it, with an open space between the land and the sea. The most remarkable instance of this appearance of the land, I observed at Macao, for several days previous to a typhon, in which the Locko lost her topmasts in Macao roads; the points of the islands and lowlands appearing the highest, and the spaces between them and the sea the largest, I ever saw. I believe it arises and is proportional to the evaporation going on from the sea. And in reflecting upon this phenomenon, I am convinced that those appearances must arise from refraction, and that, instead of the density of the atmosphere increasing to the surface of the sea, it must decrease from some space above it; and that evaporation is the principal cause which prevents the uniformity of density and refraction being continued by the general law down to the surface of the earth. And I am inclined to believe, though I mention it here as a conjecture, that the difference of specific gravity in the particles of the atmosphere may be a principal agent in evaporation; for the corpuscles of air, from their affinity with water, being combined at the surface of the fluid from expansion, form air

* Philosophical Transactions for 1797.

specifically lighter than the drier atmosphere; and therefore float or rise, from that principle, as steam from water; and in their rising (the surrounding corpuscles from the same cause imbibing a part of the moisture) become continually drier as they ascend, yet continue ascending until they become equally dense with the air*. However, these conjectures I shall leave, and proceed to the following observations upon refractions:

In the year 1793, when at Allonby in Cumberland, I made some remarks on the appearance of the Abbey-head in Galloway, which in distance from Allonby is about seven leagues; and from my window, at fifty feet above the level of the sea, at that time of tide, I observed the appearance of the land about the Head as represented in Pl. VIII. Fig. 1. There was a dry sand xy , called Robin Rigg, between me and the Head, at the distance from my house of between three and four miles; over which I saw the horizon of the sea HO ; the sand at this time was about three or four feet above the level of the sea. The hummock d is a part of the headland; but appeared insulated or detached from the rest, and considerably elevated above the sea, with an open space between. I then came down about twenty-five feet, when I had the dry sand of Robin Rigg xy in the apparent horizon, and lost all that floating appearance seen from above; and the Abbey-head appeared everywhere distinct from the surface of the sand. This being in the afternoon, the wet or moisture on the sand would in a great measure be dried up. I have reason therefore to conclude, that evaporation is the cause of a less refraction near the surface of the sea; and when so much so as to make an object appear elevated wholly above the horizon (as at d in Fig. 1.) there will from every point of this object issue two pencils of rays of light which enter the eye of the observer, and that below the dotted line AB (parallel to the horizon of the sea HO) the objects on the land will appear inverted.

To explain this phenomenon, I shall propose the following theory, and compare it with the observations which I have made. Suppose HO , Fig. 2. to represent the horizontal surface of the sea, and the parallel lines above it the lamina or strata of corpuscles, which, next the fluid, are most expanded, or the rarest; and every lamina upwards increasing in density, till it arrive at a maximum (and which I shall in future call the maximum of density) at the line DC , above which it again decreases in density *ad infinitum*.

Though this in reality may be the case, I do not wish to extend the meaning of the word density farther than to be taken for the refractive power of the atmosphere; that is, a ray of light entering obliquely a denser lamina, to be refracted towards a perpendicular to its surface; and in entering a rarer lamina the contrary; which laminae being taken at infinitely small distances, the ray of light will form a curve agreeable to the laws of dioptrics.

In order to establish this principle in horizontal refractions, I traced over various parts of this shore, at different times, when those appearances seemed favourable, with a good telescope, and found objects sufficient to confirm it; though it be difficult at that distance of the land to get terrestrial objects well defined so near the horizon, as will afterwards appear.

* Mr. Hamilton, in his very curious Essay on the Ascent of Vapours, does not allow of this principle even as an assistant; though by a remark (page 15) he takes notice of those appearances in the horizon of the sea, and says they arise from a strong or unusual degree of refraction; the contrary of which I hope to illustrate in the course of this paper.

One day, observing the land elevated, and seeing a small vessel at about eight miles distance, I from my window directed my telescope to her, and thought her a fitter object than any other I had seen for the purpose of explaining the phenomena of these refractions. The telescope was forty feet above the level of the sea: the boat's mast about thirty-five feet; she being about twenty to thirty tons burthen. The barometer at 29.7 inches, and Fahrenheit's thermometer at 54° .

The appearance of the vessel, as magnified in the telescope, was as represented in Fig. 3, and from the mast-head to the boom was well defined. I pretty distinctly saw the head and shoulders of the man at the helm; but the hull of the vessel was contracted, confused, and ill defined. The inverted image began to be well defined at the boom (for I could not clearly perceive the man at the helm inverted), and from the boom to the horizon of the sea the sails were well defined; and I could see a small opening above the horizon of the sea in the angle made by the gaff and mast; and had the mast been shorter by ten feet (to the height of y), the whole would have been elevated above the horizon of the sea, and from y to d an open space. This drawing was taken from a sketch I took at the time, and represents the proportion of the inverted to the erect object, as near as I could take it by the eye; the former being about two thirds of the latter in height, and the same breadth respectively; though at one time, during my observation, which I continued for about an hour, I thought the inverted nearly as tall as the erect object. The day was fine and clear, with a very light air of wind, and I found very little tremor or oscillation in viewing her through the telescope.

I have laid down Fig. 4. for the explanation of the above phenomena, in which A represents the window I viewed B the vessel from; HO the curved surface of the sea; CD parallel to HO the height of the maximum of density of the atmosphere; the lines marked with the small letters aa , bb , cc , dd , the pencils of rays under their various refractions from the vessel to the eye or object-glass of the telescope.

The pencil of rays aa , from a point near the head of the main-sail, is wholly refracted in a curve convex upwards, being everywhere above the maximum of density; and the pencil of rays dd , which issues from the same point in the sail, and passes near the horizon of the sea at x , is convex upwards from the sail to W, where it passes the line of maximum of density, which is the point of inflection; there it becomes convex downwards, passing near the horizon at x to y , where it is again inflected, and becomes convex upwards from thence to the eye. The pencil of rays bb , from the end of the boom, passing nearly parallel to the horizon, and near the maximum of density, suffers very little deviation from a right line in the first part; but in ascending (from the curvature of the sea) will be convex upwards to the eye. The pencil of rays cc , from the same point in the boom, may have the small part to c convex upwards: from c to z it will be convex downwards, and from z to the eye convex upwards.

From this investigation it appears that two pencils of rays cannot pass from the same point and enter the eye from the law of refraction, except one pencil pass through a medium which the other has not entered; and therefore the maximum of density was below the boom, and could not exceed ten feet of height above the surface of the sea at the time these observations were made.

Respecting the hull of the vessel being confused and ill defined in the telescope, as by Fig. 3, it arises from the blending of the rays from the different parts of the object

refracted through the two mediums; some parts of the hull appearing erect, and some inverted. Suppose the dotted line ii , Fig. 4. an indefinite pencil of rays passing from between the inverted and erect parts of the object, or the upper part of the hull of the vessel to the eye (for the lower part of the hull could not be observed), the objects cannot appear inverted, except the angles at the eye aAc and aAd exceed the angle aAi ; for the intermediate space could only be contracted by the secondary pencil of rays. The lengths of the inverted, compared with the erect image of the sail, is as the sines of the angles at the eye aAi to iAd ; and the angle at the eye aAd , made by the two pencils of rays from the same point near the head of the sail, must be double the angle aAi , when the inverted image is as tall as the erect. In this case the sines of the angles aAb , aAc , aAd , Fig. 4, are proportional to the altitudes ab , ac , ad , in the magnified view of the vessel Fig. 3.

Under this consideration, no inverted image of the sail will be formed, until the angle at the eye, made by the two refracted pencils of rays aa and dd , exceed the angle made by aa and bb , the apparent height of the sail of the vessel; for, were those angles equal, the inverted sail would only be contracted into the parallel of altitude of the boom b , and render the appearance confused, as in the hull of the vessel.

Respecting the existence of two pencils of rays entering the eye from every point of an object not more elevated than a , or less than i , Fig. 3, in this state of the atmosphere, I cannot bring a stronger proof than that of the strength of a light when the rays pass near the horizon of the sea, proved by the following observations:

Going down Channel about five years ago in the Trinity yacht, with several of the elder brethren, to inspect the light-houses, &c. I was told by some of the gentlemen who had been on a former survey, that the lower light of Portland was not so strong as the upper light at near distances; but that at greater distances it was much stronger. I suspected that this difference arose from the lower light being at or near the horizon of the sea, and mentioned it at the time; but afterwards had a good opportunity of making the observation. We passed the Bill of Portland in the evening, steering towards the Start; a fresh breeze from the northward, and clear night. When we had run about five leagues from the lights, during which time the upper light was universally allowed to be the stronger, (several gentlemen keeping watch to make observations thereon), the lower light drawing near the horizon suddenly shone with double lustre. Mr. Strachan, whose sight is weak, had for some time before lost sight of both lights; but could then clearly perceive the lower light. I then went aloft (as well as others), but before I got half mast up, the lower light was weaker than the upper one. On coming down upon deck, I found it again as strong as before. We proceeded on, and soon lost the lower light from the deck; and upon drawing the upper light near the horizon, it, like the former, shone exceeding bright. I again went aloft, when it diminished in brightness; but from the mast-head I could then see the lower light near the horizon as strong as before. This is in consequence of the double quantity of light entering the eye by the two pencils of rays from every point. To illustrate which, we compare the vessel Fig. 4. to a light-house built upon the shore, and A the place of the observer; and having brought down the light so low as to view it in the direction aa , another light would appear in the horizon at x , from the pencil dd ; and had the vessel been still enough to have observed it at this time with a good glass, I doubt not but the two images might have been distinctly seen. As the light dropped (by increasing

increasing the distance) the two images would appear continually to approach each other, till blended with double light in one, and disappear at the altitude i , above the apparent horizon of the sea. But, as explained before, if the strength of evaporation did not separate by refraction the pencils aa and dd to a greater angle than double the angle that the lamps and reflectors appear under, the two images would be blended, and the strong appearance of light would be of shorter duration. The distance run from the lights during the time each of the lights shone bright, would have been useful; but this did not occur at the time, nor have I had the like opportunity since. However, I recommend to the mariner to station people at different heights in looking out for a light, in order to get sight of it near the horizon, when it is always strongest.

Respecting the appearance of the Abbey-head, before mentioned, Fig. 1, the dotted line AB represents the limit, or the lowest points of the land that can be seen over the sea; for, as above stated, all the objects appearing below this line are the land above it inverted; and where the land is low, as at d and m , it must appear elevated above the horizon of the sea.

In Figure 5, let HO represent the curve of the ocean, and d the extreme top of the mount visible at A by the help of refraction; the dotted pencil of rays cc passing from d to the eye, in some part a little below the maximum of density, where inversion begins; therefore no land lower than this can be seen; for any pencil from a point in the land lower than this must, in the refraction, have a contrary flexure in the curve, and therefore pass above the observer. Let AD be a tangent to the curve at A , then the object d will appear to be elevated by refraction to D ; also let Av be a tangent to the pencil Ax at A , then the angle DAx will appear to be an open space, or between D and the horizon of the sea. Suppose a star should appear very near and over the mount d , as at $*$, two pencils would issue from every point of it, and form a star below as well as above the hummock d . There are always confused or ill defined images of the objects at the height of the dotted line, Fig. 1, above the level of the sea, as before mentioned; and instead of the points of d ending sharp in that line, they appear blunted, and the Abbey-head is frequently insulated at the neck m .

I have viewed from an elevated situation a point or headland at a distance beyond the horizon of the sea, forming, as in Fig. 6, a straight line AB , making an acute angle BAO with the horizon of the sea. Seeing the extreme point blunted and elevated, I descended; and though in descending the horizon cut the land higher, as at HO , HO , yet the point had always the same appearance as aaa , Fig. 6, though the land is known to continue in the direction of the straight line AB to beneath the horizon, or nearly so, as viewed from the height above.

If then, from a low situation, we view this headland through a telescope, the inclination of the surface AB to the horizon being known to be a straight line, it will appear as in Fig. 7; the dotted line (at the height of the point where a perpendicular xy would touch the extreme of the land) being at the limit or lowest point of erect vision. And if a tangent to the curved appearance of the land ab is drawn parallel to the inclined surface of the land AB , Fig. 6, touching it at C , the point C will shew the height of the maximum of density, where the pencil of the rays of light from thence to the eye approaches nearest the sea; for pencils of rays from this land, taken at small distances from C , will form parallel curves nearly through the refracting mediums, and C will be the point of greatest refraction;

tion; for above C, as to B, the refraction somewhat decreasing, will appear below the line ab , or the parallel to the surface of the land, and the refractions decrease below the point C; for, had they increased uniformly down to the surface of the sea, it would render the apparent angle of the point of land α more acute than the angle CaO , contrary to all observations.

Thus I have endeavoured to explain the phenomena of the distorted appearance of the land near the horizon of the sea when the evaporation is great, and when at the least I never found the land quite free from it when I used a telescope, and from thence infer that we cannot have any expectation to find a true correction for the effect of terrestrial refraction by taking any certain part of the contained arc; for the points α CB, Fig. 7, will have various refractions, though they are at nearly the same distance from the observer. And if the observations are made wholly over land, if the ground rises to within a small distance of the rays of light, in their passage from the object to the eye, as well as at the situation of the object and observer, the refractions will be subject to be influenced by the evaporation of rains, dews, &c.; which is sufficiently proved by the observations of Colonel Williams, Captain Mudge, and Mr. Dalby, Phil. Transf. 1795, p. 583.

The appearances mentioned by Colonel Williams, Captain Mudge, and Mr. Dalby (Phil. Transf. 1795, p. 586, 587) cannot be demonstrated upon general principles, as they arise from evaporation producing partial refractions. In those general principles it is supposed that the same lamina of density is everywhere at an equal distance from the surface of the sea, at least as far as the eye can reach a terrestrial object; but in the partial refractions, the lamina of the expanded or rarefied medium may be of various figures, according to circumstances, which will refract according to the incidence of the rays, and affect the appearance of the land accordingly, which I have often seen to a surprising degree. But my principal view is to shew the uncertainty of the dip of the sea, and that the effect of evaporation tends to depress the apparent horizon at x when the eye is not above the maximum of density; and from hence the difficulty of laying down any correct formula for these refractions whilst the law of evaporation is so little understood; which indeed seems a task not easy to surmount. The effect indicated by the barometer and thermometer is insufficient; and should the hydrometer be improved to fix a standard for moisture in the atmosphere, and shew the variations near the surface of the ocean, which certainly must be taken into the account (evaporation going on quicker in a dry than a moist atmosphere), the theory might still be incomplete for correcting the tables of the dip. I shall therefore conclude this paper, by shewing a method I used in practice, in order to obviate this error in low latitudes.

When I was desirous to attain more accurately the latitude of any headland, &c. in sight, I frequently observed the angular distances of the sun's nearest limb from the horizons upon the meridian both north and south, beginning a few minutes before noon, and taking alternately the observations each way, from the poop, or some convenient part of the ship, where the sun and the horizon both north and south were not intercepted; and having found the greatest and least distances from the respective horizons, which was at the sun's passing the meridian, and corrected both for refraction, by subtracting from the least, and adding to the greatest altitude, the quantity given by the table; and also having corrected for the error of the instrument, and the sun's semi-diameter, the sum of these two angular distances reduced as above— 180° is equal to double the dip, as by the following

Example.

Example.

The sun's declination $4^{\circ} 32' 30''$ and its semi-diameter $15' 58''$ took the following observation :

		South.	North.
The meridian distance of the sun's nearest limb from the horizon of the sea	— —	$78^{\circ} 36' 30''$	$= 101^{\circ} 1' 20''$
Refraction per table	— —	$- 0 11$	$= + 0 11$
Distances corr. for refraction	— —	$= 78 36 19$	$= 101 1 31$
Error of the sextant	— —	$+ 1 32$	$+ 1 32$
Sun's semi-diameter	— —	$+ 15 58$	$+ 15 58$
Half difference or the dip found	— —	$78 53 49$ $- 6 25$	$101 19 1$ $78 53 49$
Altitude reduced	— —	$= 78 47 24$	$180 12 50$
Zenith distance	— —	$= 11 12 36$	180
		Diff. $12 50$	
The sun's declination N.	— —	$= 4 32 30$	Half $= 6 25$
		Dip.	
Latitude of the ship N.	— —	$= 15 45 06$	

I regret that I cannot in this paper insert the dip which I have found in my observations; for I only retained the latitude of the ship determined thereby as is usual at sea. I generally rejected the error of the instrument, the dip, and semi-diameter, as they affect both observations with the same signs; and reduced the observation by the following method :

		South.	North.
Sun's distance as before	— —	$78^{\circ} 36' 30''$	$101^{\circ} 1' 20''$
Refraction	— —	$- 0 11$	$+ 0 11$
Diff. corr. for refraction	— —	$78 36 19$	$101 1 31$ $+ 78 36 19$
		Sum $179 37 50$	
Sum of semi-diameter, dip, and refraction = half difference	— —	$+ 11 5$ $78 47 24$	180 $+ 11 5$
		90	Diff. $22 10$
			Half $11 5$
		90	$101 12 36$
The zenith distance as before	— —	$= 11 12 36$	Zenith dist. $= 11 12 36$

It may be observed, that neither the dip, semi-diameter, or index error can affect the zenith distance of the sun's centre; and the refraction being small near the zenith, the result must be true, if the angles are accurately taken; and it is only necessary to observe, that when the sum of the distances is less than 180 , the half difference must be added to

the distances, as by the last reduction. There is a difficulty in making this observation when the sun passes the meridian very near the zenith, as the change in the azimuth from east to west is too quick to allow sufficient time: nor can it be obtained by the sextant when the sun passes the meridian more than 30 degrees from the zenith; for I never could adjust the back observation of the Hadley's quadrant with sufficient accuracy to be depended upon*.

II.

Remarkable Effect of Terrestrial Refraction on a Distant Headland.—Extract of a Letter from ANDREW ELLICOTT to DAVID RITTENHOUSE, Esq. dated at Pittsburgh, Nov. 5, 1787, concerning Observations made at Lake Erie†.

ON the 13th of last month, while we lay on the banks of Lake Erie, we had an opportunity of viewing that singular phenomenon by seamen termed looming. It was preceded by a fine aurora borealis on the evening of the 12th. The 13th was cloudy, but without rain. About 10 o'clock in the morning, as I was walking on the beach, I discovered something that had the appearance of land in the direction of Presqu'isle; about noon it became more conspicuous, and when viewed by a good achromatic telescope, the branches of the trees could be plainly discovered. From three o'clock in the afternoon till dark, the whole peninsula was considerably elevated above the horizon, and viewed by all our company with admiration. There was a singular appearance attending this phenomenon, which I do not remember to have seen taken notice of by any writer. The peninsula was frequently seen double, or rather two similar peninsulas, one above the other, with an appearance of water between. The separation and coincidence was very frequent, and not unlike that observed in shifting the index of an adjusted Godfrey's quadrant. As singular as this may appear, it is not more so than the double refraction produced by the Iceland crystal. The next morning Presqu'isle was again invisible, and remained so during our stay at that position. Presqu'isle was about 25 miles distant; its situation very low.

The same evening the wind began to blow briskly from about two points west of north,

* The optical phenomena which relate to the constitution of the atmosphere have not yet been much investigated. From the barometrical admeasurement of heights, as well as from experiments with the macrometer, or marine barometer of Halley (Roy, in Phil. Trans. Vol. LXVII.), it is ascertained that air abounding with humidity is more expandible by heat, and less dense at like temperatures than air which is more dry. And from Sir Isaac Newton's table, the refractive power of air, in respect to its density, compared with that of water in the same respect, is as 4160 to 7845 (Optics, Part III. Prop. 10.). These and other facts shew that the rays of light must be variously affected, according to the nature and circumstances of evaporation, condensation, and other processes carried on in the air, and particularly near the surface of water. That a lighter and less refractive stratum of air may thus be generated near the surface of the sea to a considerable elevation, and remain for a time in equilibrio without ascending, is highly probable; and in this case, whenever the quiescent state is from local or temporary circumstances destroyed, it seems a natural consequence, that sudden and irregular currents of the lower air, and descending blasts of the upper, may occasion all the strange effects of the typhon and other phenomena, not well described in that interesting tropical combination of sea and land to the eastward of Sumatra and Malacca. The Mediterranean Sea affords similar appearances, particularly the astonishing spectra in the Strait of Reggio, described by Kircher, Minazi, and others, called Fata Morgana, of which I purpose shortly to give an account, with an engraving. N.

† American Philos. Soc. III: 61.

and continued to increase till the evening of the 14th, when it was more violent than any thing of the kind I had ever been witness to before, and continued till the evening of the 16th, without the least intermission. Our tents were all blown down, and we were under the necessity of fortifying our camp by driving posts near to each other firmly into the ground on the windward side, and filling up the vacuities with bushes in the form of an hedge. During the continuance of this wind we frequently observed small black clouds hanging over the lake: they had but little velocity, and were sometimes exhausted, and disappeared without reaching the shore.

From the large bodies of timber blown down about the lakes, it appears that hurricanes are not uncommon. Coxe observes in his Travels through Russia, that the lakes in that country are subject to terrible storms.

III.

Extract of a Memoir of M. BENEDICT PREVOST, of Geneva, on the Emanations of Odorant Bodies. By Citizen FOURCROY.*

CITIZEN Duc la Chapelle, Director of the Society of the Sciences and Arts at Montauban, in the department of Lot, has forwarded to the First Class of the Institute, in the name of the Society, a Memoir of M. Benedict Prevost, of Geneva, *concerning various means of rendering the emanations of odorant bodies perceptible to the sight.* The First Class of the Institute heard the reading with great interest at its sitting of the 16th Pluviose, in the year 5.

The following are the facts consigned in this Memoir:

1. A concrete odorant substance, laid upon a wet glass, or broad saucer, covered with a thin stratum of water, immediately causes the water to recede, so as to form a space of several inches around it.

2. Fragments of concrete odorant matter, or small morsels of paper or cork, impregnated with an odorant liquor, and wiped, being placed on the surface of water, are immediately moved by a very swift rotation. Romieu had made this observation on camphor, and erroneously attributed the effect to electricity. The motion was perceptible even in pieces of camphor of seven or eight gros.

3. An odorant liquor being poured on the water stops the motion till it is dissipated by evaporation. Fixed oil arrests the motion for a much longer time, and until the pellicle it forms on the water is taken off.

4. When the surface of the water is cleaned by a leaf of metal, of paper, or of glass, plunged in and withdrawn successively until the pellicle is removed, the gyratory motion is renewed. If a piece of red wax or of taper be dipped in water, and the drops shaken off into a glass of water containing odorant bodies in motion, the movement will be stopped. The same effect is not produced by metal.

5. An atmosphere of elastic fluid is formed round odorant substances, and is the cause of the effects here described.

6. A morsel of camphor plunged to the depth of three or four lines in water, without floating, excites a movement of trepidation in the surrounding water, which repels small

* Annales de Chimie, XXI. 254.—Translation. N.

bodies in its vicinity, and carries them again to the camphor by starts. The author concludes that an elastic fluid escapes from the odorant body in the manner of the fire of a fusée, or the discharge of fire-arms.

7. When there is a certain proportion between the height of the water and that of the small fragment of camphor, the water is briskly driven off, returns again to the camphor, and again retires, as if by an explosion, the recoil of which often causes the camphor to make part of a revolution on its axis.

8. Fragments of camphor of the size of a pea, floated upon water in a dish of metallic leaf four or five lines in diameter, communicated motion to these dishes, though less swift than was exhibited by the camphor alone. If the glass in which this experiment is made be nearly filled with water, and covered by a (plain) glass which intercepts the contact of the air, the motion decays and ceases.

9. Camphor alone moves more rapidly than when it is placed upon the metallic plate. The author infers that the immediate contact of the water favours the disengagement of the fluid which produces the motion.

10. Camphor evaporates thirty or forty times more speedily when placed upon water, than when entirely surrounded with air.

11. Camphor, during the act of dissipation in the air, preserves its form and its opaque whiteness; upon water it is rounded, and becomes transparent, as if it had undergone a kind of fusion. It may be inferred that it arises from the acquired motion, which causes it to present a greater surface to the air.

12. In fact, out of 12 equal small pieces of camphor, six being suspended under a glass with quick-lime, and very dry, and six others being suspended in a moistened glass, together with a wet sponge, the volatilization was alike in each; and the water in this case not touching the camphor did not appear to contribute to the effect.

13. It is necessary for this purpose that the water should directly touch the camphor. Accordingly, fragments of camphor placed on blotting-paper continually wetted, are dissipated with the same speed, and become equally transparent, as when actually placed on water, though they exhibit no motion.

14. When small pieces of camphor are plunged in water, the camphor becomes rounded and transparent, does not acquire any motion, and its dissipation is less perceptible than in the air. The concurrence of air and water is therefore necessary to disengage the fluid which is the cause of the motion and total dissipation of odorant bodies.

15. The motion of odorant bodies upon water decays and ceases spontaneously at the end of a certain time; because, the water having then contracted a strong smell, the volatilization takes place in all the points of its surface; and the small mass being thus surrounded by the odorant fluid, which is no longer air, dissolves, as in the ordinary odorant fluids, without forming the gaseous jet which is the cause of the motion. The author compares the volatilization of the aromatic substance to a combustion excited by water.

In this place the author dwells for a short time on the phenomena he had described. He observes that these effects may be rendered palpable by touching the surface of the water on which the odorant substances move, with a pin dipped in oil. At the very instant, quick as lightning, these particles, as if struck, cease to move. A coloured pellicle, formed by the oil, is seen on the surface of the water. The water penetrates the pores of the oil,

like

like those of the camphor, and disengages a fluid which prevents the water from penetrating the odorant substance. The motion ceases, because this substance is then plunged in a fluid formed by the oil, which fluid is not air.

16. All bodies which are not odorant present, when hot, the same phenomena as odorant bodies. In fact, the heat gives them a degree of smell. To this phenomenon our author refers the bubbles and the motion, which are seen when an ignited piece of money is thrown into water. The elastic fluid is according to him the caloric, which is disengaged in proportion as the water penetrates the pores of the metal. It is nevertheless much more simple to attribute this phenomenon to the water converted into vapour round the ignited metal, and sometimes to hydrogen gas, when the metal is capable of decomposing water, especially at this temperature.

After the explanation of these experiments, Mr. Prevost hopes with reason that they will contribute to the theory of odours, which so nearly resembles that of the gases. He does not flatter himself with having exhausted this subject, but considers his discoveries as the means of rendering odour perceptible by water not only to the sight but even to the touch, as are likewise the vibrations of sonorous bodies. Men deprived of the sense of smell, and even the blind, according to him, may in this manner distinguish odorant bodies from those which have no smell. "Perhaps," says he, "this kind of odoroscope may, by improvement, become an odorimeter. The exceptions, such for example as that of the cerumen of the ears, which produces much effect on water without being perceptibly odorant, and that of the fingers when hot or moist, are merely apparent; for, if our senses do not in those cases discover odour, those of animals more powerfully energetic, such as the dog, perceive and distinguish individuals by its peculiar character. The odoroscope may afford the information which is wanting respecting these effluvia. Thus it is that the fat of game, the smell of which is nearly to us imperceptible, is very much so to dogs, and exhibits sensible marks by the odoroscope."

Mr. Prevost, who professes much brevity in this memoir, affirms, that water placed on the odorant liquor, instead of this last being put upon the water, affords a phenomenon considerably interesting, but which he has not described. A speedy communication of the rest of this interesting work is much to be desired.

We shall not in this place explain the manner in which it appears necessary to consider the principle of smell, which no longer appears to us proper to be regarded as a peculiar matter, and one of the immediate materials of vegetables, always identical, and enjoying the same properties. We shall have occasion in future to enter more fully into this subject, and perhaps to rectify the notions which have hitherto been formed concerning the spiritus rector or aroma of plants. We shall content ourselves with observing, that the phenomena described with clearness and precision by Mr. Prevost, as well as the theory he has given, are referable to the attraction of the odorant matter in the mass for air and water, and the solution which takes place in one or the other, or in both at the same time.

IV.

A Method of measuring the Force of an Electrical Battery during the Time of its being charged.
By Lieutenant Colonel HALDANE *.

LET the battery be insulated; and at a small distance from it place an uninsulated electrical jar; also near to the jar place one of Mr. Cuthbertson's electrometers.

The electrometer being adjusted according to the degree of force which is intended to be employed as a measure of force to be communicated to the battery, connect the electrometer with the jar; make a metallic communication between the interior side of the jar and the exterior side of the battery, and connect the interior side of the battery with the conductor of an electrical machine.

Then, by the operation of the electrical machine, the battery receives a quantity of the electrical fluid, and becomes charged. The fluid which departs from the exterior side of the battery, is received by the electrical jar, which also becomes charged; but this jar, being connected with the electrometer, explodes as soon as it acquires a force sufficient to put the electrometer into motion.

Now, the quantity of the electrical fluid which is received by this jar, between each of the explosions, is a measure of the quantity of the fluid in the battery; and the number of explosions or discharges of this jar shews the number of measures which the battery contains, and consequently the force which it is capable of exerting when discharged.

Demonstration.

THE electrometer remaining under the same adjustment will require the same force to put it in motion: this force results from the quantity of electrical fluid received by the jar; and since it is admitted, that, when effects are the same, the causes of them must be equal, it is evident that the quantity of electrical fluid contained in the jar at the time of each explosion is the same.

It is also obvious, that the sum of all these equal quantities of the electrical fluid which was contained in the jar at the time of each explosion, is equal to the whole quantity contained in the battery; for, the battery being insulated, the jar received all the electrical fluid which departed from the exterior side of the battery; and that quantity is said (in the theory of Dr. Franklin) to be equal to the quantity in the interior of the battery.

Therefore it is manifest that the number of explosions or discharges of the electrical jar, is the number of equal measures of the electrical fluid which the battery contains.

But without putting too much confidence in any philosophical theories, the effects of this operation may be more satisfactorily shewn by the following experiments:

Experiments.

A PIECE of iron wire, about 0.045 inches in diameter, and about two inches in length, was placed in the circuit through which the discharge of a small electrical battery, which contained about six feet superficial of coated glass, was to pass.

The electrical jar employed as the measure of the charge of the battery contained about 90 square inches; and the adjustment of the electrometer was varied in each set of

* Communicated by the Author.

experiments, by changing the weight applied to the balance, and also the distance of the discharging balls.

Experiment 1.

THE electrometer being adjusted with its least weight; the discharging balls placed at the distance of one inch; and the other parts of the apparatus arranged as before described; the electrical machine was put in motion, the battery and also the jar began to receive a charge, as was shewn by the repulsion of a pith-ball on a graduated quadrant placed upon the electrometer:

1. After the first explosion of the electrical jar, that is, after the battery had received one measure of the electrical fluid, a discharging rod was applied to complete the circuit in which the iron wire was placed; but, upon the discharge of the battery, no change of appearance was visible in the wire.

2. The operation of the electrical machine being continued, the discharging rod, after two explosions of the jar, that is, after the battery had received two measures, was applied as before; but, upon the discharge of the battery, no change appeared on the wire.

3. The battery was then charged with three measures; and upon discharging it as before, luminous particles of the wire were thrown off.

4. The battery, having received four measures, the wire, upon the discharge, exhibited nearly the same appearance as before.

5. The battery, having received five measures, was discharged; the wire was red-hot, and separated.

6. The battery, having received six measures, was discharged; the wire was dispersed in red-hot globules.

The battery, upon receiving between nine and ten measures, made a spontaneous discharge.

Experiment 2.

IN the second set of experiments, the apparatus was arranged as before, and the electrometer adjusted with the same weight, but the discharging balls were placed at the distance of two inches. The results were, upon discharging the battery, after having received

1 Measure,	-	No alteration in the wire.
2 Measures,	-	Luminous particles thrown off.
3 Measures,	-	The same, with smoke.
4 Measures,	-	Red-hot, and separated.
5 Measures,	-	Dispersed in red-hot globules.
Between 7 and 8 Measures,	-	A spontaneous discharge of the battery.

Experiment 3.

THE apparatus being arranged as before, the electrometer was adjusted with its greatest weight, and the discharging-balls placed at the distance of one inch: the results were, upon discharging the battery, after having received

1 Measure,	-	No alteration in the wire.
2 Measures,	-	Luminous particles thrown off.
3 Measures,	-	The same.

4 Measures,

- | | | |
|---------------------------|---|-------------------------------------|
| 4 Measures, | - | Red-hot, and separated. |
| 5 and 6 Measures, | - | Red-hot, and dispersed in globules. |
| Between 8 and 9 Measures, | - | A spontaneous discharge. |

Experiment 4.

THE electrometer being adjusted with the greatest weight, the discharging-balls were placed at the distance of two inches. The results were :

- | | | |
|---------------------------|---|---|
| 1 Measure, | - | Luminous particles thrown off. |
| 2 Measures, | - | The same, with smoke. |
| 3 Measures, | - | Red-hot. |
| 4 Measures, | - | The wire was dispersed in red-hot globules. |
| Between 6 and 7 Measures, | - | A spontaneous discharge of the battery. |

Experiment 5.

THE apparatus remained as in the fourth experiment, with the addition of another battery, containing 12 feet superficial of coated glass; and the iron wire placed in the circuit of the discharge of this battery was about 0.08 inches in diameter, and two inches in length. The results were :

- | | | |
|-----------------------------|---|--------------------------------|
| 1 Measure, | - | No alteration in the wire. |
| 4 Measures, | - | Luminous particles thrown off. |
| 6 Measures, | - | The same, with smoke. |
| 8 Measures, | - | Red-hot, and separated. |
| 10 Measures, | - | Dispersed in red-hot globules. |
| Between 15 and 16 Measures, | - | A spontaneous discharge. |

Harley-street; May 27, 1797.

HENRY HALDANE.

The electrical machine employed in these experiments has a glass cylinder of nearly 18 inches in diameter. It was constructed by Mr. Nairne, and is a most powerful instrument, particularly in exhibiting all the phenomena in which the negative, or electricity of the rubber is concerned.

V.

On the Mechanical Construction and Uses of the Screw.

ELEMENTARY writers on mechanics, from demonstrations founded on the leading property of the lever, teach us that the action of any power to raise a weight or overcome an obstacle is facilitated by the inclined plane in the proportion of its length to its perpendicular height, when the power operates in the direction of the plane itself, or as the horizontal base to the height when the power acts in an horizontal direction. They also shew, by the easy process of applying a right-angled triangle to the surface of a cylinder, that the thread of a screw, when its axis is placed in the perpendicular, is in effect an inclined plane, whose length in one turn is measured by the portion of the thread itself, and height is the distance between two contiguous threads: that is to say, it would be just as easy to raise a

weight by sliding it along this thread, as along an inclined plane of the same length and height. The female screw, or nut containing an hollow screw, is in fact a weight adapted to this sliding process, and does accordingly correspond with the weight on the similar inclined plane, provided the power be applied at the very thread of the screw; but if the advantage of a lever be taken to move the nut, the resistance will be less in proportion as the radius of the screw is less than the length of the lever.

Hence it is seen that the screw affords a convenient and powerful application of the inclined plane to mechanical purposes. To compute its effect, whether a lever be used or not, is equally simple; for the power, wherever it be applied, is directed in a plane at right angles to the axis of motion; and the agent itself describes the thread of a screw constantly rising through the same quantity in each turn. The proportion of the power to the weight, will therefore be as the distance between two contiguous threads to the circumference of the cylinder in which the power moves. Or in the form of a rule. Multiply the length of the lever by 44, and divide the product by 7; the quotient will bear the same proportion to the distance between thread and thread as the weight does to the power when in equilibrio.

In mixed hydraulic engines, such as water-mills, pumps, and the like, the usual addition allowed to overcome frictions, and give the velocity of working, is three-eighths of the power in equilibrio. The screw requires more than this, but the quantity must vary according to the obliquity of the thread.

The obliquity is greater the coarser the thread, and the smaller the diameter of the generating cylinder. For slow and strong pressures a fine thread is to be preferred; but for a quick stroke and speedy return, as in the printing-press and coining-engine, the thread must rise with considerable speed.

The man of science is most particularly interested in the consideration of this mechanical organ, from the extreme accuracy with which it may be applied in the measurement of lines or spaces. Thus, for example, if the threads of a screw be one-fiftieth of an inch apart, every turn in a fixed nut will cause its extremity to advance or recede that quantity; and if a graduated circular plate of one foot in diameter be fixed to the other extremity, the parts of each turn may be ascertained. One quarter of a degree, or the 1440th part, on such a circle, will be a very conveniently visible division, and will correspond with the 1440th part of one-fiftieth of an inch, which is less than the seventy-thousandth part of an inch.

A question of no small importance immediately presents itself on this occasion. Does the practice justify this theoretical conclusion? or, in other words, Do not the irregularities of workmanship shew themselves to be much greater than the quantities here attempted to be measured? The solution, which will shew the true value of the measuring screw, must be deduced from experiment:

The most obvious method of forming a screw consists in tracing helical lines upon the surface of a cylinder, and filing or cutting away the part between the intended threads. In this way some large screws are actually made; but it is immediately perceived on reflection, that the original tracing is subject to all the inaccuracies of division by hand; to which must also be added the still greater errors of filing or cutting. By this process alone it would be impracticable to make a screw even of moderate correctness.

If a number of deep marks, such as *c d*, be cut obliquely across the thread with a three-square file, in a steel screw *AB* Figure 1. Plate IX. of the kind here described, and the screw be then hardened, and tempered to the pale straw-colour, it may be used to form the instrument Fig. 2. called a screw-tool. For the screw-tool being rendered quite soft by annealing, and fixed in a proper handle, will be cut into teeth by pressing the end *A* against the screw *AB* Fig. 1, while turning in the lathe. It is scarcely necessary to say that the screw-tool will be supported by the Rest, and that oil or tallow must be used to prevent the work from heating.

It is evident that the screw-tool will be much more regular than the screw it was made from; because all the irregularities of the screw will pass in succession through each of its teeth. If the screw therefore be now softened, and the screw-tool hardened and tempered to straw-colour, and whetted on the upper face, the former operation in the lathe may be repeated; but with this difference, that the screw-tool will now cut the screw, and render it still more regular, for the reason just mentioned. After this rectification, it is usual to deepen the groove between the threads by applying an acute angled file or tool to the screw while turning.

The process of rectification naturally suggests an improvement in the order of working. It is certainly much more easy to make a regular screw-tool by hand than a screw; more especially when the thread is fine. Suppose Fig. 2. to be such a screw-tool, and the screw part of *AB* Fig. 1. to be merely cylindrical. If the hard screw-tool be then steadily applied against the soft cylinder while turning in the lathe, it will cut a number of equidistant circular grooves; but if, instead of being held in one place, the tool be regularly moved along the rest, the grooves will be helical; and the succeeding teeth, falling into the marks made by those which went before, will render the threads equidistant, and tend to produce more and more of regularity. Such a screw may nevertheless vary in the obliquity of its threads on different sides, and this imperfection will be very little amended by continuing the action of the screw-tool. It would require too long a detail to state the manipulations by which workmen acquire the facility of making screws in this way with considerable regularity; and it is besides probable that they would occur to any ingenious beginner as he proceeds.

Whether the tap (which is the name given to a screw reserved for making or tapping female screws) be made and rectified in this or the other way, the next ordinary step is to tap a pair of dies. Figure 5. Plate IX. represents a die for making screws. Its thickness is shewn in Fig. 6; and in Fig. 3. a pair of such dies are seen properly disposed in the frame *ABCD*, called a stock. The thickness of the stock is precisely equal to that of the dies, which are well fitted to slide in the opening *GHIK*. On the lower side of the stock, as it now lies, is fixed, by rivets or screws, a flat plate of the dimensions of Fig. 4, the width of the opening *PPPP* being somewhat less than that of *GHIK* Fig. 3. It therefore affords a support to prevent the dies from falling through; and the plate Fig. 4. being applied on the upper side of the stock serves effectually to prevent any motion whatever in the dies, except the direct approach or recess with regard to each other by means of the screws *EF*. The top plate is secured by four screws *LLLL*, which pass through holes of the Figure represented in the sketch. The convenience of these holes is, that the plate may be taken off to change the dies without actually drawing the four screws. A variety of fashions prevail in the form of stocks, which are supposed to have their respective advantages.

tages. Steadiness, accurate fitting, and a regular fair action of the end pressure, are the essential requisites of a good stock. The drawing here given is one of the most simple. When it is used for strong work, the stock is fixed in a piece of wood terminating in a handle on each side.

Suppose the dies NN Fig. 3. to be soft; and a notch A Fig. 5, of about sixty degrees of the circumference of the tap E Fig. 1, not reckoning the thickness of the thread, filed in each. Let them be placed in the stock, two pieces of brass OO being put in to defend them against the thumb-screws, and the whole secured by the face plate. The dies are then ready for tapping. The hardened tap AB must be fixed in the vice, with its end B uppermost, and the dies be then made to bite gently upon its screw. When the stock in this situation (the cutting parts being previously well oiled) is moved backwards and forwards on the axis of the screw, the tap cuts an hollow thread in the dies, which becomes deeper and deeper as the screws E and F are driven inwards. The dies must be taken out and examined from time to time during the process, which is to be continued until the thread is found to be as deep and perfect as may be expected. A notch C Fig. 6. is then to be filed in the bottom of the thread, the dies hardened and tempered, and the face A Fig. 5. whetted on the Turkey hone. The dies are in this state ready for tapping other screws.

For the same reason why an irregular screw will cut a more regular screw tool (Fig. 2.), it happens that the female screw is more regular than the male. The dies will therefore rectify the tap to a still greater degree of accuracy; and this tap, if thought necessary, may be again used to rectify the dies.

Excellent short screws have been made by very slow and careful tapping with new sharp dies. The principal sources of error in this process are the following:

1. The stop from change of hands produces a wave in the thread. This is frequently perceptible to the eye, and may be partly remedied by the workman walking round his work, or frequently shifting the screw in the vice.

2. Pressure in the direction of the axis, by one hand more than the other, occasions a periodical change in the obliquity of the thread, and the screw is what the workmen call *drunk*. It is difficult to avoid this error when the screw is very short, or the dies thin.

Mechanical means have been used to remove these two sources of error, by fixing the stock, and confining the tap to move by a regular rotation in the direction of its own axis.

3. Since the corners of the dies cut first, there will be little disposition in the first strokes to form the required screw. If the operation be not favoured by pressure in the direction of the axis, the dies will follow accidental irregularities of the surface or of the material; and as probably cut circular rings, or a left-handed screw, as the right-handed screw supposed to be in the dies. In these circumstances, therefore, the thread frequently proves undulated, with very little rise in the run of each corner of the die, until it suddenly falls into the cut made by the corner it follows. Each turn consists accordingly of four waves, which are amended as the dies sink deeper, and are led by their own slope. In extreme strictness it may be questioned whether a tapped screw be ever begun without this irregularity, or perfectly corrected by the subsequent operation.

4. If the dies be not well fitted in the stock, or the material to be cut be veiny or unequally hard, they will recede from the hard parts, and produce an irregular thread. Long dies are least subject to this imperfection.

5. The opposite extremes of the thread in the dies incline towards different regions, and therefore in effect cross each other. For this reason, it is impossible for two dies to be made to approach each other in the direction of the thread. In fact, they approach in a plane at right angles to the axis. This is the circumstance which limits the diameter, and the depth and inclination of the cut, beyond which dies cannot operate. Hence also it is, that a true flat-thread screw cannot be cut in dies, and a many-threaded screw, or screw of great obliquity, can only be cut in the common stock by two or three pair of dies in succession.

6. As it is certain that a pair of well fitted dies can never run both along the same stroke till perfectly home to their natural place of coincidence, the cut made by one die will tend to draw the other along the cylinder. So that while one die cuts the upper side of the thread, the other die will cut the opposite or under side. In this cross action, the frame and the dies themselves will yield from elasticity, and that the more where the material is the hardest, or the work forced. Hence with a like pressure the soft side will have the widest cut, and be soonest cut down, and the sides of the thread will be waving. This seems to be the chief reason why tapping a screw renders it less straight and round than before.

7. The dies operate, rather in consequence of the force of pressure by means of the screws of the stock than by the effect of their edge. This force must not only be productive of an oval figure and other irregularities, from the uncertain spring and yielding of the instrument and material, but must probably occasion a change, of the nature of lamination or hammering. Whenever this last event happens the thread will be thinner, the cut wider, and the screw itself coarser than it would otherwise have been. I have three flat-thread steel screws, each 11 inches long, 0.6 in. outside diameter, and depth of thread near 0.1 inch. They were tapped in dies 0.9 inch deep produced by an original tap containing 13 threads in 1.4 inch. The motion was slow and uniform between the centres of a kind of lathe constructed for the purpose. None of the screws are of the same uniform fineness from end to end. One screw is scarcely coarser than the original tap; another is half a thread coarser in 102 threads, or 11 inches; and the third is a whole thread coarser in the same quantity. These differences seemed to arise from the different degrees of softness in the steel; the last having been more annealed than the second, and the first not at all.

Hence is deduced the important conclusion, that however we may by mechanical expedients diminish the irregularities, of obliquity and distance of contiguous threads, in a tapped screw, yet in screws of several inches long there is an irregularity growing out of the nature of the materials, which does not appear to admit of any other remedy in this method than extreme care and slowness of working.

The regular inclination of a short tapped screw of about forty threads in the inch, was found, by experiments at the end of a very short lever or radius set by a spirit-level, to be discernible in much less quantities than the hundred thousandth part of an inch.

Since all these sources of irregularity in tapping would be much diminished by increasing the number of dies, I have noted among my Agenda a stock with four pair of dies acting at once with eight edges. In these the error of obliquity in coming up, as remarked in the numbered paragraph 5, and of the first run (paragraph 3.) are incomparably less than in a single pair of dies. As I have not yet made it up, and every new instrument is likely to be altered and simplified during the actual construction, I shall not particularly

state

state either the dimensions, or such circumstances as are necessary to insure the truth of workmanship.

Fig. 7. Plate IX. represents a stock containing eight dies. HHHH represents a brass ring with parallel faces. It is perforated straight through at the circle DDD. Upon a plate of brass of the same diameter as the ring, are fixed the eight pieces marked with the letter A, which afford grooves for eight dies marked B to slide in. When the brass plate is applied to the ring H, the pieces A with the dies occupy the internal part of the ring except the central hole at C; and in this situation the opposite or upper face of H forms one plane with the pieces A and the dies B. Every die is thrust forward when required by the action of a nut F upon a screw EK, which is prevented from turning by the form of the inner end K. The nuts F are properly secured in the ring H, so as to turn fairly and without shake, at the same time that they are supported against the reaction of the dies. A covering plate LLL is screwed on to secure the dies and nuts in their places. The nuts F are provided with equal and similar small wheels, which just rise above the plane of the ring, and are driven by a contrate wheel, attached to a second covering plate, rather larger in diameter than the ring H. The edge of this last plate is milled, and it turns upon and is secured in its place by a central ring GGG fixed on the first covering plate by three binding screws.

The use of this stock and dies needs very little explanation. Suppose the eight dies to be regularly numbered and put into their places; the pinions F all accurately set with a marked tooth uppermost in each; the upper plate screwed on, and the contrate wheel piece applied to the pinions in a situation marked by the coincidence of divisions made for that purpose on the central piece G. Imagine the stock in this situation to be well secured against the face of an upright poppet head, and another head at a proper distance to sustain a metallic hole lying truly in the axis of this apparatus. Let the finished and hardened tap be provided with a cylindrical tail to move in this hole, while the screw part is supported by the faces of the eight dies. In this situation let the tap be gently turned by a handle of such a construction, that by means of a joint near the centre, and another near the holding part, it shall not be possible for the hand to exert any bearing in the direction of its axis. By a careful continuation of this process, using oil, and setting up the system of back screws a very little at a time, the dies will at length receive the screw, and may be rendered perfect by the several processes of rectification before mentioned.

With such an instrument, in skilful hands, I think the process of tapping might be fairly tried in the construction of fine measuring screws, even in the hardened and tempered state. A leading screw might be used to drive the cylindrical part, if thought necessary, for the first taking of the dies. For notwithstanding all the difficulties of the method by dies, and the improvements of ingenious artists in the method of turning screws by a tool mechanically carried along the axis, there are sufficient reasons to wish that the tapping process, on which they are originally founded, should be brought to perfection. But the present communication is already of such a length, as to require that the value and importance of those methods should be discussed in a future paper.

VI.

*The Method of obtaining the Fixed Alkalies in Crystals of the greatest Purity. By LOWITZ, Professor, &c. at Petersburg *.*

1. **T**HE caustic alkalis, as obtained by the ordinary processes, are sufficiently pure for the purposes of pharmacy and the arts; but for the experiments of philosophical chemistry the greatest purity is required in every substance made use of.

2. Caustic alkalis have hitherto been very seldom used in the analysis of mineral substances. M. Klaproth † has shewn their utility in analyses, not only in effecting a more easy separation of aluminous earth, but likewise to disunite the principles of the most refractory fossils. This circumstance has induced me to give a more particular description of the discovery I made some years ago of the crystallization of the caustic alkalis, as the principal means of obtaining them in a state of absolute purity. I have likewise embraced this opportunity of rectifying the mistake of various chemists, who have imagined that crystallization cannot take place but during the most extreme cold of winter.

3. My first experiments on vegetable alkali were made in the hottest days of summer, and the most beautiful crystallization was formed even over the fire.

4. The crystallization of soda does not succeed but in winter; but the ordinary cold of five degrees (R.) is sufficient for this effect. The reason why this alkali requires so low a temperature arises from the property its crystals possess of being soluble at the slightest heat in their own water of crystallization. The same thing is observable in the muriate of soda or common salt crystallized by cold ‡.

5. Before the discovery of this method, no one had succeeded by the ordinary processes to obtain a colourless lixivium of caustic alkali. These solutions were always more or less brown. By repeated crystallizations this lixivium may be had in the most concentrated state, as limpid as the purest water; which incontestably proves that the common lixivia are contaminated by heterogeneous matters, which charcoal and the strongest calcination are incapable of destroying.

6. The whole of the operation for obtaining a caustic alkali of the greatest purity and without the least colour, consists in this: A caustic lixivium of pot-ash is evaporated to a thick pellicle. After the cooling, the foreign salt which has crystallized is to be separated, and the evaporation of the lixivium continued in an iron pot, as in the preparation of the lapis causticus. During this second evaporation the pellicle of foreign salts, particularly of carbonate of pot-ash, which continues to be formed, must be carefully taken off with an iron skimmer. When no more pellicle is formed, and the matter ceases to boil up, it is removed from the fire, and suffered to cool, with continual agitation by an iron spatula. It is then to be dissolved in double the quantity of cold water, the solution filtered,

* Von Crell's Chemische Annalen auf 1796, b. i. f. 306.

† Beyträge zur Chemischen Kenntniss der Mineral Körper, b. i.

‡ Lowitz obtained this singular crystallization of the muriate of soda by exposing a solution of this salt to intense cold. The crystals, which are hexagonal, are two inches in diameter, and one line thick. They liquefy at a few degrees below the freezing point of water, and fall into a very fine white powder at an extremely low temperature. Note of Van Mons in the Annals of Chemistry, XXII. 27.

and evaporated in a glass retort, till it begins to deposit regular crystals. If the mass should consolidate ever so little by cooling, a small quantity of water is to be added, and it must be heated again to render it fluid. After the formation of a sufficient quantity of regular crystals, the fluid, which is very brown, is to be decanted, and the salt, after being suffered to drain, must be re-dissolved in the same quantity of water. The decanted liquor must be kept in a well closed bottle, and suffered to become clear by subsidence for several days. It must then be decanted for a second evaporation and crystallization. The process must be repeated as long as the crystals afford, with the least possible quantity of water, solutions perfectly limpid. These solutions are to be preserved in well closed bottles, to defend them from the access of air.

7. The greatest difficulty of this process arises from the facility with which the lixivium assumes the solid form. To obviate this inconvenience, a small portion of the lixivium may be concentrated to the point at which it becomes converted into a solid mass by cooling. The saturation of a lixivium considerably evaporated may be ascertained by throwing small pieces of this mass into it during its cooling. When these are no longer dissolved, it is a proof that the lixivium is at the required point. I have shewn, in a particular article on the crystallization of salts, the principles on which this practice is founded*.

8. This method of proceeding is absolutely indispensable in the crystallization of soda, which in other respects is made in the same manner as that of pot-ash. The alkali would not fail, without this management, to form a solid mass during its cooling.

9. With regard to the foreign salts which are mixed with the pot-ash, the greatest portion separates by crystallization after the first evaporation of the lixivium. The rest is separated during the second concentration by the continual skimming of the pellicle. The little which may remain with the pot-ash must precipitate, for want of water of solution, in a lixivium wherein the alkali itself is no longer dissolved but by its own water of crystallization.

10. The property of caustic alkalis to dissolve in highly rectified alcohol, with the exclusion of every foreign salt, would afford an excellent means of obtaining this salt very pure, if their mutual action did not afford a new source of impurity. For when an alkali absolutely pure and crystallized is dissolved in spirit of wine, even without heat, the fluid assumes a very brown colour, which becomes still deeper after decantation from the saline mass.

11. The crystallization of pot-ash is very different, accordingly as the crystals are formed with cold or heat. In the first case, the crystals obtained are octahedrons in groups, which contain 0.43 water of crystallization, and excite by their solution in water, even in the summer, a degree of cold very near the point of aqueous congelation. In the second case, crystalline transparent very thin blades, of extraordinary magnitude, are formed, which, by an assemblage of lines, in directions that cross each other to infinity, present an aggregate of cells or cavities, most commonly so perfectly closed that the vessel may be inverted without the escape of the smallest drop of the lixivium, though sometimes included to the quantity of an ounce or two. For this reason it is necessary to break this fine crystallization, that the fluid may run off. The crystals present in their regular formation rectangular tetragonal blades, which, as they contain little water of crystallization, produce a considerable degree of heat when dissolved in water.

* Chem. Annal. auf 1795, b. i. f. 6.

12. By exposing such alkaline crystals to a red heat in a very clean crucible, they become fused; and, after cooling in proper moulds, afford a lapis causticus as white as snow, and extremely caustic and deliquescent.

13. As the crystals and the lixivium, during the length of time required to drain the salt, may frequently become charged with a portion of carbonic acid, it is advisable, in order to avoid this inconvenience as much as possible, that the lixivium, as soon as it is brought to the requisite point of saturation, should be poured into a narrow-necked bottle, and well closed therein to crystallize. After the crystals are formed, the bottle is to be reversed, without opening, and kept at a temperature rather warm until the crystals are well dried. During the winter, the liquor, after the first crystallization, continues to crystallize without being submitted to a new evaporation, provided only that it be exposed to a temperature somewhat colder than that wherein the first crystals were formed*.

VII.

Experiments on Eau de Luce. By a Correspondent.

To Mr. NICHOLSON, Editor of "*The Journal of Natural Philosophy, Chemistry, and the Arts.*"

SIR,

OBSEVING in your second number a paper on the compound called Eau de luce, I here send you the result of some cursory experiments, which I was induced, for the benefit of my female friends, to make some years since on this article. You will perhaps yourself prosecute them farther than I have had an opportunity of doing.

1. Macquer's recipe, and that in the London Pharmacopœia, were both tried; but neither of them succeeded. I was however informed by a very intelligent apothecary, that the best eau de luce might be, and always was, made without any other ingredient than oil of amber, soap, alcohol and ammoniac: he added, that those who are judges of this article detect in a moment the presence of mastic, and esteem such as contains it of very little value. The proportions were accordingly varied. Different specimens of the oil were used—some of the first distillation, others twice rectified—but with no better success.—The best product was too highly coloured. When dashed against the sides of the phial, it did not flow back uniformly, but presented a somewhat greasy appearance, not much unlike that of a common emulsion with oil and an alkali; and after some days the oil separated like a cream on the top, and the liquor became semi-transparent.

2. Being assured from a number of enquiries, that mastic was the most ordinary basis of the emulsion, this was also tried, and succeeded tolerably at first. The milky appearance is

* Crystals of caustic pot-ash may also be obtained by the successive evaporation of a solution of the pot-ash of commerce until the appearance of a light pellicle. The first crystals which appear are the alkaline carbonate, or mild alkali, and the latter the pure pot-ash. In this way the lixivium is reduced into very regular crystals, to the very last portions of salt which it contains. Half an ounce, the residue of a solution of several pounds, still affords very regular crystals. It is even remarkable, that the latter crystallizations are formed with infinitely more facility than the first. I inserted this observation in a Memoir on the preparation of the carbonate of pot-ash for the alkaline sulphuric water of Colburn, which I addressed at the beginning of 1793 to the Société Philomatique de Paris, and has in part been printed in *L'Esprit des Journaux* for the same year. Note of Van Mons.

however

however not sufficiently permanent. After a certain time—which is longer in proportion to the weakness of the resinous solution, and the strength and causticity of the ammoniac—this also separates, and the mastic forms a hard adhesive coagulum which floats on the surface of the liquor. This effect is much accelerated by frequently opening the phial, but retarded by the addition of alcohol.

3. Elemi succeeds as well, or better. The same observations are applicable when this is used, as in the preceding case. The precipitate, however, when formed, subsides to the bottom of the liquor: It is more bulky, and not so hard as that of the mastic. It is not unlike that which is formed in such ink as contains much gum and alum.

4. A mixture of elemi and mastic in solution seems much superior to either ; and would perhaps remain suspended any length of time, if the ingredients were so apportioned as to make the compound precipitate as nearly as possible of the same specific gravity as the liquor. The best product was obtained nearly as follows ; but the ingredients not having been accurately weighed, the quantities are only given by estimation.—Digest ten or twelve grains of the whitest pieces of mastic, selected for this purpose and powdered, in two ounces of alcohol ; and, when nearly dissolved, add twenty grains of elemi. When both the resins are dissolved, add ten or fifteen drops of rectified oil of amber, and fifteen or twenty of essence of bergamot : shake the whole well together, and let the faces subside. The solution will be of a pale amber colour. It is to be added in very small portions to the best aqua ammoniæ puræ, until it assumes a milky whiteness—shaking the phial well after each addition, as directed by Macquer. The strength and causticity of the ammoniac are of most essential consequence. If, upon the addition of the first drop or two of the tincture, a dense opaque coagulated precipitate is formed—not much unlike that which appears on dropping a solution of silver into water slightly impregnated with common salt—it is too strong, and must be diluted with alcohol. A considerable proportion of the tincture, perhaps one to four, ought to be requisite to give the liquor the proper degree of opacity. I have frequently since made an eau de lucc in this way, in which no deposition has afterwards appeared.

5. The resin formed from oil of amber by the nitric acid was, after boiling it in water, tried for this purpose. It was itself of a light chocolate colour, and produced an opaque fawn-coloured liquor. Common yellow resin formed a coagulum like that of mastic, but almost instantly. Its colour was bad, and its factor too strong to be disguised.

6. Balsam of copal was tried, and appeared to be a very good substance for this purpose; but some other objects of enquiry intervening, the experiments were discontinued. Copal is, it is said, equal, if not superior, to any of the resins; particularly on account of its having a less unpleasant odour than most of them. I have not tried it myself; but, from the deserved reputation of some trading chemists who I know make their eau de lucc of it, I have no doubt of its answering extremely well*.

I am, Sir, your most obedient Servant,

12th June 1797.

J. F—:—:—:—:—R.

1. Such a fluid must consist of parts that differ in their refractive power, and are not smaller than a determinate magnitude. Newton's Optics, III. Prop. 4. a. The denser particles will descend with velocities nearly uniform.

VIII.

An Account of Experiments described, and in part repeated, at the Sitting of the National Institute of France, on the 15th Germinal, in the Year 4. By Citizens FOURCROY and VAUQUELIN; On Detonations produced by Concussion.*

THE energy and rapidity with which the super-oxygenated muriate of pot-ash inflames and burns most combustible substances, is the source from which the Citizens Fourcroy and Vauquelin have derived the new facts which they have communicated to the National Institute of France, and exhibited at the public sitting of the 15th Germinal, in the 4th year of the Republic. Several chemists had observed, that this salt, discovered by Berthollet about nine years ago, and which seems to contain the elements and phenomena of thunder, decrepitated and emitted small electrical sparks by friction, and spontaneously took fire with sulphur. And an unfortunate experiment made in October 1788, at Essône, had proved, that when ground with sulphur and coal, to convert it into gunpowder, it takes fire spontaneously. Such were the first observations made by Lavoisier, Pelletier and Van Mons, when Citizens Fourcroy and Vauquelin undertook to examine the series of effects of the super-oxygenated muriate of pot-ash on the several known combustible bodies. The following are the principal facts which they have discovered:

1. Three parts of super-oxygenated muriate of pot-ash, and one part of sulphur in powder, triturated in a metallic mortar, caused numerous and successive detonations to be heard, resembling the cracks of a whip, the report of a pistol, or even that of musquetry, according to the rapidity of the motion, or force of pressure applied. A few centigrams (or fifth parts of a grain) of the same mixture, struck briskly with a hammer on an anvil, exploded with a noise similar to that of a musket, and torrents of purple light were seen round the anvil. The same mixture thrown into concentrated sulphuric acid immediately takes fire, and burns without noise with a white dazzling flame.
2. Three parts of the salt, half a part of sulphur, and half a part of charcoal, afford stronger detonations than the foregoing, when triturated in the mortar, and a more considerable noise when struck on the anvil. The flame of this mixture, when caused to detonate, or when thrown into the sulphuric acid, is more rapid, strong and red than that of the preceding mixture.
3. Equal parts of super-oxygenated muriate of pot-ash, and of antimony in powder, fulminate by the stroke, and produce only reddish sparks with the sulphuric acid. Zinc at a light dose also fulminates with a white flame, but suffers no change by the sulphuric acid.
4. Metallic arsenic detonates very strongly by the stroke of the hammer, and takes fire with rapidity and extraordinary brilliancy by the contact of sulphuric acid. In this last

form, and cæteris paribus in the direct subduplicate ratio of their diameters. Atwood on Motion, Sect. V. Prop. 10. And 3. The magnitude, and perhaps density, of all chemical depositions are affected not only by the nature of the ingredients and the quantity of solvent, but very much by the order of mixture and other circumstances of manipulation. This last observation serves to account for the various results of these and many other experiments in which transparency, opacity, or colour, are required to be produced. N.

* Annales de Chimie, XXI. 235.

experiment a smoke arises which assumes the form of a crown, in the same manner as phosphorated hydrogenous gas when spontaneously inflamed in a calm atmosphere.

5. The sulphure of iron, or martial pyrites, inflames rapidly, but without noise, when triturated in a metallic mortar with the super-oxygenated muriate of pot-ash. This mixture, when struck on the steel anvil, detonates strongly with a red flame.

6. The red sulphure of mercury, or cinnabar, and the sulphurated oxides of mercury, detonate by the stroke with the super-oxygenated muriate of mercury (q. pot-ash); but they do not take fire with the sulphuric acid. Charcoal alone mixed with this salt has the same effects.

7. Sugar, the gums, fixed and volatile oils, alcohol, or ether mixed with the super-oxygenated muriate of pot-ash, so that these last liquid combustible substances formed a paste with the salt, have the property of fulminating very strongly by the stroke of the hammer. They all emit a very strong flame on detonation. These mixtures do not detonate nor take fire by simple trituration. Some of them take fire in the concentrated sulphuric acid. Their combustion in this case is slow and successive.

8. All the substances before mentioned, which, when mixed with the super-oxygenated muriate of pot-ash, take fire and burn in an instant, and with considerable noise, by the rapid pressure of the stroke of the hammer, produce a much stronger detonation when wrapped up in double paper, which compresses them together before the stroke.

9. The electric shock from a strong machine, by the charge of a battery of large surface, causes all the foregoing mixtures to detonate in the same manner as concussion; and in this case also they emit a strong light.

To all these facts the authors add, that it was already known that gunpowder detonates by a violent blow or sudden pressure; but they observe, that the stroke must be much stronger than is necessary to produce fulmination in all the mixtures of combustible substances with the super-oxygenated muriate of pot-ash, and that its detonation is far from being equally remarkable with those produced by the new salt.

With regard to the theory of this singular phenomenon, it appears to them to be the same as that published by Berthollet relative to the detonation of fulminating silver by the slightest contact. Pressure, and more particularly that which is effected in a very short time, as by a blow, favours the union of oxygen with the combustible body. This combination effected by the oxygen, separated all at once from the super-oxygenated muriate of pot-ash, is accompanied by a sudden expansion, and instantaneous formation of gaseous matters, which strike and compress the surrounding air with such velocity as to produce a considerable noise. The light, the vapour, and the odour peculiar to each combustible body made use of, prove that a true inflammation takes place, and that the strong detonation is owing to its violence and rapidity.

The inflammation produced by the concentrated sulphuric acid arises from the disengagement of the super-oxygenated muriatic acid in gas, by means of which the combustible matters mixed with the salt take fire still more rapidly than in the ordinary oxygenated muriatic acid gas.

IX.

A Memoir on the Combination of Oils with Earths, Volatile Alkali, and Metallic Substances.
By M. BERTHOLLET*.

SOAP agrees so far in its properties with neutral salts, that oil must form in the manner of acids a great number of combinations hitherto neglected. This notion led me to make the experiments I am about to relate.

The action of oil upon calcareous earth has long been known; but Mr. Costel was the first who described the method of accurately combining these two substances, by pouring a solution of soap into lime water †.

The lime unites to the oil of the soap, and forms a combination which is insoluble, and may be retained on the filter while the caustic alkali is set at liberty, and may be separated by evaporation. It retains a small quantity of oil, which, according to Mr. Thouvenel, may be washed off with spirit of wine ‡. This chemist remarks, in his analysis of the waters of Contrexeville, that the caustic alkali cannot decompose calcareous soap; so that we may rigorously affirm, that oil has a stronger affinity to calcareous earth than to fixed alkali; but according to the same chemists, if the effervescent fixed alkali be poured on calcareous soap, this last is decomposed, the alkali unites with the oil, and the calcareous earth becomes disengaged at the same time that it acquires the property of effervescence. The discoveries made since this dissertation was written, explain what happens upon this occasion. A double decomposition and recombination take place. The cretaceous acid unites with the calcareous earth of the oleo-calcareous combination, and the oil of the same combination unites with the alkali thus deprived of its cretaceous acid.

M. Thouvenel on this occasion makes a reflection of too great importance to be omitted. Physicians often prescribe the use of soap and lime together, or lime water, without attending to the change and decomposition which result from the mixture of these two substances. For, in this case, it is the caustic alkali disengaged from the soap which becomes the active medicament; and consequently that the effects of such mixtures must vary according to the proportions and other concomitant circumstances.

I have likewise examined the effects of volatile alkali upon calcareous soap. The caustic volatile alkali had no stronger action upon this combination than the caustic fixed alkali; but the volatile effervescent alkali decomposed it in the same manner as the effervescent fixed alkali. The volatile alkali assumed the appearance of an oil, and the earth remained at the bottom in the effervescent state.

After having decanted this saponaceous substance, I evaporated the superfluous volatile alkali at a gentle heat, and there remained a soap of a more pungent taste than common soap, and somewhat less consistent. It became decomposed by long exposure to the air. Spirit of wine dissolves it well, but the quantity taken up by water is extremely small. This last property convinced me that there was no need of so complicated and so long a process to

* Acad. Par. 1780.

† Analyse des Eaux de Pongues.

‡ Qu. M. Berthollet has since shewn the caustic alkali is also soluble in that fluid. See also Philos. Journal, I. 165. N.

make this soap; and that, for this purpose, I might make use of the action of double affinities in a different manner.

I therefore mixed a solution of common soap with a solution of sal ammoniac, and I saw clots formed at the same instant, which consisted of the ammoniacal soap, and were retained on the filter; so that the fixed alkali of the soap united with the marine acid, while the volatile alkali combined with the oil. Strongly persuaded as I am, that new medicines ought not to be proposed but with extreme circumspection; and that we should rather endeavour to diminish than increase the list of *materia medica*, I am nevertheless tempted to propose the medical use of this soap, which must possess more active virtues than the common soap, and which has the advantage over that of Starkey, of a very easy and speedy composition, uniformity of properties, and is capable of being preserved in close vessels. I am aware that use has been made in medicine of a mixture of volatile alkali and oil, of which a combination is attempted to be made by agitation or trituration; and that a mixture of this kind is to be found in the London Pharmacopeia, under the name of volatile liniment. But the combination obtained in this way is very imperfect, and totally different from the soap here described, as may be easily seen by inspection.

When common soap is mixed with selenitic water, two decompositions and two new combinations take place, as Mr. Costel has proved. The alkali of the soap unites with the acid of the selenite, and the earth of the selenite combines with the oil of the soap, forming an insoluble oleo-calcareous matter in flocks, which cannot answer the purposes of ordinary soap. Hence the selenitic waters have been distinguished by the name of hard water. Selenite, however, is not the only substance proper to form the oleo-calcareous combination; for every solution of this earth is equally proper. The solution in the marine or nitrous acid may be used for this purpose. When soap is therefore decomposed in hard waters, the effect depends not only on the selenite and the calcareous earth held in solution by carbonic acid, but likewise upon all the salts with basis of lime, or even magnesia, which may exist in the water, as will hereafter be shewn.

The mixture of a solution of soap and a solution of Epsom salts afforded a combination of the utmost whiteness. It is unctuous, dries with difficulty, and preserves its white colour after desiccation. It is insoluble in boiling water, but has nevertheless a decided taste of soap. Expressed oil, as well as spirit of wine, dissolves it in considerable quantity; when water is added to the solution in the latter fluid, it becomes milky. This combination melts with a moderate heat, and forms a transparent mass slightly yellow and very brittle. The oleo-calcareous combination cannot be fused but very imperfectly, and at a much stronger heat.

I have combined oil with clay by mixing a solution of alum with a solution of soap. The result of this mixture was a flexible combination, soft to the touch, which preserves its suppleness and tenacity in drying. It appeared to me to be insoluble in water, spirit of wine, and oil. It very readily enters into fusion, after which it exhibits a mass of a beautiful transparency rather yellow.

The solution of ponderous earth in marine acid, afforded with soap a combination nearly the same in appearance and properties as the calcareous compound.

The very simple method used in these experiments to form combinations of oils and earths, was attended with equal success when applied to metallic substances.

When a mixture of the solutions of soap and corrosive sublimate was made, the fluid assumed the appearance of milk, but soon after exhibited small coagula. It is almost impossible to filter this liquor, but the greatest part of the mercurial combination is slowly deposited. This deposition may be accelerated by means of spirit of wine. The same combination may be much more readily effected with the nitrous solution of mercury*.

The oleo-mercurial combination is viscid, dries difficultly, dissolves very well in oil, and very sparingly in spirit of wine. It loses its white colour by exposure to the air, and acquires a slate colour, which gradually becomes deeper, more especially if it be exposed to the sun or any other heat. It readily becomes soft and fluid. It must be distinguished from the mercurial ointment used in medicine, for in this the mercury possesses the metallic state; whereas, in our combination, the mercury is in the state of calx, and forms with the oil a true combination, which perhaps might prove useful in medicine.

The combination of oil and zinc, which I made by means of white copperas, is of a white colour inclining to yellow. It dries speedily, and becomes friable. The combination of cobalt and oil, made by means of the solution of regulus of cobalt in aqua fortis, is of a dull leaden colour, and dries with difficulty, though its parts are not connected together. Towards the end of the precipitation some coagula of a green colour, and much more consistent, fell down. I apprehend that this was a combination of oil and nickel; for it is known that this semi-metal is almost always contained in the regulus of cobalt, and that it forms green solutions with acids, while those of cobalt are red. I could not establish my conjecture, because I was unable to procure nickel. But if it should prove well founded, this process will afford an easy method of separating the two metallic substances.

I made the combination of oil and tin by means of the solution of this metal in aqua-regia. It is white, not fusible when exposed to heat, like all the other oleo-metallic combinations; but it is decomposed without any change in the form of its parts. I attribute this circumstance to the great quantity of metal contained in this combination, as we shall see.

The oleo-martial combination is of a reddish brown colour, tenacious, and easily fusible. When spread upon wood it soaks in and dries. It is easily soluble in oil, more particularly oil of turpentine, to which it gives a good colour, which may prove useful as a varnish.

The oleo-cupreous combination which I have made by means of blue vitriol, is resinous to the touch while moist, is of a green colour, and becomes dry and brittle. When digested in spirit of wine its colour becomes deeper, and it liquefies, but does not dissolve in the cold. Ether renders its colour deeper and more beautiful, instantly liquefies it, and dissolves a considerable quantity. This combination is abundantly soluble in oils, to which it communicates a pleasant green colour.

The combination of oil and lead, made by means of the solution of sugar of lead, is white, tenacious, and very adhesive when heated. The union of oil and the lead is not so intimate in the diachylon (or direct solution of litharge in oil) as in this combination; for this last, when fused, is transparent, and becomes rather yellowish if the heat be somewhat increased; but the diachylon is opaque, and the oil which enters into its composition

* Probably with different effects, accordingly as the solution, supposed to be saturated, is made with or without heat. N.

has acquired an acrid property from the heat it has been subjected to. It is probable, therefore, that the combination I have formed might in some cases be advantageously substituted for that compound. Geoffroy formerly remarked, in the Memoirs of the Academy for 1741, that the combination of oil which he made in the manner of plasters, formed a kind of soap.

The combination of oil and silver is white when first made; but after a few instants' exposure to the air, it assumes a red tinge, which no doubt depends on the facility with which this metal yields its oxygen to all combustible matters*. The change of colour in the oleo-mercurial combination appears to me to depend on the same principle. When the combination of silver is fused, its surface becomes covered with a very brilliant iris; and beneath the superficies it is black.

The combination of gold and oil partly floats on the mixture in the form of a cream, which is at first white, soon after which it assumes a dirty purple colour. It dries with difficulty, and adheres to the skin, so that it is difficult to efface the impression.

I have combined the metallic principle of manganese by mixing a solution of soap with a solution of manganese in the marine acid. This combination is at first white. It assumes in the air a reddish colour of peach blossoms, which becomes more and more deep. It speedily dries to a hard brittle substance, and by liquefaction it assumes a brown blackish colour.

In order to ascertain whether essential oil had likewise the property of combining with metallic substances, I mixed a solution of Starkey's soap newly made with a solution of vitriol of copper. The same thing happened as with the common soap, excepting that the combination was rather a lighter green, and more friable. Black soap, which is said to be made with whale-oil, afforded me, with the solution of vitriol of copper, a combination which, compared with that obtained by means of common soap, is rather of a deeper green, remains somewhat softer, and possesses a very disagreeable smell.

I was desirous of forming a soap with caustic alkali and rectified animal oil, with the intention of forming other combinations afterwards; but this oil formed no union with alkali.

We have seen that the calcareous earth (and it is the same with ponderous earth) has more affinity with oil than fixed alkali, and this has more than magnesia; but the combination of magnesia is not decomposed by the caustic volatile alkali, so that magnesia follows the fixed alkali in the order of affinity. Afterwards comes the volatile alkali, which decomposes all the metallic combinations with more or less facility. It totally dissolves the combination of silver; but the mercurial combination is that which appeared to me most strongly to resist decomposition. With regard to clay, its combination is decomposed by the caustic volatile alkali, even more readily than the metallic combinations. Whence I infer that it may be placed after the metallic substances.

Oils by expression did not appear to dissolve the calcareous and argillaceous combinations; oil of turpentine dissolved only a small portion of the calcareous combination, but rather more of that of clay, with which it formed a jelly. Ardent spirit dissolves some of the oleo-metallic combinations without heat. It requires heat to dissolve some others;

* The learned author will certainly approve my translating the only theoretical passage in his memoir according to the simple doctrine he now maintains. In the original the effect is ascribed to the attraction of silver to phlogiston. N.

but though by this means it attacks them all, it nevertheless dissolves much less than the oils, particularly the oil of turpentine.

I calcined part of the combinations here described to determine the quantity of earth or metallic calx they contained. I employed half an ounce of each. The residue of magnesia amounted to 32 grains, which did not effervesce with an acid; that of calcareous earth was 36 grains, which effervesced; that of clay 28 grains; of iron 48 grains; of copper 33; of zinc 42; of manganese 40; the residue of silver amounted to 30 grains in the metallic state; that of tin 1 gros 7 grains of the reduced metal; that of lead formed by calcination a pyrophorus. When these combinations are to be made, it is proper to use the solutions in a saturated state; for, if there be an excess of acid, part of the soap is decomposed by this excess; a portion of the oil floats above; but part of the oil enters the combination which is formed, and alters its properties. In whatever acid an earth or a metal may be dissolved, the same combination is always formed by means of soap; nevertheless this combination sometimes exhibits different appearances. Thus the oily compound of mercury is much more tenacious and adhesive when corrosive sublimate is used, than when the nitrous solution of the metal has been employed *.

When the filtered liquor, after forming the oily combination, is evaporated, a salt is obtained of the particular kind which results from the alkali of the soap, and the acid of the solution made use of. M. Costel made the experiment with selenite, and I repeated it with Epsom salt and vitriol. I first evaporated to perfect dryness, after which I dissolved the residue in filtered water, and then evaporated and crystallized †.

X.

Analysis of a Memoir of Citizen BONHOMME on the Nature and Treatment of Rachitis, or the Rickets ‡.

SINCE the object of the Society of Medicine, in the offer of its prizes, is to bring our knowledge of the healing art to the greatest degree of precision and accuracy of which it is susceptible, this company must necessarily direct its attention to the progress of the chemical analysis of animal matters, and the information which sooner or later it must afford respecting the nature and treatment of many disorders.

Those disorders which alter the composition of the fluids and the consistence of the solids, are assuredly the first which ought to be determined by chemical research. The rachitis is of this number. The change which the bones undergo in this disorder, has long been attributed to the action of an acid on their substance; but this opinion was grounded on mere supposition and remote analogy. The subject is treated in a new manner in the memoir, of which the Society has directed us to present the results; and the experiments as

* Subsequent experiments have shown that a much greater quantity of oxygen is present in the former than in the latter case. N.

† I suppose M. Berthollet's experiments were made with soap of vegetable expressed oil. The common soaps of this country contain animal fat. N.

‡ This Memoir was read to the National Society of Medicine at Paris. The analysis was made by Hallé, and is inserted in the seventeenth volume of the *Annales de Chimie*, whence this translation is made.

well as the observations upon which the author grounds his inferences, present to view matters of fact sufficiently remarkable to afford a presumption that new experiments will confirm their truth. We announce them on the present occasion with expressions of doubt, because these are the expressions which the wisdom and modesty of the author have suggested, and because we think with him, that facts of this nature cannot be stated as if fully proved, but in consequence of multiplied experiments.

The principal notions which constitute the basis of this memoir are the following :

1. According to the author, the nature of the rachitic disorder arises on the one hand from the development of an acid approaching in its properties to the vegetable acids, particularly the oxalic, and on the other from the defect of phosphoric acid, of which the combination with the animal calcareous earth forms the natural basis of the bones, and gives them their solidity.

Whence it follows, that the indication resulting from this proposition, if once adopted, would be, that the treatment of rachitis must depend on two principal points, namely, to prevent the development of the oxalic acid, and to re-establish the combination of the phosphoric acid with the basis of the bones to which they owe their solidity.

2. The author proves by experiments and observations, in the first place, that alkaline lotions of the parts affected with rachitis contribute to their cure; next, that the calcareous phosphate taken internally is really transmitted by the lymphatic passages, and contributes to ossification; and lastly, that the internal use of calcareous phosphate, whether alone or combined with the phosphate of soda, powerfully contributes to restore the natural proportions in the substance of the bones, and accelerate the cure of rachitis.

On the present occasion we shall only collect the proof of these fundamental facts, which form the absolutely new part of this memoir, in which the author has besides inserted an excellent abridgement of all that had been ascertained before him on the nature of the bones, the rachitis, and the treatment of this disorder.

With regard to the first parts, the author endeavours to establish these two propositions :
1. That the calcareous phosphate is wanting in the bones of those who are disordered with rachitis. 2. That the development of oxalic acid is the cause of this alteration.

We must not conceal, that this ingenious part of his memoir contains rather views than absolute proofs of the nature of the rachitic acid. The author himself declares, that he was not provided with the necessary means to establish an exact and complete analysis. He therefore presents his ideas in this respect, merely as conjectures approaching to the truth.

The effect of the action of acids upon bones, was before known; that is to say, that when deprived of calcareous phosphate, and reduced to the gelatinous parenchyma which forms one of their elements, they lose their consistence and become flexible. Hence it was already conjectured by various physicians, that the rachitis was the effect of a peculiar acid.

A disposition to acceffence in the first passages is observable in all infants. The odour which characterizes this acceffence is often manifest in their breath, and even their perspiration. The bile corrects this disposition; but in general the bile is wanting in rachitic infants. It does not colour their excrements, and the acids accordingly are developed in a very decided manner. They disturb the circulation, and attack and soften the bones. As it is by defect of animalization that these acids develop themselves, it follows that their character

character is analogous to the fermentescible vegetable acids, and more or less to the oxalic acid; and that, on the contrary, the animal acid or phosphoric acid ceases to be formed, and to unite with the animal calcareous earth; whence they are deprived of the principle of their solidity. This is the theory of Citizen Bonhomme.

In order to establish this doctrine upon precise experiments, it was requisite to analyse rachitic bones comparatively with those of healthy individuals of the same age; and as it is known that the urine of rachitic subjects deposits a great quantity of a substance of sparing solubility and earthy appearance, it would have been advantageous to have joined a complete analysis of this urine and its sediment.

Citizen Bonhomme not being provided with the means sufficient to make these analyses, and being besides of opinion that such rachitic bones as are destroyed by this malady exist in a progressive state of change, which might render their analysis scarcely susceptible of comparison, limited himself to a collection of some of the most remarkable phenomena of the urine of the aged, the adult, and infants in the healthy state, of infants in the rachitic state, and of patients after the perfect cure of this disorder. From these observations he has deduced several important results.

It is known that when the urine contains disengaged phosphoric acid, as happens to aged individuals, and in some peculiar circumstances of the system, if lime water be poured in there is a speedy deposition of calcareous phosphate. It is also known, that when a solution of the nitrate of mercury is poured to the fresh urine of adults, a rose-coloured precipitate is formed, which is a phosphate of mercury produced by the decomposition of the phosphates contained in the urine. These two proofs are therefore extremely proper to ascertain the presence of phosphoric acid, whether free or combined, in a fluid which in its natural state contains a remarkable proportion. Besides this principle, the urine deposits more or less of sediment, either gelatinous or of an earthy appearance; and lastly, by evaporation, a saponaceous and saline extract, in greater or less abundance, is obtained by evaporation. By means of these four methods of examination, the author has ascertained the following facts:

1. In the healthy state, the sediment naturally deposited by urine is almost totally gelatinous in the infant and the adult, and in the aged individual it is surcharged with an abundant sediment of an earthy appearance similar to the earth of bones, which consequently is calcareous phosphate.

2. The quantity of brown saponaceous saline extract afforded by evaporation is greater in proportion to the age.

3. The presence of disengaged phosphoric acid, as shewn by lime water, is none in the urine of infants, scarcely perceptible in that of adults, but very remarkable in that of old men. For two ounces of this last urine afforded by this means ten grains of phosphate of lime.

4. The decomposition of the phosphates by nitrate of mercury is not seen in the urine of infants; an abundant precipitate of a light rose-colour is produced in this way from the urine of adults; and in that of old men this precipitate is always of a grey colour, and very abundant.

Hence Citizen Bonhomme concludes, that the phosphoric acid, whether at liberty or combined, does exist in the urine of healthy individuals in proportion to the destruction of the solids by age, and that it increases with the age.

With

With regard to the urine of rachitic subjects, the most remarkable facts are, 1. The abundant and apparently earthy sediment it deposits (spontaneously) is different from that of old men, by its colour, which is grey and does not resemble phosphate of lime, and also by its much greater quantity. For a pound of this urine let fall two gros, whereas the same quantity of the urine of old men deposited only 45 grains.

2. The extract left by evaporation is likewise much more considerable than in other urine. It is one third more in quantity than the extract afforded even by the urine of aged persons.

From these two first observations it follows, that the solids in rachitic subjects are destroyed with much more rapidity than even in old men; and that they afford a much more abundant portion of waste to the urine.

3. The light deposition occasioned by lime water in the urine of rachitic subjects is very small in quantity, brown, gelatinous when fresh, and pulverulent when dry. It does not at all resemble calcareous phosphate.

4. The deposition formed by the solution of mercurial nitrate is not abundant, neither of a rose colour as in the urine of adults, nor grey like that of old men. It is always white, and consequently has no external resemblance to the phosphate of mercury. The author affirms that it resembles a mercurial oxalate.

Lastly, the urine of the same rachitic subjects when cured, exhibits again all the characters observed in the urine of healthy children.

We shall not add to the reflections of the author. In effect, though these first observations are curious, they are incomplete. We offer them to physicians simply as the elements of an investigation which it is of importance to continue and bring to perfection. We shall therefore proceed to the curative and experimental parts of the memoir.

We must remark, however, in this place, that the author presents in his work a judicious and methodical exposition of the whole treatment of the rachitis by other physicians; that he estimates the value of each remedy, and every part of the treatment, from the circumstances to which they are applicable, the degrees of the malady in which they are to be admitted, and the indications they answer. Setting aside, therefore, every thing which is not peculiarly his own, we shall in this place attend only to that which relates to the use and effects of calcareous phosphate and alkaline lotions.

One of the facts which it was of the utmost importance to establish, was the transition of the calcareous phosphate from the intestinal passages, into those of circulation and secretion. Fourcroy had already well ascertained that the serum of milk contains this salt naturally. Vauquelin had proved its existence, as well as that of pure soda, in the seminal fluid; but was it possible that it could pass unaltered from the stomach and intestines into the vessels which contain the blood and lymph? Could it by this means apply itself to the bones? This was to be ascertained by experiments.

[To be continued.]

XI.

On the Nature of the Diamond. By SMITHSON TENNANT, Esq. F. R. S. *

SIR Isaac Newton having observed that inflammable bodies had a greater refraction in proportion to their density than other bodies, and that the diamond resembled them in this

* Philosophical Transactions, 1797, p. 123.

property, was induced to conjecture that the diamond itself was of an inflammable nature. The inflammable substances which he employed were camphor, oil of turpentine, oil of olives, and amber; these he called "fat, sulphureous, unctuous bodies;" and using the same expression respecting the diamond, he says, "it is probably an unctuous body coagulated." This remarkable conjecture of Sir Isaac Newton has been since confirmed by repeated experiments. It was found, that though the diamond was capable of resisting the effects of a violent heat when the air was carefully excluded, yet, that on being exposed to the action of heat and air it might be entirely consumed. But as the sole object of these experiments was to ascertain the inflammable nature of the diamond, no attention was paid to the products afforded by its combustion; and it still therefore remained to be determined, whether the diamond was a distinct substance, or one of the known inflammable bodies. Nor was any attempt made to decide this question till M. Lavoisier, in 1772, undertook a series of experiments for this purpose. He exposed the diamond to the heat produced by a large lens, and was thus enabled to burn it in close glass vessels. He observed that the air in which the inflammation had taken place had become partly soluble in water, and precipitated from lime-water a white powder, which appeared to be chalk, being soluble in acids with effervescence. As M. Lavoisier seems to have had little doubt that this precipitation was occasioned by the production of fixed air, similar to that which is afforded by calcareous substances, he might, as we know at present, have inferred that the diamond contained charcoal; but the relation between that substance and fixed air was then too imperfectly understood to justify this conclusion. Though he observed the resemblance of charcoal to the diamond, yet he thought that nothing more could be reasonably deduced from their analogy, than that each of those substances belonged to the class of inflammable bodies.

As the nature of the diamond is so extremely singular, it seemed deserving of further examination; and it will appear from the following experiments that it consists entirely of charcoal, differing from the usual state of that substance only by its crystallized form. From the extreme hardness of the diamond, a stronger degree of heat is required to inflame it, when exposed merely to air, than can easily be applied in close vessels, except by means of a strong burning lens; but with nitre its combination may be effected in a moderate heat. To expose it to the action of heated nitre free from extraneous matters, I procured a tube of gold, which, by having one end closed, might serve the purpose of a retort; a glass tube being adapted to the open end for collecting the air produced. To be certain that the gold vessel was perfectly closed, and that it did not contain any unperceived impurities which could occasion the production of fixed air, some nitre was heated in it till it had become alkaline, and afterwards dissolved out by water; but the solution was perfectly free from fixed air, as it did not affect the transparency of lime-water. When the diamond was destroyed in the gold vessel by nitre, the substance which remained precipitated lime from lime-water, and with acids afforded nitrous and fixed air; and it appeared solely to consist of nitre partly decomposed, and of aerated alkali.

In order to estimate the quantity of fixed air which might be obtained from a given weight of diamonds, two grains and a half of small diamonds were weighed with great accuracy, and, being put into the tube with a quarter of an ounce of nitre, were kept in a strong red heat for about an hour and a half. The heat being gradually increased, the nitre was in some degree rendered alkaline before the diamond began to be inflamed; by which means almost all the fixed air was retained by the alkali of the nitre. The air which

came

came over was produced by the decomposition of the nitre, and contained so little fixed air as to occasion only a very slight precipitation from lime-water. After the tube had grown cold, the alkaline matter contained in it was dissolved in water, and the whole of the diamonds were found to have been destroyed. As an acid would disengage nitrous air from this solution as well as the fixed air, the quantity of the latter could not in that manner be accurately determined. To obviate this inconvenience, the fixed air was made to unite with calcareous earth by pouring into the alkaline solution a sufficient quantity of a saturated solution of marble in marine acid. The vessel which contained them, being closed, was left undisturbed till the precipitate had fallen to the bottom; the solution having been previously heated, that it might subside more perfectly. The clear liquor, being found by means of lime-water to be quite free from fixed air, was carefully poured off from the calcareous precipitate *. The vessel which was used on this occasion was a glass globe, having a tube annexed to it, that the quantity of the fixed air might be more accurately measured. After as much quicksilver had been poured into the glass globe containing the calcareous precipitate as was necessary to fill it, it was inverted in a vessel of the same fluid. Some marine acid being then made to pass up into it, the fixed air was expelled from the calcareous earth, and in this experiment, in which two grains and a half of diamonds had been employed, occupied the space of a little more than 10.1 ounces of water.

The temperature of the room when the air was measured was at 55°, and the barometer stood at about 29.8 inches.

From another experiment made in a similar manner with one grain and a half of diamonds, the air which was obtained occupied the space of 6.18 ounces of water; according to which proportion, the bulk of the fixed air from two grains and a half would have been equal to 10.3 ounces.

The quantity of fixed air which was thus produced by the diamond does not differ much from that which, according to M. Lavoisier, might be obtained from an equal weight of charcoal. In the Memoirs of the French Academy of Sciences for the year 1781, he has related the various experiments which he made to ascertain the proportion of charcoal and oxigene in fixed air. From those which he considered as most accurate, he concluded that 100 parts of fixed air contain nearly 28 parts of charcoal and 72 of oxigene. He estimates the weight of a cubic inch of fixed air, under the pressure and in the temperature abovementioned, to be .695 parts of a grain. If we reduce the French weights and measures to English, and then compute how much fixed air, according to this proportion, two grains and a half of charcoal would produce, we shall find that it ought to occupy very nearly the bulk of 10 ounces of water.

M. Lavoisier seems to have thought that the aërial fluid produced by the combustion of the diamond was not so soluble in water as that produced from calcareous substances. From its resemblance, however, in various properties, hardly any doubt could remain that it consisted of the same ingredients; and I found, upon combining it with lime, and exposing it to heat with phosphorus, that it afforded charcoal in the same manner as any other calcareous substance.

• If much water had remained, a considerable portion of the fixed air would have been absorbed by it. But by the same method as that described above, I observed that as much fixed air might be obtained from a solution of mineral alkali, as by adding an acid to an equal quantity of the same kind of alkali. T.

XII.

Useful Notices respecting various Objects.—Improvement of Telescopes.—Imperfections of Optical Glafs.—Purification of Mercury.

THE eye has often been described with admiration by opticians as a perfect instrument, as well as by others in a more loose way, in illustration of the argument from final causes. When the correction of the aberrations from sphericity and colour began to be investigated, it was presumed that the arrangement and figure of its parts were directed to remedy these defects; a notion which has since been sufficiently refuted by Drs. Maskelyne* and Blair†. If the general remarks had ever been extended to the improvement of optical instruments, instead of having been applied to them subsequent to the periods of actual discovery; it might afford matter for surprise, that the most variable of all its adjustments, namely, that of aperture, has never yet been introduced into our artificial combinations. When we look towards a window or any luminous object, the iris immediately contracts; and on the contrary, when we direct our sight to the inner part of a room, or to any obscure place, the aperture for the reception of light is as speedily enlarged. To what extent this enlargement may be carried in situations of extreme darkness, cannot be observed; but it is certain, that in the observable variations of the human eye, the aperture is thirty times as large at one time as at another; and that in the cat the proportion is much greater than an hundred to one. Hence we might reasonably infer, on general principles, that the distinctness of vision through telescopes would be improved, if this adjustment also were added to that of mere focal distance.

Every attentive observer must have taken notice, that light is of as much consequence to artificial vision as magnifying power. Distant woods and other land objects are invisible to an high magnifying power for want of light, when the same objects may be distinctly seen with a lower. Luminous objects are seen very perfectly by a small aperture, though coloured by a greater: but objects less enlightened will receive all the advantage of superior distinctness from greater light, when the fringes of colour may be too faint to be of any consequence. From these circumstances it frequently happens, that an achromatic perspective, admitting plenty of light, may exhibit land objects, especially in the evening, to much greater advantage than a reflector, admitting less light but performing its office with more accuracy; whereas, on the other hand, the latter instrument shall have greatly the advantage when tried upon a planet.

An ingenious mechanic will find little difficulty in contriving an artificial iris. Suppose a brass ring to surround the object end of the telescope, and upon this let eight or more triangular slips of brass be fixed, so as to revolve on equi-distant pins passing through each triangle near one of its corners. If the triangles be slid inwards upon each other, it may readily be apprehended that they will close the aperture; and if they be all made to revolve or slide backwards alike, it is clear that their edges will leave an octagonal aperture, greater or less according to circumstances. The equable motion of all the triangles may be produced either by pinions and one concave toothed wheel, or by what is called snail-work. Another kind of iris more compact may be made by causing thin elastic slips of brass to slide along

* Philof. Transf. LXXIX. Art. 21.

† Edinburgh Transf. II.

parallel to the tube, and be conducted each through a slit in a brass cap which shall lead them across the aperture in a radial direction. It is probable also that the artist who shall carry these hints into effect, may also think of several other methods.

2. Imperfections of Optical Glafs.

THE only sure method of ascertaining whether glafs be fit for optical purposes, is to give it the true figure of a lens, and examine this by the test of a high magnifying power. It is desirable however to make some selection by trial previous to this labour. A lens may be supposed to consist for the most part of a substance of uniform refractive density, but more or less mixed with glafs of another kind, which exhibits the appearance of spots or veins which interrupt the regular course of the light. Where this interruption is produced by considerable masses, the imperfections may be seen by the naked eye; but where the masses are smaller, the glafs may appear very clear, and yet be less fit for optical purposes than such as may contain several visible imperfections. From these considerations it appeared probable to me, that glafs might be advantageously examined by the magnifier applied near its surface.

An exceedingly clear, brilliant, yellow-tinged, plano-convex lens, of about four inches focus, was examined by looking at a candle through it, and a lens of half an inch focus next the eye. When the candle was about two feet distant, I observed a prodigious number of small erect images of candles of various sizes, which became spots at a greater distance of the large lens from the eye. Removing to the distance of seven or eight feet from the candle, the small images became luminous spots, some of them surrounded by circles of alternate light and darkness. At this distance, the spots, by removing the large lens further from the eye, were rendered much more perceptible.

Several other lenses were viewed, which exhibited a few images; but in a white common spectacle glafs of twelve inches focus, the images appeared so numerous, that the lens resembled the waved window-glafs, and, at a distance, appeared prodigiously spotted. This lens had nothing of an opaque appearance, but the light passed through it too irregularly to give a distinct picture in the camera obscura.

A good greenish object-glafs of six feet focus, which has a large vein or two across it, produced no more effect in these circumstances than if it had not been interposed between the eye and the candle.

From the circumstances of these experiments, we may infer that the images are produced by spherical particles of less density than the rest of the glafs. We may suppose them to have been flint, the specific gravity of which is about 2.7, which is nearly the same as that of green or crown glafs, but is one-fifth less than that of flint-glafs taken at 3.2.

When the full moon was looked at in this manner, through the first-mentioned lens, the images were considerably distinct, and all nearly of the same size. Their magnitude was about one two-hundredth part of an inch, or one-fifth part of the size of a bubble in the glafs, which, by estimate, might have a diameter of one-fortieth of an inch.

3. Purification of Mercury.

MERCURY which is considerably adulterated may be most conveniently and effectually purified by distillation. But many persons, such as mathematical instrument-makers, water-

water-gilders, and others, who have occasion to use pure mercury, may reject such as contains very little adulteration for want of the chemical apparatus to distill it. The process by agitation, first discovered by Dr. Priestley, is far from being generally known or used; but will, no doubt, prove an useful acquisition to many operators.

This process is grounded on the fact, that the metals with which mercury is usually contaminated, become converted into a black powder by partial calcination when agitated with respirable air. Take a glass vial with a ground stopper* (such being generally pretty strong), capable of containing 10 or 12 ounces of water, and fill about one-fourth of it with the foul quicksilver; then putting on the stopper, let the bottle be held inverted with both hands, shaking it violently by striking the hand that supports it against the knee. After twenty or thirty strokes, take out the stopper, and blow into the vial with a pair of bellows to change the air. For the purer the air the faster the process advances. After a short time, if the mercury be very foul, the surface will not only become black, but a great quantity of the upper part will appear as if coagulated. In this situation the vial is to be inverted; and covering the mouth of it with the finger, all the mercury that will flow easily is to be let out, and the black coagulated part put into a cup by itself. The running mercury may be separated from the black powder, by pressing this mass repeatedly with the finger. This mercury is to be added to the rest; in order to be agitated again.

The process must be repeated till no more black matter can be separated; and it is not a little remarkable, that the operator will be at no loss to know when the operation is completed. For the same quantity of lead seems to come out of it in equal times of agitation, and consequently the whole becomes pure at once. The sound of the pure mercury is louder and more harsh than while it contained the adulteration; so that by this criterion the end of the process is easily distinguished.

Dr. Priestley purposely dissolved both lead and tin in a large quantity of mercury, which he afterwards purified in this way, and found by subsequent distillation, that it had been rendered quite pure. I have myself used this process, which I find easy and convenient. The last portions of black matter, which give the mercury a dusty look, are readily separated by the paper funnel as usual.

XIII.

A Memoir containing some Results arising from the Action of Cold on the Volatile Oils, and an Examination of the Concretions found in several of those Oils†. By Cit. MARGUERON, Member of the Société des Pharmaciens at Paris.

THE volatile or essential oils obtained from vegetables owe their fluidity, in the same manner as water, to the portion of caloric they contain. Several of them lose this state at 8° above ‡ the term of aqueous congelation (50° Fahrenheit), and assume the concrete form;

* Experiments and Observations on Air, abridged and methodized, by Joseph Priestley, LL. D. F. R. S.

III. 439.

† Annales de Chimie, XXI. 174.

‡ The temperatures in this paper are according to the scale called Reaumur's, in which the freezing point is marked 0°, and the boiling water point 80°. N.

others,

others, on the contrary, preserve their fluidity at a much lower term. It is upon these last chiefly that I have directed the operation of cold, with two objects of research: the first, to observe the crystallization which these oils might exhibit by the loss of their caloric; the second, to ascertain the other phenomena which might arise during the experiments.

These experiments, though apparently simple, are not however without their difficulties.

When we reflect on the different characters of volatile oils, we find in general, that the oils obtained from the plants of hot climates are heavier and less limpid than those of colder regions, and that they contain a salt capable of crystallization; that the oils extracted from the same plants of our climates vary in colour and fluidity, according to the nature of the soil in which the plants have vegetated, the care in cultivation, the influence of light, the different states of growth, and whether they be recent or dry when subjected to distillation: that when extracted from the vegetable, they are exposed by the nature of their principles to different alterations, according to the more or less effectual action of the air, the contact of light, whether kept in bottles entirely or only in part filled, or closed with a ground stopper or with a cork. These differences will easily shew that the results must be subject to variation: and I have accordingly been very careful to use only such volatile oils as were newly extracted from fresh vegetables; with the exception of those from exotic plants.

Concerning the Action of Cold on several Volatile Oils.

I HALF filled several very thin bottles with the volatile oils of peppermint, orange flowers, lemon-peel, bergamot, buds of lavender and of thyme, being plants of our climates; oil of turpentine, a resinous substance, and oil of cinnamon, a foreign bark.

These bottles, closed with ground stoppers, were exposed for several days on a terrace where a mercurial thermometer marked 11° beneath the freezing point (7° Fahrenheit).

At this degree of cold I observed that there was formed at the upper internal part of the bottles a crystallization in different ramifications, similar to those seen on the windows of apartments in very cold weather.

Oil of bergamot exhibited a number of small elliptical laminæ in its substance; that of lemons had deposited small crystals; oil of orange-flowers exhibited a diminished fluidity; that of cinnamon was partly congealed on the augmentation of the cold, when the thermometer in my laboratory stood at 15° below freezing (—1° Fahrenheit). I took advantage of this time to expose the same oils to the action of an artificial cold, which I procured by a mixture of ammoniacal muriate and ice, which caused the thermometer to sink to 22 degrees (—17° Fahrenheit). I kept up the same degree of cold for two hours, by alternately adding the ammoniacal muriate and ice. During the action of this cold upon the oils, the stoppers were raised by the expansion and disengagement of a gaseous substance, which perfumed the laboratory and its environs more strongly than when these oils were distilled in summer. The upper part of the bottles was lined with needles, of which the ramifications formed dendrites. When the surrounding ice began to change its temperature, I proceeded to examine the oils as speedily as possible, and remarked:

1. The interior surface of the bottle which contained the oil of peppermint was covered with small needles which formed a capillary vegetation. They were white, and liquefied on the fingers. When applied to the tongue, they afforded the fresh and pungent smell of the oil.

oil. Their solution in alcohol became white by the addition of water. The oil possessed an imperfect fluidity; its smell was less intense, and its colour deeper. It was soluble in alcohol, and had lost by congelation one * part of its weight †.

2. The bottle containing volatile oil of orange flowers exhibited different ramifications in its superior internal cavity. Upon drawing the stopper a disengagement of air took place, together with the other phenomena observed in the oil of peppermint, with this difference, that the mass of the oil was more coloured; that it had lost its fluidity, so as to adhere to the bottle like turpentine; and when poured into water some particles separated, which remained constantly at the bottom of the fluid.

3. The artificial cold produced no other effect on the oil of bergamot than the appearance of some crystalline laminæ of an elliptical form. This oil resumed its fluidity at four degrees below 0 (or 23 Fahrenheit). Its properties were not perceptibly changed.

4. The volatile oil of lemon-peel, when taken out of the bath of ice and salt, appeared to have lost its fluidity. At the expiration of some days, I perceived that an amber-coloured liquor had separated from this oil, with several small crystals. I decanted these several products from each other, and obtained the following results:

A. The oil from which the amber-coloured liquor had been separated possessed the colour and transparency of the common oil, but its smell and taste were less pungent. Its solution in alcohol was similar to that which had not been subjected to cold.

B. The amber-coloured liquor afforded by the volatile oil of lemon-peel by the action of cold, possessed an empyreumatic smell. Its taste was bitter and slightly acid. It mixed with water, reddened the tincture of tournsole, did not precipitate lime-water, and caused an effervescence with the carbonate of pot-ash. This liquor was afforded from the oil of lemons in the proportion of one-tenth part of the whole.

C. The concretions or small crystals obtained from the volatile oil of lemons did not possess a well determined form. They were white, scarcely consistent, and became opaque and friable by the contact of the atmospheric air. They emitted an odour of oil of lemons, were insoluble in cold water, liquefied in boiling water, and formed a pellicle at its surface while cooling. When heated in a capsule, they melted, and crystallized in needles by cooling. They did not take fire by the flame of a candle. Their solution in alcohol reddened the tincture of tournsole.

5. The bottle containing the volatile oil of turpentine had its superior internal surface covered with a slight efflorescence, produced by a portion of the oil which had risen by evaporation. Concretions were formed in the mass of the oil adherent to the sides of the bottle in the form of flattened tears. These were white, opaque, and more firm than turpen-

* This fraction in the original is rendered imperfect by the type for denominator having fallen out. N.

† Citizen Pelletier has some oil of peppermint which crystallizes in long needles at the temperature of six degrees below congelation (or 18 Fahrenheit), and almost totally at a few degrees lower.

In a package of the volatile oil of peppermint, which he received from London, he found a pint bottle broken, and the oil spilled among the hay made use of for the package. He reserved this hay for distillation, in order to recover the oil. At the end of eight days, perceiving that the hay began to ferment, he immediately subjected it to distillation, and obtained a very fine oil possessing the property of crystallization. Citizen Pelletier is of opinion that it acquired this property by combining with the elastic fluid produced in the incipient fermentation of the hay. M.

tine. They resumed the fluid state at seven degrees below congelation (or 16° Fahrenheit). This fluid, being poured into water, sunk to the bottom.

The oil from which the concretions were obtained did not appear to be changed.

6. The volatile oil of lavender, newly distilled, rose slightly in vapour, and formed a congelation in the upper part of the bottle. The portion of oil that remained had lost its fluidity, and acquired a deeper colour.

7. The oil of thyme, recently obtained, afforded the same results.

8. Volatile oil of cinnamon, subjected to the action of artificial cold, became very thick, and exhibited an irregular crystallization; and, as Citizen Baumé has observed, this oil resumed its fluid state at four degrees below 0 (or 23° Fahrenheit) without alteration.

Concerning the Action of Cold and of Water, during its Transition to the Glacial State, upon the Volatile Oils.

WATER, in its passage to the state of ice, loses part of its caloric, increases its volume, and exerts a pressure on surrounding bodies.

Being desirous of knowing what would be the result of these phenomena on the volatile oils, I made the following experiment:

I poured distilled water into several bottles, and added volatile oils which had not been subjected to any experiment. I placed these bottles in capsules of glass, and exposed them at a window where the thermometer marked 11 degrees below freezing (or 7° of Fahrenheit). At the moment when the water assumed the state of ice, several of the stoppers were forced up by the disengagement of an aromatic principle. The frozen water speedily afterwards increasing its volume, caused several of the oils to flow over the external surface of the bottles.

The bottle containing volatile oil of peppermint was covered with a capillary vegetation, as well as the capsule. The other oils exhibited nothing particular.

Proust had occasion to observe a similar phenomenon on pouring out the oil of lavender which had been evaporated by cold: part of the volatile oil which had been thus exposed to cold having escaped out of the bottle, he saw a few instants afterwards a kind of snow on the whole surface of the bottle which had been covered with the oil.

The action of cold and of water on the oil of peppermint had augmented the intensity of its colour, and weakened its smell. This oil had preserved its solubility in alcohol. The needles which had separated from the volatile oil of peppermint during its escape, were white, silky, and brittle; they had the smell of mint. When applied to the tongue, they left the cool and pungent taste of the peppermint drops or pafils. They suffered no alteration by the contact of the air; were fused without inflammation by the flame of a candle, and became consistent and transparent by cooling. When these needles were triturated in distilled water, they communicated to the fluid the property of reddening the tincture of tournsole; their solution in alcohol was attended with a slight noise and agitation (fremissement), and did not become white by the addition of water, as usually happens with such solutions. From these various experiments it follows, that a temperature of 11 degrees below 0 of the thermometer of Reaumur (or 7° of Fahrenheit) produces no change in several of the volatile oils, but thickens those of cinnamon and bergamot, and determines a crystallization more or less regular; that the intense cold obtained by a mixture of muriate

of ammoniac and ice disengages from the volatile oils part of their aroma; renders their colour more intense, thickens them, produces concretions in the oils of turpentine and lemon, and separates from the latter an amber-coloured fluid, which appears to possess saline properties; and lastly, that the action of the cold from water, with the concurrence of the air on the volatile oil of peppermint, afforded a crystalline matter disposed in silky needles, which various experiments prove to be a peculiar salt; whence it may be concluded, that the needles of camphor, which are said to have been found in the distilled water of peppermint, are of the nature of this salt.

[To be concluded in the next Number.]

MATHEMATICAL CORRESPONDENCE.

QUESTION III. Answered by J. F. R.

LET A, B, C and D, be the players in the order of seniority. Then, the last card being a trump, there will, when D begins to deal, be 12 trumps out of 51 cards, to be distributed among them. The chance that A's first card is not a trump, will, therefore, be $\frac{39}{51}$. This having happened, the chance that the next card will not be a trump, will become $\frac{38}{50}$, and the next $\frac{37}{49}$. Then A's chance that his first card will be one, will, of course, be $\frac{12}{48}$ or $\frac{1}{4}$; which having happened, A's chance of failure becomes $\frac{36}{47}$, and so on. By proceeding in this way, we find the whole value of D's chance for all the trumps = $\frac{39 \cdot 38 \cdot 37 \cdot 36 \cdot 35 \cdot 34 \cdot 33 \cdot \&c. \cdot \cdot \cdot 4}{51 \cdot 50 \cdot 49 \cdot 47 \cdot 46 \cdot 45 \cdot 43 \cdot \&c. \cdot \cdot \cdot 5} \times \frac{1}{4}$, which, by striking out common multipliers, becomes $\frac{1}{17 \cdot 10 \cdot 7 \cdot 47 \cdot 46 \cdot 5 \cdot 43 \cdot 7 \cdot 41} = \frac{1}{158753389900}$: so that the odds against it are 158753389899 to 1.

The same. Answered by W. T. of Bath.

SUPPOSING a pack of cards to be properly shuffled, and the bottom card to be afterwards turned up, and consequently known; it is required, in the strict sense of the question, to find the probability that every fourth card, dealt off from the top, shall be of the same suit with the trump card which is exposed. But as the chance for some one of the trumps lying in the 4th, 8th, 12th, &c. place, is evidently the same as for its lying in any other place; it amounts to the same thing as to determine the probability of a person's drawing the 12 remaining trumps, one by one, at random, in 12 trials. To effect this, it is necessary to observe, that as there are now 51 cards in all, and only 12 in his favour, the probability of his drawing one of the trumps, the first trial, is $\frac{12}{51}$; and if this be supposed to be done, and the card taken away, there will now remain 50 cards in the pack, and only 11 in his favour; whence the probability of his succeeding the next trial is $\frac{11}{50}$. In like manner,

manner, the probability of his succeeding the third time is $\frac{10}{49}$, the fourth time $\frac{9}{48}$, the fifth $\frac{8}{47}$, the sixth $\frac{7}{46}$, the seventh $\frac{6}{45}$, the eighth $\frac{5}{44}$, the ninth $\frac{4}{43}$, the tenth $\frac{3}{42}$, the eleventh $\frac{2}{41}$, and the twelfth $\frac{1}{40}$. And since the probability of the happening of any number of events, is equal to the product of the several probabilities of each of them happening, when considered separately, we shall have $\frac{12}{51} \times \frac{11}{50} \times \frac{10}{49} \times \frac{9}{48} \times \frac{8}{47} \times \frac{7}{46} \times \frac{6}{45} \times \frac{5}{44} \times \frac{4}{43} \times \frac{3}{42} \times \frac{2}{41} \times \frac{1}{40} = \frac{1}{158753389900}$ for the probability required. So that the chances against the dealer, at the game of whist, having all the 13 trumps in his own hand, are above a hundred thousand million to one. And if the chances thus determined be substituted in the analytical formula, given by Demoivre, in his Doctrine of Chances, 3d edit. Prob. III, it will be easy to shew that the number of trials in which this event may probably take place is 11112737229; whence, reckoning every game, one with another, to take 10 minutes, it would be necessary, in order to make it an equal chance for the event happening or failing, that the gamesters should play on, uninterruptedly, for above two hundred thousand years.

QUESTION IV. Answered by ANALYTICUS.

IT is laid down, by Newton, as a law of refrigeration, that the quantities of heat lost, in given small spaces of time, are always proportional to the heat remaining in the body; or that, if the times be taken in arithmetical progression, the remaining heats will be in geometrical progression; reckoning these heats to be the excess by which the body is warmer than the surrounding atmosphere.

Suppose, therefore, that b is the temperature of any body above that of the atmosphere, and that it cools d degrees in one minute. Then, by the above law, its temperature at the end of one, two, three, &c. minutes, will be $b - d$, $b - d$ ($\frac{b-d}{b}$), $b - d$ ($\frac{b-d}{b}$)² &c. or $b - d$, b ($\frac{b-d}{b}$)², b ($\frac{b-d}{b}$)³ &c. Whence, universally, its temperature at the end of m minutes will be b ($\frac{b-d}{b}$) ^{m} , and at the end of n minutes b ($\frac{b-d}{b}$) ^{n} .

And if these temperatures be determined by experiment, and denoted by a and b respectively, we shall have b ($\frac{b-d}{b}$) ^{m} = a , and b ($\frac{b-d}{b}$) ^{n} = b ; from which equations b is

$$\text{found} = \frac{a^{\frac{n}{n-m}}}{b^{\frac{n}{n-m}}}, \text{ or } \log. b = \frac{1}{n-m} (n \log. a - m \log. b), \text{ and } d = b \times \left(1 - \frac{1}{a}\right)^{\frac{1}{n-m}}$$

$$= \frac{a^{\frac{n}{n-m}}}{b^{\frac{n}{n-m}}} \times \frac{1}{a^{\frac{n}{n-m}} - b^{\frac{n}{n-m}}}. \text{ Also } \frac{b-d}{b} = \frac{1}{a^{\frac{1}{n-m}}} = r = \text{rate of cooling.}$$

Again, because $b \left(\frac{b-d}{b}\right)^m$ is the temperature of the body at the end of m minutes, and $b \left(\frac{b-d}{b}\right)^n$ its temperature at the end of n minutes, either of these forms will express the temperature of any body at the end of any given time, from its first beginning to cool; and their difference $b \left(\frac{b-d}{b}\right)^m - b \left(\frac{b-d}{b}\right)^n$ or $b \times \left(\frac{b-d}{b}\right)^n - \left(\frac{b-d}{b}\right)^n$ is the heat which the body would lose between the times m and n ; b and d being found as above.

In like manner, since $b \left(\frac{b-d}{b}\right)^m = n$, or $\left(\frac{b-d}{b}\right)^m = \frac{a}{b}$, we shall have $m \log. \frac{b-d}{b} = \log. \frac{a}{b}$, or $m (\log. b - d - \log. b) = \log. a - \log. b$; and consequently $m = \frac{\log. a - \log. b}{\log. b - d - \log. b}$ = time which a body takes in cooling, till it arrives at the observed temperature a .

And if it be required, as in the question, to find the degree of heat to which any body has been exposed, we shall have, in this case, br^m = number of degrees between that temperature and the heat to which the body has been raised in the time m . Hence $b - br^m$ = number of degrees the body has been heated, which call t ; then $b - br^m = t$; from which equation b is found = $\frac{t}{1-r^m}$, where r , m and t are known terms.

These formulæ will be found of considerable use in several branches of chemistry, and are capable of a number of practical applications, which are left, for the present, to the consideration of the reader:

NEW MATHEMATICAL QUESTIONS.

QUESTION VII. By W. T. of Bath.

AS a proper sequel to Question III, it is proposed to determine the odds against the dealer and his partner, at the game of whist, having between them all the thirteen trumps.

QUESTION VIII. By S. S. of Reading.

Given $a^x + b^x = c$, to determine the value of x , either by logarithms or a converging series.

SCIENTIFIC NEWS.

Letter from Sir BENJAMIN THOMPSON, Kt. Count of Rumford, F. R. S. to the Right Hon. Sir JOSEPH BANKS, Bart. K. B. P. R. S. announcing a Donation to the Royal Society for the Purpose of instituting a Prize Medal.*

AT the anniversary of the Royal Society, held the 30th of November 1796, the President acquainted the Society that Count Rumford had transferred one thousand pounds

* From the Transactions for 1797.

three per cent. consolidated Bank Annuities to the use of the Society, on certain conditions stated in a letter to the President; which was read as follows:

“SIR,

“DESIROUS of contributing efficaciously to the advancement of a branch of science which has long employed my attention, and which appears to me to be of the highest importance to mankind; and wishing at the same time to leave a lasting testimony of my respect for the Royal Society of London, I take the liberty to request that the Royal Society would do me the honour to accept of one thousand pounds stock in the three per cent. consolidated public funds of this country; which stock I have actually purchased, and which I beg leave to transfer to the President, Council, and Fellows of the Royal Society; to the end that the interest of the same may be by them and their successors received from time to time for ever, and the amount of the same applied and given, once every second year, as a premium to the author of the most important discovery or useful improvement which shall be made and published by printing, or in any way made known to the public, in any part of Europe during the preceding two years, on Heat or on Light; the preference always being given to such discoveries as shall, in the opinion of the President and Council of the Royal Society, tend most to promote the good of mankind.

“With regard to the formalities to be observed by the President and Council of the Royal Society, in their decisions upon the comparative merits of those discoveries, which, in the opinion of the President and Council, may entitle their authors to be considered as competitors for this biennial premium, the President and Council of the Royal Society will be pleased to adopt such regulations as they, in their wisdom, may judge to be proper and necessary. But in regard to the form in which this premium is conferred, I take the liberty to request, that it may always be given in two medals, struck in the same die, the one of gold and the other of silver, and of such dimensions that both of them together may be just equal in intrinsic value to the amount of the interest of the aforesaid one thousand pounds stock, during two years; that is to say, that they may together be of the value of sixty pounds sterling.

“The President and Council of the Royal Society will be pleased to order such device or inscription to be engraved on the die they shall cause to be prepared for striking these medals, as they may judge proper.

“If, during any term of years, reckoning from the last adjudication, or from the last period for the adjudication of this premium, by the President and Council of the Royal Society, no new discovery or improvement should be made in any part of Europe, relative to either of the subjects in question (Heat or Light), which in the opinion of the President and Council of the Royal Society shall be of sufficient importance to deserve this premium, in that case it is my desire that the premium may not be given; but that the value of it may be reserved, and, being laid out in the purchase of additional stock in the English funds, may be employed to augment the capital of this premium; and that the interest of the same, by which the capital may from time to time be so augmented, may regularly be given in money with the two medals, and as an addition to the original premium, at each succeeding adjudication of it. And it is further my particular request, that those additions to the value of the premium, arising from its occasional non-adjudications, may be suffered to increase without limitation.

"With the highest respect for the Royal Society of London, and the most earnest wishes for their success in their labours for the good of mankind, I have the honour to be, &c.

"London, July 12, 1796.

(Signed) "RUMFORD.

"To Sir JOSEPH BANKS, Bart. K. B. President
of the Royal Society of London."

The Society hereupon resolved, that they accept of the donation, and accede to the conditions annexed to it by the Count; and also directed, that a letter be written to the Count, acquainting him of this acceptance; returning him thanks for the liberal donation, and assuring him that the conditions annexed to it will be strictly adhered to.

NEW PUBLICATIONS.

VOYAGE dans les Alpes, &c. Tom. V. VI. VII. & VIII.; or Travels in the Alps, with a Preliminary Essay on the Natural History of the Environs of Geneva. By Horace Bénédict de Saussure, Emeritus Professor of Philosophy in the Academy of Geneva, and Member of various other Academies; two volumes in quarto, or four in octavo. Printed at Neuchâtel, and imported by De Boffe, Gerrard-street. Price of the former , and of the latter 11. 1s.

The former volumes of the Travels of this celebrated author, so rich in geological and philosophical facts and observations, are too well known and esteemed to require any general character in this place. These volumes are equally interesting, and contain a treasure of information, which, nevertheless, from its peculiar nature and locality in many respects can admit of no abridgement.

The First Report of the Society for bettering the Condition and increasing the Comforts of the Poor. Octavo, 68 pages. Becket, London, 1797. Price one shilling.

The contents of this valuable pamphlet are enumerated with so much perspicuity and effect by Thomas Barnard, Esq. in the Preliminary Address, that I shall have the pleasure of giving them in his own words:

"Friendly societies are the objects of the first paper, which presents an interesting detail respecting one at Castle Eden, upon a scale capable of general adoption: it contains an important illustration of the true principle of action with regard to the poor, and proves how much they may in a short time learn to do for themselves, and to what a degree of kindness and affection they may be habituated to extend their interest in the welfare of each other. The manner in which the poor and industrious member of that society has been assisted in the purchase of his cow, and its beneficial consequences both to the individual and to the property with which he is connected, by increasing and improving the stock upon it, is deserving of attention and imitation.

"The second is an account of a village shop:—a subject, the importance of which will be felt by all who interest themselves in the domestic concerns of the poor, when it is known that a saving of 20 per cent. may be thereby made to the labourer in the purchase of the necessaries of life; that it is the most effectual means to prevent his running in debt; that the expence and trouble to the charitable founder of the shop are inconsiderable; and that it is liable to no objection but what may be easily obviated.

"The next communication is upon workhouses of united hundreds, an enquiry of no small importance at the present moment. The mode of their management *, and the ob-

* See Sir William Young's Observations, published in 1780, and his Considerations on the Subject of Poor-houses and Work-houses 1796.

jections and inconveniences that attend them under the best regulations and management, are stated with clearness and perspicuity. The rules of a spinning-school, established with success at Oakham, upon the principles of Count Rumford, is the next in order;—a school where the poor attend with pleasure and regularity, and thankfully receive the benefit of a cheaper and more nourishing diet supplied to them at a very small price; and for these reasons simply:—because they are allowed to continue free agents, and to retain an option on the subject; and because they have the whole of their earnings inviolably at their own disposal. May the example be speedily followed in other parts of England!

“The fifth is an account of the jail and house of correction at Dorchester. When we consider the important consequence of what has been effected there, in annually saving, to the public and to themselves, many persons otherwise abandoned to destruction, we cannot help lamenting that so very few similar instances are to be found in the whole kingdom. The principle of this reform will apply, with still greater force, to every measure that regards the local and domestic concerns of mankind; in all of which it will invariably be found, that in proportion as coercion is given up, and the interest of the party is made the spring of action, temptations to vice will be excluded, and habits of labour and honesty will be gradually acquired.

“In the next paper, upon fuel, the reader will find a very gratifying proof that the poor may be easily reconciled to inclosure, or to any other measure of public benefit, where their own feelings and interests are only properly consulted.

“The last communication is on parochial relief, and the mode and principle on which it has been administered by the magistrates of the hundred of Stoke.”

Philosophical Transactions of the Royal Society of London for the Year 1797, Part I. quarto, 218 pages, with 26 pages of Mineralogical Journal, and four plates. Sold by Elmsly, London, price 8s. 6d.

This Part contains: 1. The Croonian Lecture, in which some Morbid Affections of the strait Muscles and Cornea of the Eye are explained, and their Treatment considered. By Everard Home, Esq. F. R. S. 2. Observations on Horizontal Refractions, &c. By Joseph Huddart, Esq. F. R. S. (See p. 145 of this work). 3. Recherches sur les principaux Problemes de l'Astronomie. Par Don Josef de Mendoza y Rios, F. R. S. 4. On the Nature of the Diamond. By Smithson Tennant, Esq. (see p. 177). 5. A Supplement to the Measures of Trees printed in the Philosophical Transactions for 1759. By Robert Marsham, Esq. F. R. S. 6. On the Periodical Changes of Brightness of two Fixed Stars. By Edward Pigott, Esq. 7. Experiments and Observations made with the View of ascertaining the Nature of the Gas produced by passing Electric Discharges through Water. By George Pearson, M. D. F. R. S. 8. An Experimental Enquiry concerning Animal Impregnation. By John Haighton, M. D. 9. Experiments in which, on the third Day after Impregnation, the Ova of Rabbits were found in the Fallopian Tubes; and on the fourth Day after Impregnation in the Uterus itself; with the first Appearance of the Fœtus. By William Cruikshank, Esq. 10. Letter from Sir Benjamin Thompson, Knt. Count of Rumford, F. R. S. &c. (for which see p. 189.). Appendix, containing the Meteorological Journal kept at the Apartments of the Royal Society.

Geometrical and Graphical Essays; containing a general Description of the Mathematical Instruments used in Geometry, Civil and Military Surveying, Levelling, and Perspective, with

with many new practical Problems; illustrated by thirty-four Copper-Plates. By the late George Adams, Mathematical Instrument Maker to the King, &c. Second edition, corrected and enlarged by William Jones, Mathematical Instrument Maker. Octavo, 515 pages. Sold by Jones in Holborn, price 14s. boards.

The character of this work is already well known to the public. It contains a large quantity of useful matter, treated with much perspicuity, and illustrated by a number of accurate and beautiful engravings. Mr. Jones has considerably augmented and improved the present edition.

Count Rumford's Experimental Essays, Political, Economical, and Philosophical. Essay VI. of the Management of Fire, and the Economy of Fuel. Octavo, 196 pages, with six plates. Price 3s. 6d. in boards. Cadell and Davies, 1797.

The reputation of this author, as a philosopher and earnest promoter of the immediate interests of humanity, is fully established. The present essay is distinguished by the same clear and perspicuous detail of interesting facts and inferences as the former Essays which appeared before the commencement of this Journal. It is divided into six chapters: 1. General View and Importance of the Subject. 2. Of the Generation of Heat in the Combustion of Fuel. 3. Of the Means of confining Heat and directing its Operations. 4. Of the Manner in which Heat is communicated by Flame to other Bodies. 5. Experiments with Boilers and Fire-Places of different Constructions. Relative Quantities of Heat from various Fuels. Estimates of the Total Quantities and Loss under various Circumstances. 6. An Account of a Number of Kitchens, Public and Private, and Fire-Places for various Uses, which have been constructed under the Direction of the Author, in different Places.

* * J. F. R is respectfully desired to reconsider his solution of the fifth question (p. 138)—more especially because he has inadvertently supposed the second table on p. 39 to be capable of affording the specific gravities of mixtures of common salt and water. For the reasons there mentioned, instead of attempting to give the specific gravities answering to degrees from a repetition of Baumé's experiment, I took the extreme number from the instrument, as found by De Morveau, and calculated the intermediate numbers, as they correspond with equal differences of immersion, of a cylindrical stem; which was the case above the first fifteen degrees, and probably also in those very nearly. Various authors have given the specific gravities of the solutions of common salt. The following table was formed by experiment with very fine dry crystals of marine salt, by Dr. Watson, now Bishop of Landaff. I have extracted it from a paper inserted in the supplement to the thirteenth volume of the Journal de Physique, page 76, for the year 1778. I suppose it to be taken from the Philosophical Transactions, but have not at this time the opportunity of consulting the whole set.

A TABLE of the Specific Gravity of various Solutions of common Salt, at the Temperature of between 46° and 55° of Fahrenheit. Quantity of Salt by Weight = 1.

Water.	Sp. Gr.	Water.	Sp. Gr.	Water.	Sp. Gr.	Water.	Sp. Gr.	Water.	Sp. Gr.	Water.	Sp. Gr.
2	1.206	11	1.059	26	1.027	47	1.014	143	1.004	847	1.0008
3	1.160	13	1.050	27	1.025	53	1.013	161	1.003	1023	1.0006
4	1.121	14	1.048	29	1.024	55	1.012	191	1.0029		
5	1.107	15	1.045	31	1.023	71	1.009	255	1.0023		
6	1.096	17	1.040	35	1.020	83	1.007	319	1.0018		
7	1.087	20	1.032	38	1.019	107	1.006	447	1.0017		
8	1.074	23	1.029	41	1.015	125	1.005	511	1.0014		



Fig. 3.

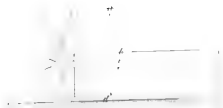


Fig. 4.



Fig. 5.



Abbey Head

Fig. 6.



Fig. 7.



Fig. 8.

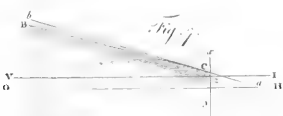
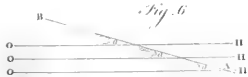


Fig. 9.



Stock for tapping screws. Fig. 3.

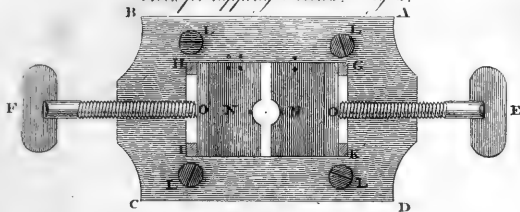
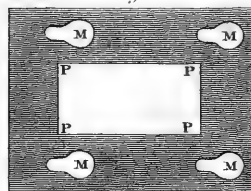


Fig. 4.



Tap. Fig. 1.



Screw Tool. Fig. 2.

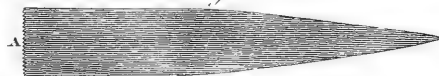


Fig. 5.



Compound Stock for four pair of Dies. Fig. 7.

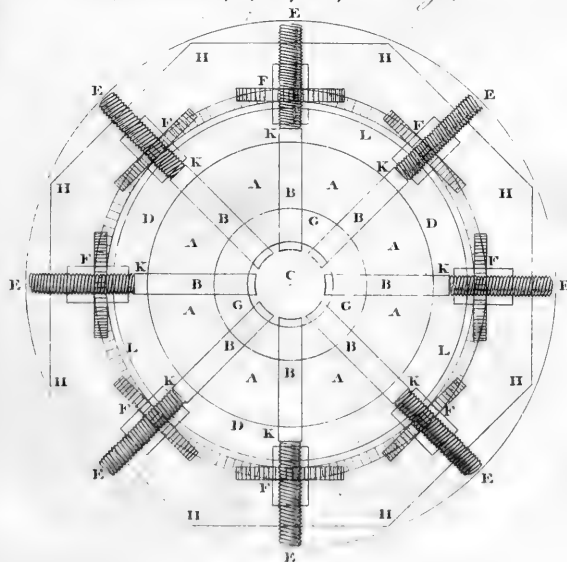


Fig. 6.





A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

AUGUST 1797.

ARTICLE I.

An Account of the New System of Measures established in France. By CH. COQUEBERT.*

FOR several centuries, reason, good faith, and all the principles of social order have exclaimed against the diversity of measures in France. Nothing was wanting for the destruction of this abuse, already condemned by the public opinion, but a favourable concurrence of circumstances. The members of the Constituent Assembly, charged by their electors to establish at last that uniformity which had constantly been demanded by the States General, and no less frequently promised by Ministers, gave charge to the Academy of Sciences to direct their researches to this great change. There was no doubt of finding in the learned a sufficient degree of zeal for a work that must facilitate the communication of intelligence, extend the dominion of reason, and economise that time of which men the most instructed are ever the most sparing. From this rare and valuable concurrence between the sciences and the legislation has resulted the system we are about to explain to our readers. In this system they will discern the character of a work independent of peculiar times or individual locality. Its authors have been aware that they could not worthily serve the French nation in this respect, without embracing in their plan the interests of the whole human race. They were desirous that their labours should be useful to all nations; and that the commerce which serves to unite them might everywhere speak the same language. It is more particularly on this account, that instead of confining themselves to render the measures of Paris of general use throughout France, they have sought in the nature of things for the invariable basis of a complete system connected in all its parts.

The pendulum was proposed in the last century to serve as the original of all measures;

* Journal des Mines, publié par le Conseil des Mines de la République, N^o. XIV. 73.

but it presented two inconveniences: the first, that the division of time is in itself arbitrary*; the second, that the measures thence resulting, and geographical measures depending on the magnitude of the earth, would have no mutual agreement†.

The preference was given to refer all measures large and small to the terrestrial globe. Among the great circles which measure its circumference, the preference was not given to the equator. The nature of the countries over which it passes, renders the observations too difficult. The determination was made in favour of meridians‡.

The whole meridian was not taken as the unity for subdivision, for two reasons: the first, because the quadrant is a true unity for mathematicians and astronomers, as is well known to all that cultivate the sciences; second, because the magnitude of the circumference of the earth is known to us only by conjecture. All the great astronomical and geodesical undertakings have been performed in the northern hemisphere. We have yet no correct notion of the figure of the southern hemisphere. It would even result from the observations of Lacaille, at the Cape of Good Hope, that the terrestrial spheroid is more flattened on this side. No hypothesis can be admitted in a subject of this nature. The learned could therefore propose as a fundamental unity that only which was well known to them, namely, the distance from the north pole to the equator.

This distance is obtained with certainty from the measure of an arc of the meridian intersected by the forty-fifth parallel. France alone offers in a civilised country an arc of the requisite magnitude, which, if we comprehend a small part of the Spanish territory, terminates in the sea at both extremities. This arc has besides the advantage to have been repeatedly measured with the utmost care. Two celebrated astronomers are at this moment employed on this operation; not that it is presumed that the result of their labour can have

* We reckon 86400 seconds in a day; because the day is divided into 24 hours; and the hour into 60 minutes. Every other division would render the element of time longer or shorter, and accordingly influence the length of the pendulum. The pendulum which should make one hundred thousand oscillations in a day, would be only 2.283 feet in length, whereas the pendulum for seconds is three feet 7.21 lines under the equator. C.

All the measures in this paper must be understood to be French, as I have thought it unnecessary to make any other reductions but those in the table p. 199. N.

† Under the name of geographical measures we denote in this place the degrees of latitude and longitude. It is commonly supposed that a degree of the meridian is twenty-five common leagues; but in order to afford this accurate quotient in round numbers, it is necessary to reckon the league at from 2281 to 2283 toises. The league cannot be at the same time a multiple of the pendulum in round numbers, and an aliquot part of a degree. C.

‡ This notion seems to have presented itself to the ancients. There are satisfactory proofs that the cubit of the Nilometer in Egypt, the length of which is 1.712 foot, was contained four hundred times in a stadium, two hundred thousand times in a degree, and consequently seventy-two millions of times in the circumference of the earth. It is thought that the great Pyramid is a monument of this system, and that its base is exactly the length of the ancient Egyptian stadium of five hundred to the degree. The ancients had nevertheless neglected to adopt an uniform method of division, and in this their notion required improvement. The same observation may be made on the system of measures likewise proposed to be deduced from the magnitude of the earth, in the year 1670, by Mouton, an astronomer of Lyons. He thought it necessary to preserve the division of a circle into three hundred and sixty degrees, and the degree into sixty minutes; so that he does not propose the decimal division but from the minute of the meridian. The thousandth part of the minute of the meridian affords a measure of five feet eight inches and a half, which he proposes for the usual unity. C.

any sensible influence on the usual instruments of measure, but to present to Europe, in all the parts of this great operation, a degree of precision honourable to the Sciences and to the Republic.

It was not thought necessary to wait the termination of their work previous to ascertaining the distance from the pole to the equator in the ancient measures. It is sufficiently known, by the observations of learned men, that the length of a degree of the meridian, taken at an equal distance from the pole and the equator, is 57027 toises.

It is likewise known, that from this mean degree the degrees become longer each by about twelve toises in approaching the pole, and shorter in the same proportion by advancing towards the equator, on account of the oblate figure of the terrestrial globe towards the poles. This degree, intersected by the forty-fifth parallel, is therefore the mean term of an arithmetical progression, which multiplied by 90 will give the distance from the pole to the equator.

57027 multiplied by 90 produces 5132430 toises, or 30794580 feet.

This is the fundamental unity expressed in ancient measures from the iron toise of the Academy, supposed to be at the temperature of 13 degrees of Reaumur's thermometer.

If the new observation should give this distance a minute quantity larger or smaller, it will not be necessary on that account to change the number of toises which represent it. It will be sufficient to vary in the proper direction the assumed temperature of the iron toise *.

The unity being thus determined, the second step was to divide and subdivide it in such a manner as to obtain linear measures appropriated to the different uses. An essential condition was to choose a proper number to be employed, and to employ it constantly and exclusively, in order to reduce the arbitrary elements of the system as much as possible. Some learned men were desirous that the number 12 might be adopted, because divisible immediately into the half, the quarter, and the sixth; others, on the contrary, being impressed with the inconvenience of these fractions of variable denominators, would have preferred some prime number, such as 7 or 11, which would have rendered the use of all such fractions impracticable. But very effectual considerations proved decisive in favour of the number 10. For, 1. This number is in some respect pointed out by nature, since we find the decimal numeration among nations which do not appear to have had any communication.

2 Nothing can be better adapted to facilitate calculations, than to subject the division of measures to the ordinary laws of arithmetic; whence the conversion of one measure into another of a superior or inferior order is effected by a single stroke of the pen, and fractions may be worked with the same ease as whole numbers. This last advantage could not be united to those which were supposed to exist in the numbers 7, 11 and 12, without entirely changing the system of oral and written numeration; an enterprise which perhaps is out of the power of any government to accomplish.

The distance from the pole to the equator being taken as the fundamental unity, and the number 10 for the only divisor, constitute the simple and productive principles on

* Men of science agree that the measure of the arc of the meridian may be depended on to the five thousandth part (or barely to four places of figures. N.). If therefore it should happen that the error amounts to the utmost possible quantity, it would be entirely remedied by supposing the toise to be lengthened or shortened one five-thousandth part by the effect of temperature. Every degree of variation, according to the scale of Reaumur, produces on the length of an iron bar a variation of one-75000th part. C.

which this system is established. They are so little arbitrary, that posterity, even if our books and monuments were lost, might easily recover them; whereas, for want of a clue of this nature, the learned are not yet agreed concerning the true dimensions of most of the Greek and Roman measures. Magnitudes arbitrarily taken cannot be preserved but by means of metallic standards, which are altered by time or negligence, and in the revolution of empires are totally lost. The French nation will have no variation to fear in its measures, because it has referred them not to the perishable works of man, but to every thing which is most durable in nature.

Names were to be affixed to each of the measures resulting from the system we have exposed. It was not enough to denote them by their relation to the fundamental unity, and to say, for example, the hundred thousandth, the millionth, or the ten millionth part of the quarter of the meridian. These are definitions; but the mind has need of particular signs, to give, as it were, a body to each idea.

The words primes, seconds, thirds, and the like, might have been used to denote each degree of the decimal division. And these words would have possessed the advantage of always denoting the proportion of each of these inferior unities to the prototype which is the basis of the system.

It was thought proper to follow another method, perhaps less exact, but more conformable to the ordinary practice. Having arrived, after seven successive divisions, to a measure of three feet eleven lines and forty-four hundredth parts, which is the ten millionth part of the fundamental unity, a name was given to this usual and portable measure, adapted to take place of the foot, the toise, and the ell. It was called metre. Proceeding afterwards from this measure as the common unity, its multiples have been named by prefixing to the word metre, one of the Greek words deca, hecaton or hecto, chilio or kilo, and myria, which signify ten, a hundred, a thousand, and ten thousand; and its sub-multiples by means of the Latin prefixes deci, centi, milli.

Consequently a decametre is a measure consisting of ten metres (30 feet nine inches), proper to be substituted in place of the measuring chain for land.

The kilometre and the myriametre are itinerary measures; the one of 1000, and the other of 10,000 metres (513 and 5132 toises), the first being equal to a small quarter of a league, and the second two medium leagues, or one post.

Ten myriametres make the 100th part of the quarter of the meridian, which may, if thought fit, be called the decimal degree.

The decimetre, or 10th part of a metre, answers to three inches eight lines and four points.

The centimetre, or 100th part of a metre, is four lines and five points.

And lastly, the millimetre is five points and a third.

Every other kind of measure is derived from the metre and its parts; such as the measures of land, of solids, of liquids, of grain, and even of weights and coin. The whole is therefore derived from the same origin, and the system exhibits a complete whole.

A portion of land of one square decametre, or one hundred metres square, has received the name of are.

A square hectometre, or ten thousand square metres, constitutes one hundred ares, or an hectare. These are the agrarian measures, substituted in the place of the perch and the acre (arpent).

The cubic metre receives the name of *stere* when employed to measure wood for fuel, in which it is substituted for the cord and other ancient measures. The tenth part of the *stere*, or *decistere*, corresponds very well with the measure commonly called *solive* in the commerce of wood for carpentry.

A vessel of a cubical form, having for its side one decimetre, or a cylindrical vessel of the same solid contents, has received the name of *litre*. It contains about two pounds of water, or twenty-five ounces of wheat. It is assumed as the element for measures of capacity, and every other measure of this kind is considered as the decimal multiple or sub-multiple of this. So that we have the *decalitre* of ten litres, containing sixteen pounds of wheat, or four-fifths of the bushel of Paris; the *hectolitre*, which contains near 160, or two-thirds of the *setier*. One thousand litres are equal in capacity to the cubical metre.

With regard to weights, the original weight is taken to be equal to the quantity of distilled water contained in a cubical vessel, the side of which is one-hundredth part of the metre. This water weighed in *vacuo*, and at the temperature of melting ice, is equal to eighteen grains and eight hundred and forty-one thousandth parts. The denomination *gramme* is given to this weight, and from the *gramme* are deduced by multiplication or division all the weights superior and inferior.

So that in ascending we have the *decagramme* equal to $2\frac{2}{3}$ gros.

The *hectogramme* equal to $3\frac{1}{4}$ ounces.

The *kilogramme* equal to 2 pounds 5 gros 49 grains.

The *myriagramme* equal to about $20\frac{1}{2}$ pounds.

And by descending we have the *decigramme*, which is nearly 2 grains.

The *centigramme*, $\frac{1}{100}$ th of a grain nearly.

And the *milligramme*, which is nearly the $\frac{1}{500}$ th of a grain.

Lastly, the division may be carried to the ten-thousandth part of a *gramme*, which may be substituted for the *essayer's* weight of the $\frac{1}{512}$ th part of a grain.

We see therefore that the *gramme* is placed between two series of like number of terms; the one, which increases to the *myriagramme*, with which all heavy weights will be ascertained; and the other, which descends to the ten-thousandth part of the *gramme*.

It is this consideration which has caused the radical name to be given to a weight so small and so little adapted to the ordinary uses of life. It is not pretended, that on this account it should be considered as the sole or even the principal unity. Like the others, it is merely a term in the series of weights; any one of the unities of which may be taken according to convenience. The word radical could not be applied to a unity of a superior order, without sacrificing much of the regularity and method of this nomenclature.

The pieces of money being adjusted to a certain number of grammes, are connected with the general system of measures.

Pieces of one centime will be formed of copper, weighing one *gramme*; of five centimes, or a sou, weighing five grammes; of one decime, weighing ten grammes; and of two decimes, weighing twenty grammes.

In silver there will be pieces of one franc weighing five grammes, and of five francs weighing twenty-five grammes.

Lastly, in gold, there will be pieces of ten grammes.

This explanation is sufficient to exhibit the system in its true light, and to shew its beauty, simplicity, and numerous advantages. Those who are desirous of more extensive developments,

developments, will find them in the works published by the temporary Commission of Weights and Measures, and by the Agency instituted to conduct this great and useful operation to its end. We have thought it necessary to annex to this memoir, 1. A table of the new system. 2. Tables of the relation between the old (French) measures and the new; by which it is easy to reduce the one to the other. The object of these tables is to reduce all the calculations relative to this transformation to simple addition. They will likewise serve to determine the price of the new measures from that of the old. In order to render them less voluminous, the simple unities are given from one to nine only. From the value of the unities it is easy to infer the value of decimal multiples or fractions, by shifting the decimal point.

To these tables we have added the logarithms of the ratios between the old and the new measures.

By this means every one who is engaged either in the art of mining, or the sciences relating to that in the memoir, will find collected in one view all that is wanting to facilitate the study and the practice of the new system.

*• The table numbered 2, in the original, occupies considerable space, and, though of great utility to the inhabitant of France, is comparatively of small importance to any other nation. It is merely a table for facilitating reductions of the old and new measures of France into each other, and does not convey any additional information to that in the memoir. For these reasons I have not copied it.

In the way of cursory observation on the objects of this honourable and useful undertaking, it may be remarked, that arguments deduced from considerations of the immediate numerical application of an original measure, are of little consequence as to the choice of the means of obtaining it. The chief, and indeed the sole, motive of preference must consist in the greater degree of accuracy, or more perfect agreement of the results wrought out by different observers. In experiments to determine the length of a pendulum vibrating through any known portion of the time employed by the earth in its rotation, we have to enquire, 1. Whether that rotation be theoretically uniform? and if not, what are the quantities and periods of irregularity, perceptible in such an experiment? 2. What are the best methods and limits of error in measuring the length of a pendulum between its centres of oscillation and suspension? 3. Whether the errors from temperature can be rendered insensible in this measure of time and length; and if not, what are their limits? And above all, 4. What are the effects of the escapement part, or simplest maintaining power in a clock; and what are the best means of diminishing or removing them altogether.

In the method of deriving an original standard from the measurement of lines and angles upon the earth, the objects of enquiry are certainly not less numerous than in the pendulum; but the superiority of one method beyond the other would require a complete treatise for its discussion. Among the latest modern surveys, those of General Roy, in the Philosophical Transactions, made with instruments of wonderful accuracy, will afford the greatest instruction to such as may wish to enter this path of investigation. In the last paper of the continuation of this survey, by Lieutenant Colonel Williams, Captain Mudge, and Mr. Dalby, inserted in the Transactions for 1793, a fundamental base was measured on Hounslow Heath, equal to 27404.2 English feet, and connected, by observation of the angles of seventeen triangles, with a base of verification measured on Salisbury Plain. As the triangles presented several ranges of connection, these were computed from the fundamental base, and gave the numbers 36574.8 and 36573.8 for the extreme results, expressing the base of verification in feet. The mean of these, namely, 36574.3, proved to be about one inch short of the actual admeasurement of the base of verification. It seems probable that there may have been some fortunate compensation of errors in this wonderful co-incidence; but the result appears to prove, that the method of terrestrial admeasurement will give an original measure to be depended on for more than four places of figures, and less than five.

When surfaces and solids are to be derived from linear measures, if there be any error in the original line, the error in the surface or area will be twice as great, and that in the solid three times as great, as in the line very nearly, as may be easily deduced from the common process of involution. That is to say, in most cases it will affect the same figure of the result. N.

TABLE

TABLE OF THE NEW MEASURES OF THE (FRENCH) REPUBLIC,

Containing the *Metrical System of Nomenclature, and their Proportion to the Ancient Measures.*

New System of Weights and Measures of France.

199

Proportions of the Measures of each Species to the principal Measure, or Unit.			PRINCIPAL MEASURES OR UNITIES.					EXAMPLES Of the Names, composed, in order to express different Unities of Measures.							
First Part of the Name which indicates the Proportion to the principal Measure, or Unity.			Length.	Capacity.	Weight.	Agrarian.	For Fire-wood.								
Letters.			Metre	Litre.	Gramme	Are	Stere								
Figures.															
Ten thousand	10000	Myria													
Thousand	1000	Kilo													
Hundred	100	Hecto	Ten millionth part of the distance from the Pole to the Equator.	A pint and $\frac{1}{8}$ or one litron and a quarter nearly.	A decimetre cube.	100 square metres.	One cubic metre.								
Ten	10	Deca													
One	1	Deci													
Tenth	$\frac{1}{10}$	Centi													
Hundredth	$\frac{1}{100}$	Milli													
Thousandth	$\frac{1}{1000}$														
Proportion of the principal Measures between themselves and the Length of the Meridian															
Value of the principal Measures, and the ancient French Measures															
Value in English Measures															

Myriametre, length of ten millimetres, equal to about two leagues (or six English statute miles one furlong and 193 yards).
Kilogramme, weight of 100 grammes, equivalent to a little more than two pounds, *poids de marc* (or 22666 grains English, which is 3lb. 4oz. very nearly).
Hectare, Agrarian measure, equal to about two arpents des eaux et forêts, or three measures of Paris (or 1968 square yards English, which is rather less than 2½ acres).
Decalitre, Measure of capacity for corn, equal to about 4-fifths of the Paris bushel (or 61.083 cubic inches English, which is to the English peck of 537.6 inches, nearly as 8 to 7).
Decimetre, tenth part of a metre, equal to about three (English) inches and a half (or 3.937 inches English).
Centigramme, hundredth part of a gramme, equal to about 3-fifths of a French grain, or 0.2-9666 grains English).
Kilatre, and all those which are formed with the letters, will not be used.

C O I N.

The monetary unity is called *Franc*. The *Franc* is divided into ten *Decimes*. The *Decime* is divided into ten *Centimes*. The *Franc* does not differ from the ancient *Troie ternaire*; its value is that of a piece of silver of nine-tenths fine, weighing 565 grammes.

New System of Weights and Measures of France.

Myriametre, length often millimetres, equal to about two leagues (or six English statute miles, one furlong and 150 $\frac{1}{2}$ yards).
Kilogramme, weight of 1000 grammes, equivalent to a little more than two pounds, *poids de marc* (or 22966 grains English, which is 3lb 4 $\frac{1}{2}$ oz. very nearly).
Hectare, 100 ares, or 10000 square metres, equal to about two arpents (or about 4000 square yards English, which is rather less than $\frac{1}{2}$ acre).
Decalitre, Measure of capacity for corn, equal to about 4-fifths of the Paris bushel (or 610.83 cubic inches English, which is to the English peck of 537.6 inches, nearly as 8 to 7).
Decimetre, tenth part of a metre, equal to about three (French) inches and a half (or 3.9383 inches English).
Centigramme, hundredth part of a gramme, equal to about 3-16ths of a French grain (or 0.22966 grains English).
N.B. Many compounds, such as *Decalitre*, *Kilatre*, and all those which are formed with the here, will not be used.

C O I N.

The monetary unit is called *Franc*.
 The Franc is divided into ten *Centimes*.
 And the *Decime* is divided into ten *Centimes*.
 The *Franc* does not differ from the ancient *France tournois*: its value is that of a piece of silver of nine-tenths fine, weighing 5 grammes.

PROPORTION between the ANCIENT FRENCH MEASURES and the NEW.

The distance from the Pole to the Equator being $\left\{ \begin{array}{l} 5132430 \text{ toises.} \\ \text{or } 30794580 \text{ feet.} \end{array} \right.$

		Logarithms of the Pro- portions.			Logarithms of the Pro- portions.
Value of the metre in Paris elis	0,841712	9,925164	Value of the Paris ell in metres	1,188,05	0,074836
Value of the metre in feet	3,07946	0,488474	Value of the foot in metres	0,324732	0,511526
Value of the square metre in square feet	9,48306	0,976048	Value of the square foot in square metres	0,105451	9,023052
Value of the are in poles or perches of 18 feet square	2,92687	0,466403	Value of the pole or perch of eighteen feet square in ares	0,341,662	9,533597
Value of the cubic metre in cubic feet	29,2027	1,465423	Value of the cube foot in cube metres	0,0342434	8,534577
Value of the litre in Paris pints	1,05130	0,021725	Value of the Paris pint in litres	0,951206	9,978275
Value of the decalitre in parissh bushels	0,788473	9,896787	Value of the Paris bushel in decalitres	1,26827	0,103213
Value of the gramme in grains, <i>poids de marc</i>	18,841	1,275104	Value of the grain, <i>poids de marc</i> , in grammes	0,053076	8,724896
Value of the decagramme in ounces	0,327101	9,514681	Value of the ounce in decagrammes	3,05716	0,485319
Value of the kilogramme in pounds, <i>poids de marc</i>	2,04438	0,310561	Value of the pound, <i>poids de marc</i> , in kilo- grammes	0,489146	9,689439

II.

Analysis of a Memoir of Citizen BONHOMME on the Nature and Treatment of Rachitis, or the Rickets.

[Concluded from page 177.]

THE following are the experiments by which Citizen Bonhomme appears to have succeeded in proving this truth. I shall transcribe his own words.

"I caused several young fowls of the same incubation to be fed in different manners. Some received the usual food without any mixture; others received daily a certain quantity of calcareous phosphate mixed in the same paste as formed the support of the others; and lastly, one of them was fed with variations in the use of the mixture: the calcareous phosphate was sometimes given and sometimes suspended. When these fowls, after two months, had acquired their ordinary growth, I examined and carefully compared the state of their bones. The progress of the ossification in the epiphyses was various according to the nature of the food the animal had received. The bones of the last fowl, which had received the phosphate only from time to time, were rather more advanced than the bones of those which had

been fed without mixture. The bones of those fowls which had been habitually fed with the mixture were evidently more solid, and their epiphyses were much less perceptible. Simple inspection was sufficient to shew these differences when the bones were mixed together.

"I had fed several young fowls of the same incubation according to another plan. Some were fed on a simple paste, without mixture; for others it was mixed with pulverised madder-root; and a third composition was made of this last paste and calcareous phosphate. This was also given habitually to other fowls. When after two months I examined the progress of ossification in the bones of these different animals, I easily perceived the red traces of the madder in the ossified parts of all those which had used it; but I observed that the ossification was not more advanced by the simple mixture of this root than by the ordinary food: on the contrary, the bones of those fowls which had swallowed the phosphate mixed with madder were much more solid than the others. The red colour served admirably to distinguish the extremities of the long bones from their epiphyses. After an exact comparison, there could be no doubt of the efficacy of calcareous phosphate in favour of the progress of ossification. The virtue of the madder seemed confined to that of giving colour to the ossified parts."

From these experiments, it was natural to make the trial of calcareous phosphate in addition to the remedies made use of in the treatment of rachitic subjects. Here follows what the Author himself says, after having spoken of the exaggerated praises given by Haen to the use of oyster-shells (*ostracodermata*) in the treatment of rachitis.

"Without pretending," says he, "to a result so brilliant as that announced by Haen, I can affirm that the calcareous phosphate has succeeded very well in the greatest number of rachitic subjects to whom I have given it. I shall not multiply my accounts of successful cases in this place, but shall relate only two remarkable instances."

"The daughter of Mr. Ranchon, watch-maker, aged two years and a half, walked with a feeble and tottering pace, and the extremities of all her bones presented epiphyses very prominent. In this situation she exhibited the appearance of imperfect rachitis, or the first period of this disorder. Alkaline lotions, which I immediately advised, were attended with a good effect. Her sleep became more firm; and as the first passages were in a good state, I gave, without internal preparation, one scruple of a mixture of equal parts of phosphate of lime and phosphate of soda twice a day. In the course of three weeks her legs were perfectly restored; and this amiable infant has ever since had the satisfaction to run with spirit and agility."

"A female infant, of the name of Boiard, aged four years, had experienced from her birth the most decided symptoms of rachitis. The protuberance of the epiphyses and tumefaction of the abdomen first indicated the disease. The impossibility of supporting herself and walking at the usual age confirmed these unfortunate symptoms. By degrees the glands of the neck and of the mesentery became swelled; the teeth were blackened, became carious, and were not replaced. This situation became still more afflicting by crises almost periodical at an interval of three or four weeks. At these afflicting periods, a fever of considerable strength, cardialgia, and even convulsions, particularly in the night, were observed. The termination of each paroxysm was announced or ascertained by abundant stools, and the evacuation of urine strongly charged with an earthy sediment. The

imprudent exhibition of a purge at the beginning of one of these crises had nearly deprived the patient of her life. In this state it was that I beheld her for the first time in the month of January 1791. The alkaline lotion was the only remedy the mother adopted in the first instance, and it produced a remarkable effect. After eight days the infant was so much better as to be able to support herself. The remedy was then laid aside, and eight days afterwards the child was incapable of standing without support. The use of the alkaline solution being renewed, was attended with the same success, and its discontinuance was again followed by the complete return of all the symptoms. In the first days of March, the other remedies I had advised were exhibited. The constipation which had always existed became less, and the following crisis was effected without pain. And at length the convulsions, the pains, and the crises disappeared; but the impossibility of walking still remained. At this time, namely on the second of May, I gave the child the phosphate of soda and calcareous phosphate mixed together, in the dose of half a dram twice a-day. At the end of the month she was able to stand upright, leaning against a chair, and the swellings began to diminish. She continued for a long time afterwards to take the mixture of the phosphates. I likewise gave her occasionally one grain of the extract of bile, prepared with spirit of wine; and at length in the month of July I had the pleasure to see the patient run and play in the middle of the street with the other children of her own age, &c.

"J. B. Magné, aged two years and a half, appeared strong and well formed. A general rachitis manifested itself rapidly in his constitution, without apparent cause. He soon became incapable of walking, and his relations learned from his playfellows that he had frequently evacuated white and thick urine. Most of the remedies and methods proposed by different authors were made use of. At length discouraged at so many useless trials, the parents gave up every remedy, and the child remained incapable of walking. The bones of the legs were softened and bent. At the age of four years and a half young Magné had the small-pox; the disorder was acute, but terminated happily without any particular attention. When the desquamation was complete, the child was no longer capable of supporting himself. At this period he was brought hither, and I was consulted. I observed that part of the bones were so much softened as to occasion variations and even violent pain in his walk. I advised a purgative, with alkaline lotions and the use of calcareous phosphate, and a proper regimen. Eight days afterwards, the whole surface of the skin was covered with an infinity of small blisters, resembling the itch. My prescriptions were regularly continued, and in one month the solidity of the bones was entirely secured. For more than a year he has experienced no relapse. In this case we may remark the inutility of the usual remedies, the advantage of cutaneous eruptions, and the efficacy of the treatment I propose."

The incurvated spine, (though apparently confined to the vertebræ, which bones are not only affected with preternatural enlargement, but frequently also with caries) is this disorder, I say, to be regarded as analogous to rachitis, or at least to accidental rachitis? Both have been produced by the repercussion of cutaneous eruptions. Citizen Bonhomme relates observations in which the two affections are so combined that they appear to form one single disorder, varied only according to the parts affected. Is the use of calcareous phosphate applicable to the treatment of the vertebral disease, to complete and

accelerate

accelerate the frequently partial process of other remedies? The Author thinks that the following observation is conclusive in this respect:

"At the end of the month of February 1790," says this physician, "a rachitic child, aged eight years (J. Esprit Guinde), was presented to me in the General Hospital of this town. He had been for several weeks among the patients under the care of the surgeon. At the first moment a prominence of the first dorsal vertebræ was observed, with a considerable weakness of the legs. A large blistering plaster had been applied over the commencing gibbosity, and the blister was kept open by powder of cantharides for more than a month. The situation of the infant was not amended. I remarked in this patient all the characters of the disease described by Pott. I concluded that the blister had produced as great a discharge as was necessary, and that it was more essential to oppose the progress of the rachitic habit. I ordered the whole of the spine to be carefully washed three times a-day with an alkaline and aromatic lixivium. The phosphate of soda was mixed with twice as much powder of burnt hartshorn. Of this I gave to the child one scruple twice a-day in a spoonful of soup. After the first week he was able to walk with assistance; by degrees the solidity of his legs was re-established, and the spine appeared to recover itself. And at length, on the 31st of December, when I quitted the service of that hospital, the boy thought himself sufficiently recovered to return to his friends.

"Eight or nine months afterwards, I found this unfortunate child in another hospital appropriated to the reception of scrophulous patients. From his answers to the questions I proposed, I found that the treatment of the month of December had not been continued long enough to destroy the principle of the rachitis; that the disorder had been as it were subdued during six months, but that coarse food had developed it again. And lastly, that the swelling of certain glands, and a diminution of the solidity of several ribs and of the humerus of the right side, were the effects of its renewed activity. I ordered purgatives to be repeated every eight days, the alkaline lotion, and the mixture of the phosphate of lime and soda; in addition to which I prescribed the use of the saponaceous and mercurial pills. The efficacy of these remedies was very sensible. The advances of the rachitic disorder were stopped; the evacuations were kept up; *the distortion of the ribs and of the humerus disappeared*; but the original gibbosity remained. This deformity, which could not be effaced, did not diminish the pleasure I had to see this child perfectly cured after three months treatment."

In most of these observations mention is made of alkaline lotions and their beneficial effects. The following is the Author's own account:

"In ordinary cases of rachitis, particularly at the commencement of the disorder, it is of advantage to use a simple solution of pot-ash to wash the parts affected. This solution is made by dissolving from half an ounce to an ounce of purified pot-ash * in a pound of dis-

* M. Halle, in a note on this passage, observes, that pure pot-ash, or the vegetable alkali, is a most powerful caustic, and cannot be used in these proportions. He found a solution of only one-eighth part of the salt here indicated was too strong a lotion for the skin of an infant which was very irritable. Citizen Bounhomme, upon enquiry made, acquainted him that the pot-ash he used was that of the shops, which is very far from being in a caustic state. Probably it may be the same as the common salt of tartar of most of our shops in London, which is nothing but pot-ash or pearl-ash dissolved in water, then filtered, and the water driven off by evaporation. It accordingly retains all its saline simplicity. N.

tilled or very pure spring water. When it is to be used, the skin must first be rubbed with a dry cloth, or a piece of fine flannel. After this precaution, the diseased extremities are to be washed carefully with the warm solution, and at length wiped, so as to leave no trace of moisture. This practice and washing must be repeated at least twice a-day. I can affirm, from repeated trials, that it will soon be attended with success.

"The solution of por-ash is not very costly; nevertheless, the habitual and long continued use of this remedy for a rachitic patient in the second period, becomes expensive to poor parents. The lixivium of wood-ashes, which has been used for washing fine linen, in which aromatic plants may be infused, becomes of remarkable utility for the rachitic children of the poor. I have seen the most decided success result from its use. Several children, who, after having walked alone, found a difficulty in supporting themselves, were washed two or three times a-day with this lixivium, and in the space of one or two weeks they walked with ease and agility, &c.

"I have seen various instances of children cured of their disposition to rachitis merely by washing with alkaline liquids; but in most cases I have thought it necessary to secure the first success by other remedies. When the rachitis has already made some progress, it is evident that all the remedies must be united, and steadily continued. The alkaline lotion is a remedy the more preferable, as it is not at all disgusting, and scarcely in any respect troublesome to children; but the internal remedies possess superior efficacy."

If the facts here announced should be confirmed by further experience, might we not hope to obtain similar advantage in such other diseases as attack the substance of the bones, and probably have more analogy to the rachitis than has been imagined? Such are the *spina ventosa*, scrophulous tumours and caries, the difficulties in the formation of the bony process, after fractures, slowness and irregularities of dentition, &c. These questions, highly deserving of investigation, are produced by the Author of this memoir as consequences of what he has related. He likewise communicates some useful notions respecting the cases in which the other combinations of phosphoric acid may be employed, such as the phosphates of iron and of mercury, concerning which experience has yet afforded him no information.

With the same candour and diffidence as is displayed by the Author of this memoir, we present his experiments as essays worthy of the attention of enlightened physicians. We have no doubt but the Author will direct his attention in future to extend his proofs and increase the degree of precision and evidence. And even if others should go beyond him in this important enquiry, he will nevertheless enjoy the honour of having begun it.

Let us conclude this abstract by a few reflections. It seems to us, that a multitude of ideas must present themselves to him who shall meditate on the facts here displayed. Without dwelling on those which are still too hypothetical, the single fact of the transition of calcareous phosphate through the passages of circulation, and consequently its solution in the fluids, and its application to the substance of the bones, deserves the highest attention. The means which nature employs to render the calcareous phosphate soluble are well entitled to the researches of chemists: without doubt its combination with soda may have some part in this effect; but it is not less important, in this place, as a remarkable phenomenon in the animal economy, which is essentially connected with nutrition, and the development of our organs. If we compare the experiments of Vauquelin on the seminal

fluid, with those of Fourcroy on the serosity of milk, and more especially a remarkable fact communicated to us by this last chemist, namely, that the nearer the milk of the human female approaches to the period of parturition, the more the serosity is charged with calcareous phosphate; and the more remote, on the contrary, the time is, from that moment, the less is the proportion of that substance, while the other nutritive parts of which it is composed are augmented in an inverse ratio;—if we consider that at the epocha of gestation and parturition a diminution of solidity takes place in all the articulations of the mother, and a relaxation of the cartilages; that fractures of the bones at this time are most particularly slow in uniting by the formation of the callosity; that it is at this very time of change in the osseous system that the milk becomes charged with calcareous phosphate, which it loses in proportion as the infant and the mother become more removed from the time of the birth; so that the fluids which contribute most to the formation, the growth, and the nutrition of the fœtus, contain in themselves the essential principle of solidity, the elements of ossification; and lastly, that it is not till this ossification is very distinct, and the digestive organs of the infant are sufficiently strengthened to answer the purposes and the work of animalization, that the basis disappears from the milk of the mother;—we shall be compelled to acknowledge a particular direction of nature, by which the calcareous phosphate becomes the matter of a peculiar secretion, essentially ordained to give firmness to our organs, and to consolidate the first elements of man.

May these reflections impart to medical men a conviction how truly important the physical sciences may become to their researches, though too often regarded as merely accessory to medical science! May they see the advantage and necessity of studying the progress of nature in these respects! For it is by these sciences, and more especially by the improvements of animal analysis, which are rapidly advancing, that we may probably expect in a short time to diminish the uncertainty of a large number of our methods of practice, and, to speak freely, the too frequent ambiguity of our conjectures.

III.

*Abstract of a Memoir read at the Sitting of the French National Institute on the 26th Pluviose, containing an Account of some Experiments concerning the Section which Cylinders of Camphor undergo at the Surface of Water; with Reflections on the Motions which accompany this Section. By J. B. VENTURI, Professor of Natural Philosophy at Modena, Member of the Institute of Bologna, &c.**

ROMIEU formerly observed, that small pieces of camphor have a progressive and rotatory motion upon water, and attributed this effect to electricity. Lichtenberg attributed it to the emanation of an ethereal spirit from the camphor itself. Volta produced the same motion of turning, by throwing upon water small bodies soaked in ether, or particles of the acids of benzoin and amber. Brugnatelli found that the bark of aromatic plants moves on the surface of water like camphor. There was always a certain mysterious caprice in these motions, according to which they sometimes could not be produced; and on other

* Annales de Chimie, XXI. 462.

occasions, the motions were instantly stopped when the water was touched with certain bodies, without its being easy to guess the reason. When particles of camphor were fixed to the extremity of a very delicate electrical fly, they produced no motion. All these circumstances tended to envelop the phenomenon with obscurity. Other philosophers in Italy constantly supported the opinion of Romieu in their publications. Citizen Venturi communicated his notions on these facts two years ago at one of the public sittings of the Literary Society of Modena, his country. He was present at the sitting of the National Institute on the 21st Pluviose, when a memoir of Citizen Prevost on the emanations of odorant bodies was read *. He announced the observations he had made; and in consequence of an invitation to that purpose, he communicated a memoir of which the following is an abstract:

Pieces of camphor were cut into the form of small columns, one inch in length; a bafe of lead was fixed to each column; they were then placed upright in very clean faucers, and pure water poured in, to half the height of the column. Two or three hours afterwards, an horizontal notch was manifest in the column of camphor at the surface of the water; in the course of twenty-four hours, or thereabouts, by the notch becoming gradually deeper, the column of camphor was cut in two at the middle. The two pieces of the column, nevertheless, that is to say, the lower, which was immerfed in the water, and the upper 'in the air, suffered scarcely any perceptible diminution.

From this experiment, and others made with different pieces of camphor, kept separately in the air, in the water, and at the surface of the water, the Author deduces that the most active virtue for dissolving camphor resides at that part where both the air and the water touch the camphor at the same time. Hence he explains, why, in like circumstances, camphor evaporates more quickly in a moist than in a dry air; and why the Hollanders use water in their process for subliming this substance.

It might be thought that the camphor was decomposed at the surface of the water; that the water might seize the acidifying part, which renders the camphor concrete; and that the volatile part is dissipated in the atmosphere. The Author rejects this notion. He thinks that water with camphor floating on its surface becomes charged with no more than a very small portion: 1. Because in these circumstances the water acquires the same taste and smell of camphor as it obtains when a small quantity of this substance is kept plunged in the same fluid. This water, by exposure to the air, loses the qualities with which it had been charged, and becomes insipid, and without smell. 2. Because when the water is saturated with all it can take up, the dissipation of the camphor continues at its surface as before. 3. Because the aerial emanations of camphor made at the surface of water, do themselves crystallize into camphor.

Camphor, at the surface of the water, does nothing, therefore, but dissolve; and when dissolved at the ordinary temperature of the atmosphere, it is not at first in the state of vapour, as has been thought. It is simply a liquid which extends itself over the surface of water itself; and by this means coming into contact with a great surface of air, it is afterwards absorbed and evaporated. This is proved by the following facts: 1. The solution of camphor at the surface of water is more rapid in proportion to the extent of the surface.

* Philosophical Journal, I. 153.

In narrow vessels, the section of the column would not be completed in a decade (ten days), even though the water might be extremely pure. 2. When the column of camphor has projecting parts, the liquid may be seen issuing by preference from certain points of the column, covering the surface of the water, and driving small floating bodies before it, in the same manner as floating bodies go and return in a basin into which the water of a canal enters with rapidity. 3. If a small piece of camphor, already wetted at one end, be brought near the edge of water contained in a broad saucer, and be made to touch the saucer itself, it deposits a visible liquor, which is oily, and by attaching itself to the saucer destroys the adhesion between the vessel and the border of the water; so that the water retires on account of the affinity of aggregation, which not being opposed by the attraction of the saucer, causes the water to terminate in a round edge. If you remove the piece of camphor, the water will not return to its place until the oily fluid is evaporated. 4. In the same manner, when the column of camphor is half immersed in the water, the oily liquor which issues forth destroys the adhesion of the water to the column, and produces a small surrounding cavity. The solution stops, or is retarded for a moment, until the fluid, extending itself over the water, becomes evaporated: the water then returns to its place, and touches the same part of the camphor; the solution begins again, and in this manner the process is effected by alternations of contact and apparent repulsion.

The rotation of small pieces of camphor at the surface of the water, is simply the mechanical effect of the re-action which the oily liquor, extending itself upon the water, exercises against the camphor itself. If the retro-active centre of percussion of all the jets do not coincide with the centre of gravity, a combined motion of rotation and progression must follow. Since the departure of the oily solution takes place only at the surface of the water, the rotation cannot be effected but round an axis perpendicular to the horizon; and since in similar bodies of different magnitudes the algebraic ratio of the sides to the mass increases in the inverse duplicate ratio of the sides themselves*, the small particles must have proportionally more jets, and must revolve more speedily than the larger.

The Author reduces to one general rule all the apparent irregularities observable in the motions of the camphor. When these small parts are briskly moved at the surface of the water, if you touch this fluid with any other body, whether conductor or non-conductor of electricity is of no consequence, provided it be well cleaned from every oily substance; in this case, the motion of the camphor will not be affected. But if the same body be afterwards greased by a small drop of fixed oil, or a greater quantity of volatile oil, and then applied to the water at one extremity of the plate, you will discern a scarcely perceptible film advancing at that instant over the whole surface of the water, which will repel the small morsels of camphor, and, as if by a stroke of magic, deprives them of their motion and vivacity. One ounce of oil, poured on one extremity of a basin of water of twenty feet diameter, very soon stops the motion of camphor revolving at the other end. This rapid diffusion even of a fixed oil over a great surface of water, prevents the camphor from extending itself, and stops the solution and rotation of the small particles. It is for this reason, likewise, that particles of saw-dust, soaked in fixed oil, begin to turn the moment they touch

* So in the original. In truth, the sides of like solids are in the inverse triplicate ratio of their solidities. This oversight of the ingenious Author does not impair his deduction with regard to the velocities of rotation. N.

the water, but cannot continue their motion because the film which they form at the surface of the water is not dissipated in the atmosphere. From this last observation the Author deduces this consequence, that volatility and the odorant property are not qualities necessary to produce these rotations. Volatility is merely necessary for their continuation.

At the end of his memoir, the Author speaks of certain other motions observed in nature, which, by the mechanism of their cause, are in some respect analogous with the motions of oily bodies at the surface of water. In bodies brought near the fire, the humidity retires always to the extremities most remote from the fire itself; because the vapour which is disengaged from the part most heated repels the rest in the opposite direction. So likewise drops of water, thrown on an ignited plate of metal, remain, and are agitated in the form of a sphere; because the vapour which is produced by the contact of the plate agitates them, and does not permit them to touch the metal. By the same principle, it is very easy to explain the motions of the tremella described by Lanson and Corti, which had inclined some naturalists to rank this byssus in an intermediate class between plants and vegetables. The tremella is a mass of very minute fibres, which being suspended in the water must be sensible to the slightest impression. It is at present known, that plants, when exposed to light, decompose the water in their vessels, of which they seize the hydrogen, and extrude the oxygen in the form of gas, by an operation nearly the reverse of animal respiration. If from one of the fibres of the tremella the gas issues on one side, the fibre itself must bend toward the opposite part. If, as in large trees, the gas issues from the tremella in the greatest quantity from the side of the plant which is opposite the light, the tremella must be repelled towards the source of the light itself, to which it always tends when included in an opaque vessel, into which the light enters through a hole. It is not therefore necessary for the explanation of this fact, either that we should suppose any degree of animality in the plant, or attraction at a distance between the oxygen and the light. In the same manner, the gas which is abundantly produced by the light of the day, becomes accumulated between the fibres of the tremella, and floats it to the surface of the water. The Author takes occasion to express his acknowledgment to Citizen Fourcroy, who afforded him the means of repeating the experiments in his laboratory, and who, by his excellent lessons, informed him of the facts newly discovered since the communication between his country and France had been unfortunately interrupted.

ADDITIONS to the preceding MEMOIR. In a Letter from the AUTHOR to Citizen FOURCROY.

CITIZEN;

AS I understand that an extract of my memoir upon the section of camphor at the surface of water is intended to be printed, I request you will add the following facts and reflections:

1. Dry camphor is very perceptibly volatilized under the weight of the atmosphere, at the 50th degree of Reaumur (145 Fahrenheit). It melts at the 120th degree (302 Fahrenheit), and its volatilization is then extremely rapid. In the Torricellian vacuum it rises even at the ordinary temperature of the atmosphere. This vapour has very little elasticity. It crystallizes along the sides of the tube.

2. A column of camphor is cut asunder much more speedily at the surface of boiling than

than of cold water. Camphor upon boiling water sublims in great abundance with the vapour of the water itself.

3. Camphor, when floating upon water, turns and is dissipated by the contact of the oxigene, carbone, hydrogen, and azotic gases. These two last afforded more rapid movements, and a more speedy dissipation. They likewise more readily dissolve carbone, phosphorus, and sulphur.

4. When camphor is burned or heated on a float at the surface of water, if it touch the liquid, it gives a considerable motion to its support; but if it do not touch the water, it remains motionless. The motion is not therefore produced by the simple emission of volatilized particles from the camphor. An action likewise takes place on the part of the water.

5. This action appears to me to depend on the principle of the motions of bodies which float at the surface of water, and has been explained with remarkable perspicuity by Monge. Of two small pieces of paper, twisted up and moistened, the one with the pure water, and the other with water well saturated with camphor, the first attracts and the second repels camphor on the surface of water, which does not actually hold that substance in solution. Water consequently has more attraction for solid camphor, than for the little portion which it has already dissolved to saturation. It mounts along the solid piece, where it forms a curvilinear inclined surface. The small portion which is dissolved and saturated, descends along this inclined surface, and in its descent it repels backwards, by the laws of mechanics, both the surface itself, and the solid particle to which it adheres. This separation of the dissolved part accelerates the dissipation of the solid piece, by affording a current of water constantly renewed. The atmosphere absorbs that part of the camphor which is already dissolved and extended on the surface of the water; the evaporation being perhaps assisted by a small portion of the water itself.

6. If a drop of oil had no affinity with the surface of the water, it would lodge itself in a small cavity; and, though more elevated than the surface itself, it would preserve the globular form by its affinity of aggregation. But since it extends in a film over the water, the drop itself, or some of its principles, must necessarily be attracted in the same manner as fluids which rise along the sides of vessels.

7. Air, strongly impregnated with ether, or very hot exhalations of camphor, exercises on the small floating particles at the surface of the water a repulsion similar to that of oil, or of camphor dissolved without heat in the water itself. The former are elastic fluids, and the latter simple liquids. They must not be confounded together.

I have perhaps dwelt too long upon a subject which seems rather to be matter of curiosity than immediate utility. But it is always a pleasure to me to embrace an opportunity of expressing my esteem and attachment to you.

J. B. VENTURI.

IV.

*Analysis of four Specimens of Steel; with Reflections on the new Methods employed in this Analysis.
By Citizen VAUQUELIN*.*

Preliminary NOTE of the (French) EDITOR.

THE steels which Citizen Vauquelin has analysed were sent to the Council of Mines numbered 864a, 864b, 977 and 1024. These steels were produced at the forge of Remmelsdorff, situated in the canton of Gros-Remmelsdorff, in the department of the Moselle, at the distance of 5000 metres north from Bouzonville, and the left side of the Nied. The crude irons which are there refined come from the furnaces of Dilling and Betting, which, according to the report of Citizen Dietrich, obtain their iron stone from the communes of Grefaubach and Steinbach, as well as from the forest of Hommelswald; and their calcareous flux (castine) from the woods of the commune of Mersching. The establishment consists of two fineries, one large hammer, two smaller, and a heating furnace. Citizen Soller, the proprietor, had established before 1785 a manufactory of steel by cementation, which, after having been abandoned for several years, has again been put into activity. The cementing furnace contains, according to the account furnished by this citizen himself to the Council of Mines, about six thousand weight of iron, and can be charged two or three times a month.

Citizen Dietrich, who visited this establishment in 1785 by order of Government, inspected the process, and the furnace of Citizen Soller, of which he gave the most advantageous account. By this report it is seen that Citizen Soller uses coak for cementation†. We think it may be of advantage to transcribe in this place the report which was made on these same steels in 1786, by the Commissaries of the Academy of Sciences. It is thus by comparing the results of operations performed in manufactories under the inspection of learned men, with those afforded by the chemical analysis, that we may hope to learn to what extent the nature and proportion of the constituent parts may influence the qualities of steel; and whether we may expect from chemical science new and direct methods of ascertaining what kinds are best appropriated to the different uses of this substance in the arts.

Extract from the Registers of the Academy of Sciences, March 15, 1786.

ON the 15th of February last, Mr. Soller presented some steel of cementation from his works at Remmelsdorff, in Lorraine, near Saar-Louis. The Academy has charged Mr. Vander Monde and myself to give an account of the same.

We must distinguish three kinds of steel, each of which possesses properties relative to the different uses they are applied to.

1. Natural steel (*acier de fonte*) is obtained immediately from the crude iron. This steel is usually unequal in its quality; subject to have cracks and scales; is less hard and brittle than the two other kinds; welds better, and is principally used for instruments of

* Journal des Mines, publié par le Conseil des Mines par la République, N° XXV. 1.

† Houillé. Doubtless as the fuel; not as the cementing compound. N.

husbandry, common cutlery, and springs. As this kind of steel demands less expence in its fabrication, it is of course the cheapest. The greatest quantity is imported (into France) from Germany.

2. The second kind is steel of cementation. It presents a more equal grain in its fracture, and takes a better polish than the former. It is also harder, more brittle, and requires more particular attention in the forging. This kind being more perfect, is of great use in all the circumstances which require the qualities here mentioned.

3. Lastly, the third kind is cast-steel, obtained by fusion of one of the other of the foregoing kinds. It is characterised by its uniformity of texture, and is deprived of those scabrous places which are observable even in the steel of cementation. This third kind of steel is therefore susceptible of the most beautiful polish, and is proper to form razors, lancets, ornamental work, wire-plates, laminating rollers, &c. The English have hitherto been almost totally in possession of the manufactory of cast-steel, which is the most valuable of any.

Four bars of cemented steel, manufactured by Mr. Soller, and chosen from among the steels of different qualities in his warehouse at Paris, were submitted to the following experiments:

With the first piece some hook-tools* for turning iron were made. They forged very well, the steel being very hard in the fire, and consequently easy to be wrought. The hook-tools, on trial, proved very good. A graver for turning iron in the turn-bench was made out of the same piece. It was tried upon steel, and found to be as good as an English graver.

The second piece was used to make a plane iron for metal. It succeeded perfectly well, and kept its edge in planing iron as long as those which are made of English steel.

Out of the third piece, which was doubled into three, and welded upon a piece of iron, a carpenter's chisel was made 26 lines in breadth. The steel welded perfectly, without requiring any particular care. The edge proved very good, and on comparison with an English chisel of the same dimensions it was found to possess the same qualities.

A carpenter's chisel, 14 lines broad, was made out of the fourth piece of steel. It welded extremely well, and proved to be as good as the foregoing instrument. The workmen estimate that a greater number of English chisels would be found at the venders' below than above this quality.

Mr. Soller likewise sent to the commissioners a sample of iron, which was part of the tie

* The hook-tool is used by the turners of strong metallic work; but from its steadiness, and other good qualities, it well deserves to be applied in many delicate operations with steel and brass, by a numerous set of workmen in this capital who are entirely unacquainted with it. Its usual form is that of a hook or claw, less than half an inch long, proceeding nearly at right angles from the side of a straight stem, which last terminates below in a point. The cutting edge is at the extremity of the claw, and is variously figured, according to the nature of the work. By bearing the angular point on the Rest of the lathe, the claw may be applied to the work with great power and steadiness, while the perpendicular handle bears against the shoulder of the workman. The steel commonly used in London for this instrument is called *sheer-steel*, from its mark. The edge stands very well against soft steel, when the tool is a little harder than a hard saw, or just capable of yielding to a good Lancashire file. If it be harder, there is a greater probability of its breaking. This kind of work is performed by a slow motion, with the constant application of water to the piece to prevent heating. It is obvious from these facts that tenacity is the requisite most wanted here. N.

of a faggot of steel, and manufactured out of a mixture of half its weight of old iron with the same quantity of pig-iron. This iron, from which the steel presented to the Academy appears to have been made, is of the first quality. It cannot be broken without tearing. It extends under the hammer with the greatest facility, whether heated or cold.

The samples of steel from Mr. Soller exhibited in these trials the qualities of the ordinary steel of cementation which we receive from England under the name of *acier punte* *. Like that steel, they may be twilled while hot without cracking, but they cannot be compared with the fine English steels.

Mr. Soller has likewise communicated to us the details of his process, and the design of his furnaces. We have every reason to believe that he may fabricate steel of an excellent quality. But to prevent the objections which might be made on the supposition that these pieces had been selected, he has drawn out the whole contents of a case of steel made by a new cementation. This steel will be sent to us, with every attestation in form which can be desired. We shall submit it in like manner to different trials, and render an account to the Academy. In the mean time, we are of opinion, that the enquiries and the undertaking of Mr. Soller are among those which the Academy will never fail to applaud.

Done at Paris, at the Academy, March 15, 1786.

(Signed) { VANDER MONDE
AND
BERTHOLLET.

I certify that the present extract agrees with its original, and the judgment of the Academy.

Paris, March 18, 1786.

(Signed) CONDORCET.

SECTION I.

INTRODUCTION †.

THE analysis of steels is one of the least advanced and most difficult parts of chemistry, more especially when the accurate proportions of constituent parts are required to be determined.

The agents commonly made use of to disunite these principles do themselves suffer part of their elements to escape, which re-act on those of the steel, and put them into a situation which renders it difficult to form any estimate.

Thus, for example, we find, that when steel is dissolved in the sulphuric acid, diluted with water, the hydrogen gas which is developed, dissolves, and carries with it a portion of carbone, the quantity of which varies from a great number of circumstances.

The insufficiency of the methods hitherto proposed for acquiring this knowledge is well shewn from the results they have afforded to those who invented them, and who were no less skilful in experiments than in the art of reasoning.

It is well known that we are indebted to Bergman for the first analytical processes with iron and steel; and that little addition has been made to them since. But these processes

* Is this spur-steel? N.

† This word, and several other sentences or subordinate titles, appear in the margin of the original as sidenotes. I have inserted them in the text. N.

are inaccurate, as we shall shew in the course of the present memoir, and as it also appears from the different results he obtained from the analysis of various kinds of steel, the usual properties of which did not indicate so great a difference.

It is admitted, since the valuable experiments of Bergman, and particularly those of Berthollet and Monge, that steel differs from pure iron only from the presence of a certain quantity of carbone, which is intimately combined; a proportion which perhaps may admit of some degree of latitude; but which, nevertheless, if above or below a certain quantity, affords either an over-cemented, brittle, and too fusible kind of steel, or else steel which too nearly approaches to iron in its qualities.

If steel were merely and constantly a combination of carbone and iron, in determined quantities, it would be easy to ascertain this point once for all; but it almost always happens, that steel contains silic, phosphorus, and sometimes manganese, of which the influence upon the qualities of the steel is not known, even on the supposition that steel is necessarily no more than a combination of iron and carbone.

If we admit that these different substances are not indispensable to the constitution of steel, it must nevertheless be allowed that they will affect its properties by greater or less modifications, dependent on their own quantities respectively.

If, therefore, as it seems out of doubt, the various qualities of iron and steel arise from the several principles of which they are composed, and their respective proportions, it is equally interesting to philosophy and the arts to determine, by chemical experiments, what influence each of these principles may exert in the combination, and to find by simple, ready, and cheap experiments, in what uses these metallic substances may be most advantageously employed. But in order to arrive at this desirable termination, a more precise knowledge is required of the usual properties of the known kinds of iron and steel, compared with the chemical nature of those matters. There is not, however, any other method than that of examining the two means at the same time, by which we may succeed hereafter in establishing and fixing by chemical essays the qualities of the kinds of iron and steel already used, and even those which may hereafter be fabricated.

It may be easily conceived that this undertaking would demand a great number of experiments. For it not only requires a knowledge of the number and nature of the elements necessary for the formation of steel of the best quality, but likewise the proportions of these elements, which may be infinitely varied, and to determine besides the modifications which may be produced by various quantities of substances not essential to the constitution of this metal.

In the mean time, till circumstances shall permit the execution of this useful plan, I proceed to offer the results of the analysis of four kinds of steel, the difficulties I have found in the progress of this work, the means I have used to overcome them, and the methods I have substituted, instead of ancient processes, to ascertain and measure the essential and accidental principles of the steel.

SECTION II.

STEEL, NO. 864—SMALL PIECE.

Experiment I. 576 grains, or 30,57 grammes, of this steel, reduced into filings, and dissolved in sulphuric acid, diluted with five parts of water, afforded 1,98 grains of black residue.

Experiment

Experiment II. 100 grains, or 5.3 grammes, of the same steel dissolved in the requisite quantity of sulphuric acid, produced 108 inches, or 2169 millimetres cube of hydrogen gas.

New Process to separate the Phosphate of Iron from Steel.

Experiment III. The excess of acid contained in the solution No. 1. having been saturated with carbonate of pot-ash, there were deposited 19 grains, or about one gramme, of a white tasteless powder, completely soluble in muriatic acid.

This matter, boiled in a solution of caustic soda, assumed a deep red colour, and greatly diminished in volume.

The liquor having been filtered, and mixed with concentrated muriatic acid, gave no sign of effervescence, and formed, before and after its mixture with the muriatic acid, a white precipitate, on the addition of lime-water.

This calcareous deposition, being washed and dried, was soluble in acids, and without producing any effervescence; whence we may conclude that it is phosphate of lime, and consequently that the steel No. 864, the small piece, contains phosphorus. Experiments still more positive afford an incontestable proof of the truth of what is here advanced on the nature of this substance.

The Inconvenience of using the Sulphuric Acid to determine the Quantity of Carbone contained in Steel.

CHEMICAL experiments having demonstrated that hydrogen, when it develops itself from the interior part of bodies which contain carbone in a very divided state, dissolves a certain quantity, which depends on the more or less elevated temperature, and the more or less rapid disengagement of the gas, I had reason to presume that the results offered by the experiment numbered 1, could not give an accurate expression of the quantity of carbone contained in the steel. I therefore sought another method, in which the iron dissolved without the disengagement of hydrogen gas should afford the real or absolute quantity of carbone which renders it steel.

New Method of accurately determining the Quantity of Carbone contained in Steel.

AS the sulphureous acid possesses the double advantage of dissolving iron without producing gas, and does not act upon the carbure of iron, it presented this desirable requisite.

Experiment IV. I put into a bottle 288 grains, or 15.28 grammes, of steel, in fine filings, with about two pounds, or about 978.24 grammes, of distilled water, and I passed into this bottle sulphureous-acid gas, disengaged from the sulphuric acid by mercury*. When the sulphureous acid had ceased to act on the steel, I carefully decanted the liquor with a siphon. I washed the black precipitate several times with distilled water. After desiccation by the gentle heat of the stove, it weighed seven grains, or about 0.37 grammes.

We see that this quantity of carbure of iron is much more considerable than that of the first experiment. For 576 gave only 1.98, whereas, in the present experiment, I obtained 7 from 288. But on examining this matter, I found that it contained sulphur in a state of mixture. For by exposing it on a heated metallic plate, it took fire, like sulphur alone.

* We shall not in this describe the phenomena which appear during this operation. They have been exposed by Citizen Berthollet, and we have developed and extended them to other metals in a particular memoir on the metallic sulphates. V.

To extract this combustible substance from the carbure of iron, I heated the mixture slightly with a solution of caustic pot-ash; and having left the matter to settle, I decanted the fluid. The powder, then, after washing and drying, weighed no more than four grains, and gave no further signs of sulphur by combustion*. This experiment evidently proves, that in the experiment No. 1, where sulphuric acid was used, the greatest part of the carbone was dissolved, and carried off by the hydrogen gas, since 576 grains afforded only 1.98 grains of this matter; though the same quantity of steel with the sulphureous acid afforded eight grains. This steel contains therefore 0.014 of its weight of carbure of iron.

Analysis of the Carbure of Iron.

Experiment V. To ascertain the quantity of carbone contained in the four grains of carbure of iron obtained in the experiment No. 4, they were submitted to the action of fire in a small porcelain faucer, under a muffle. As soon as the temperature was sufficiently raised, it took fire, and left 1.9 grains of a grey yellowish mass, which treated with the boiling muriatic acid became white, and was reduced to 0.44 grains.

The muriatic acid acquired in this operation a lemon colour. It afforded with very pure prussiate of pot-ash a blue precipitate, and with ammoniac reddish flocks, which consisted of oxide of iron. The 0.44 grains of white matter insoluble in the muriatic acid, presented on examination all the characters of silic.

From these experiments it follows, that the steel No. 864, small piece, contains phosphorus, carbone, and silic, of which the proportion with the mass of iron is established in thousandth parts in a table at the end of this memoir.

Essay to discover the Presence of Manganese in the Steel No. 864, Small Piece.

Experiment VI. Bergman having announced the presence of manganese in all the iron and steel he examined, it became a necessary part of my plan to examine whether this metal existed in the steel which formed its object. Accordingly I took a known quantity of the solution of 576 grains, (*Experiment No. 1.*) from which the phosphate of iron had been separated. I precipitated it by well saturated prussiate of pot-ash, to which I had added a small portion of acid, in order that there might be no precipitation of iron in the state of oxide.

The washed precipitate was then boiled with muriatic acid a little diluted. The mass of the matter was perceptibly diminished, and the liquor assumed a purplish red colour; which phenomenon led me to presume that this steel contained manganese. During the solution or rather the ebullition of the liquor, a very perceptible smell of prussic acid was emitted. The muriatic fluid, mixed with a solution of carbonate of potash, afforded a white precipitate, which became yellow by the contact of the air. This precipitate, dried by the stove, and exposed to the action of heat, under a muffle, in the cupelling furnace, acquired a black colour. It was again dissolved in the muriatic acid, and, upon the addition

* It appears that pot-ash acts not only on the sulphur, but that it dissolves also a portion of iron. When an acid is poured into the fluid, there is found at first a black precipitate, which is soon followed by another white deposition. This precipitate being collected and examined afforded sulphur and perceptible traces of iron. The four grains of black precipitate obtained in this experiment are therefore carbone in a pure state, or nearly so. V.

of prussiate of pot-ash, afforded prussiate of iron as beautiful as at first. The second precipitate, treated as before, and strongly heated under a muffle, became at first black, but afterwards acquired a brown-red colour, which began to destroy the opinion I had conceived respecting the existence of manganese.

Nevertheless, to clear up this doubt, I applied the sulphureous acid to the calcined precipitate, in the hope of dissolving the manganese, if it should exist, without touching the iron. But I was convinced that a small portion of iron only had been taken up; for the sulphureous acid afforded a blue precipitate by the prussiate of pot-ash, and a deep green by caustic pot-ash. This proves at the same time that the iron does not contain manganese, and that this method is insufficient to demonstrate its presence; more especially to separate it from the iron. For, on the one hand, the muriatic acid decomposes, or at least dissolves, the prussiate of iron; and, on the other, the sulphureous acid likewise dissolves the oxide of iron.

This process being founded on the assertion of chemists, that the prussiate of iron is insoluble and not decomposed in acids; and that the prussiate of manganese, on the contrary, is easily dissolved by the same agents;—if the last assertion be true, the present experiments prove that the first is false. It is nevertheless probable, that the weaker acids, such as the acetous and sulphureous, greatly diluted, may not have so evident an action on the prussiate of iron; and that they may be preferable to separate it from the prussiate of manganese. But experiment has not yet decided in this respect, though it is certain that it is by no means a matter of indifference what acid is employed in this operation.

The Method of Bergman to separate Manganese from Iron.

THE bad success of this process engaged me to follow another method proposed by Bergman, in his Dissertation on the White Ores of Iron, and other parts of his works, to separate iron from manganese.

I took one part of the oxide of iron which had been precipitated from the solution of the same steel by the caustic alkali, and afterwards washed, dried, and calcined in the cupelling furnace. Upon this I repeatedly boiled the nitric acid to dryness, and calcining it in each operation. Lastly, I treated it with the same acid in which a small quantity of sugar had been dissolved. During this operation, a great quantity of nitrous gas was disengaged, and the fluid assumed a brown colour. After evaporation to dryness, and washing of the residue, part was dissolved in water. The solution afforded a beautiful blue precipitate by the prussiate of pot-ash, green by caustic pot-ash, and white by the carbonate of pot-ash.

The sulphuric acid, poured on the ferruginous solution, did not disengage nitrous vapours, but a white fume, the smell of which considerably resembled that of the acetous acid.

It is therefore proved, that the method proposed by Bergman, and followed and repeated by chemists in general, is very faulty, because it favours the solution of iron, and the acid does not, when applied to a mixture of this metallic gas and the oxide of manganese, act exclusively on the latter, unless by accident such a quantity of sugar should be employed as might be capable of forming so much acid only as could dissolve the oxide of manganese alone. But how is it possible to ascertain this quantity, when the proportion of the two metals is unknown?

A New Method for separating Manganese and Iron, and determining their Proportions.

THE uncertainty of the methods proposed by chemists to ascertain and determine the quantities of manganese contained in different kinds of iron and steel, induced me to seek another which combines simplicity and accuracy. I think I have obtained, in this respect, all the success which can be desired in a matter of such importance.

For this purpose, a known quantity of iron crude or malleable, or of steel, suspected to contain manganese, is to be dissolved in sulphuric acid, diluted with four or five parts of water. The solution is to be precipitated with the caustic alkali; and the precipitate, after washing, and drying in the air, is to be calcined under the muffle of the assayer's furnace. When the calcination is as much advanced as it can be by this method, the metallic oxide is to be weighed, and dissolved in muriatic acid; and after having evaporated the excess of acid, a solution of carbonate of pot-ash, well saturated with carbonic acid, is to be poured into the fluid diluted with water, until no more precipitate is afforded. The filtered liquor is then to be boiled; and if it contains the oxide of manganese, this metal will fall down in the form of a white powder, which is a true carbonate of manganese.

This process is founded on the consideration that the carbonate of pot-ash, well saturated with carbonic acid, does not precipitate the salts of manganese, because the quantity of carbonic acid contained in the pot-ash necessary to the saturation of the acid, united to the oxide of manganese, is sufficiently great to hold the latter substance in solution; whereas the ferruginous salts, when their basis is highly oxidized, are entirely precipitated by the same re-agents.

On this subject I have made many experiments, synthetical and analytical, upon very small quantities of iron and of manganese, which have left me no doubt with regard to the accuracy of the method. But the manganese exists in iron in the metallic state; and in the present experiment it is obtained in combination with oxigene and carbonic acid. It is necessary, therefore, to divide the mass, after drying it in the air, by 2.5. Thus, if 100 parts of carbonate of manganese be obtained, the metallic part will be 100 divided by 2.5, that is to say, 40.

I have not yet had occasion to apply this method to a very great number of specimens of steel or iron; those which I have hitherto examined have exhibited no traces of manganese. Whence I am strongly inclined to think that Bergman, who found this substance in every kind of iron and steel, must frequently have mistaken iron for manganese, particularly in the instance where he affirms that he obtained 30 per cent.

[To be continued.]

V.

Description of an Artificial Rock Crystal produced in the Humid Way. By M. TROMMERS-DORFF, Professor of Chemistry at the University of Erfurt.*

IT is well known that flex, either pure, or mixed with heterogeneous substances, and crystallized in beautiful transparent columns, constitutes rock crystal. The curious enquirer,

* Journal der Pharmacie für Ärzte, Apotheker und Chemisten, 2 Band. 1 St. pag. 76.

who for the first time beholds a fine group of these crystals, will naturally be disposed to ask, What are the means employed by nature to produce this form? Does this power operate by the humid or by the dry way?

The secrets of nature are concealed behind a thick veil, which is seldom to be penetrated. Too often it happens, that when we think ourselves in possession of the object of our enquiry, we find ourselves deceived by the varying powers around us, which exhibit new forms. The presumptuous spirit of man becomes impatient; cool experimental investigation is laid aside; the powers of imagination are resorted to, and a region of chimerical images presents itself. Vanity and indolence both conspire to render the philosopher a creator of hypothetical worlds. Under these circumstances it is not surprising that a variety of discordant theories have been invented concerning the production of rock crystal; that imaginary reasoning has been offered and received, and that no chemist has yet succeeded in the artificial production of this substance.

The extent of our deductions by analogy is, that flint, before its crystallization, must have been dissolved in some liquid; but the question is to determine which? In fire this is not credible, for flint is apyrous. In acids?—Except the acid of fluor there is no other which acts on this earth, and this acid is a solvent which would not have again been separated.

Bergman has indeed assured us, that he found hard crystals having the form of rock crystal, in the fluat of flint; but as he did not carefully examine them, we may still question whether they were true rock crystal.

Achard likewise attempted to imitate nature, and endeavoured, as is well known, to compose even the precious stones, by means of carbonic acid; but it is also known that his notions were not confirmed, and that this celebrated man either deceived himself, or was deceived by others; for his excellent character absolutely forbids the suspicion of voluntary deception.

It is known that the alkalis are capable of dissolving flint; it is also known that the acids only are capable of destroying this combination. The flint in that case, when the solution is weak, falls down. The precipitate is very fine and transparent. When the solution is more saturated, it assumes the form of a jelly, or appears like gelatinous condensed flakes, but never in crystals.

Professer Siegling the elder, my intimate friend, had prepared the liquor of flints after the ordinary process, which he set aside in a glass vessel covered with paper. This fluid remained undisturbed for eight years, when by accident his attention was directed to it, and he remarked a beautiful crystallization. On this occasion he had the goodness to send me the vessel.

It was a jar of greenish glass, capable of holding twelve ounces. The bottom was covered with a number of small crystals, over which reposed about two ounces of a very transparent fluid. This liquid was covered with a crystalline crust, so strong that the vessel could be inverted without any loss of the fluid.

The jar was then broken, and the contents more particularly examined.

The crystals at the bottom were composed of sulphate of pot-ash, and caustic pot-ash, in a crystallized state. The fluid exhibited the appearance of a caustic lixivium; but which had attracted a small quantity of carbonic acid, and still contained a little flint. Hence

it effervesced, and deposited silex when treated with acids. The crust was visibly composed of two kinds of crystals. When boiled with water, a great part of these were dissolved, and afforded, after the evaporation, carbonate of pot-ash. The remaining crystals were tetrahedral pyramids in groups. Acids had no action upon them. *They were perfectly transparent, and so hard as to give fire with steel.* They dissolved in four parts of deliquescent pot-ash, and afforded a mass of soluble glass, from which silex could be precipitated. The fluoric acid attacked them, and dissolved a considerable part. They were therefore perfect rock crystals, produced by art. Part of the crust, which was less transparent, fell down during the boiling. It was siliceous earth, so hard and firm that it acted as a grinding powder on glass.

Though I am convinced that this observation affords us no light with regard to the process of nature in the production of rock crystal, it has not given me less satisfaction, from the proof that it is possible at least for us to imitate this natural production, though perhaps by different means.

I explain the origin of rock crystal in this glass nearly in the following manner: The liquor of flints was very weak, and super-saturated with alkali. This disengaged alkali insensibly attracted the carbonic acid from the atmosphere, and formed in this manner a crust upon the liquor, which was at rest, and was not therefore so much exposed to the action of the carbonic acid. The alkali could not attract this last principle but with difficulty, and consequently the silex was separated very slowly in the form of a fine powder. Now as the most perfect repose obtained in this operation, the fine particles of the silex were not disturbed in their attraction, which they exerted without impediment, and formed crystals. As soon as a double crust was formed of carbonate of pot-ash and rock crystal, the carbonic acid could no longer penetrate to the liquid; whence followed a crystallization of part of the disengaged alkali.

An admirer of hypotheses might apply this observation to the formation of natural rock crystal. It would be sufficient for this purpose to suppose that a soluble mass of silex and soda was formed by any subterraneous revolution; that, by the presence of moisture, this mass was converted into the liquor of flints, from which afterwards, by a slow attraction of carbonic acid, the rock crystal was deposited.

But it will reasonably be asked, What has become of the soda in this operation? Perhaps it may have been neutralised in the course of time by other acids, or else (for it is easy to find support for an hypothesis) it may be changed into magnesia; and in this case the experiments of Osburg and some other authors would prevent themselves to be quoted. For my part, I prefer an avowal of my ignorance, and am disposed to say with a certain poet, that "no mortal can penetrate the depths of nature, and few even the surface." I am nevertheless desirous that this observation may engage other chemists to repeat the experiment, and examine more particularly into the facts. Circumstances, perhaps, in part to me unknown, may have a marked influence on the success of the experiment.

VI.

Geological Observations on North Wales *. By Mr. ARTHUR AIKIN.

I. THERE are no proper volcanic productions to be met with in North Wales. By proper volcanic substances I mean ashes, pumice, lava, and scoriæ, or semi-vitrified stones, such as are the peculiar products of Etna and other acknowledged volcanos. A variety of porous stones may be found on Cader Idris, and Snowdon; and these have been mistaken for cellular lava; they consist, however, merely of decomposed granite, porphyry, or toad-stone. Fragments of this last, indeed, I have found in the plain of Salop, so porous, and penetrated with carbonate of iron, as greatly to resemble a slag.

II. The indefatigable Saussure, whose accurate researches into the position and nature of the Alps, and the other surrounding mountains, have deservedly ranked him among the most illustrious and persevering mineralogists, says, in the first volume of his *Voyages dans les Alpes*, "It is a general observation, with few exceptions, that, in the greater chains of mountains, the exterior ridges are of lime; the next contain slates; to these succeed the primitive stratified rocks, and then the granites." The relative position of the Welsh mountains tends to confirm a remark made among the Swiss Alps. For if from the central ridge of the Snowdon chain (in which term I comprehend the whole mountainous extent of Caernarvonshire from north to south) we proceed to the Menai, it will be found that the primitive rocks in mass, such as the granites and porphyries, occupy the interior and higher peaks; to the side of these are applied the banks of primitive stratified rocks; then come the slates, which terminate in the lime-stone, which forms the bank of the Menai. The same gradation of strata will appear, if, instead of the western, we examine the eastern side of Snowdon; the variation is not indeed so sudden, but perhaps on that very account is more interesting, as the species and varieties of rocks are more numerous, and in larger masses. From the peak of Snowdon to Llanrwst through Capel Cerig are found granite and porphyry in mass, micaceous schistus, and other primitive stratified rocks; serpentine in large blocks and of extraordinary beauty, and hornblen slate mixed with veins and rocks of quartz: from the vale of Llanrwst to Llangollen extend the slates, which are there circumscribed by the lime-stone range already mentioned. The general disposition of the mountains of North Wales may be described in a very few words. There are two ridges of primitive mountains extending nearly due north and south, of which one is the Snowdon chain, and the other the Cader Idris chain (comprehending, besides this mountain, the Arrans and other lofty peaks that overlook the southern extremity of Bala-Pool). Owing to the near approach of the primitive and secondary mountains to the coast of Merioneth, the lime does not commence till near the port of Crickath; hence proceeding northwards in an interrupted line

* From "A Journal of a Tour through North Wales and Part of Shropshire; with Observations in Mineralogy and other Parts of Natural History." By Arthur Aikin. Small octavo, 231 pages. Printed for Johnson, London, 1797.

This Journal is of much value for the interesting scientific observations it contains, as well as for authentic accounts of the manufactories and mine-works in the district through which the tour was made. Its plan in some respect resembles that of the celebrated De Saussure, like whom our author has given some animated sketches of the grand romantic scenery of a mountain country. N.

along the shore, it arrives at Caernarvon. From this place it proceeds along the Menai, forming the eastern bank, as far as Bangor Ferry. Hence to Orme's-head it is cut off by the northern extremity of the Snowdon chain, which terminates in the bay of Conway, by the cliffs of Penmaen-mawr and Penmaen-bach. The lime recommencing in the lofty promontory of Orme's-head, continues the boundary of the coast as far as the mouth of the Dee; it then takes a westerly direction, curving to the south, as it passes by Holywell and the upper end of the vale of Clwyd to the Eglwyfyr rocks in Llangollen vale; then, passing due south, it appears on the opposite side of the vale; is broken near Oswestry by the Ferwyn mountains, appears again at Llanymynech, and is at length stopped in its course by a line of primitive mountains stretching northwards out of Radnorshire. The slates occupy the whole intermediate space between the ridge of lime and the primitive mountains.

The primitive, secondary, and derivative mountains may in general be distinguished by peculiarities in their form, as well as by their relative position; the primitive rocks are craggy, steep, and tending more or less to a peak, or slender-pointed summit; the loftiest mountains are generally about the middle of the chain, which both commences and terminates in abrupt precipices: these, together with the insulated peaks that are continually interrupting the outline of the chain, form a very striking distinctive character. The plates to Mr. Pennant's *Snowdonia* will convey a clearer idea of this than the most laboured description. Indeed it is but justice to observe, with respect to these engravings, that they are perfectly accurate representations of the scenes which they profess to describe.

The slates are distinguishable from the primitive mountains by their inferior height, by the evenness and almost squareness of the individual hills, and by the easy flowing though varied outline of the chains, such as that of the Ferwyn mountains already described, Chap. III.

The lime and sand-stone hills are considerably lower even than the slates; rising in general very gradually at one extremity, and terminating abruptly at the other. The banks of sand rock are however broader and rounder than the lime: where the lime is the hardest, its form is the most perfect; but as it becomes slaty, soft, and mixed with clay, it approaches nearly to the form of the slate-hills, as is remarkably the case in the southern part of Wenlock-edge. The sand-stone, too, where it contains but little iron and clay, being almost wholly composed of sand and lime, resembles most the limestone hills. This may be observed by comparing the difference of form between the red sand rocks of Nefcliff, and the white freestone of Grinshill.

III. I have already mentioned the beds of rounded pebbles that are to be found on the highest parts of the slate mountains. Their present situation could never have been that in which they were formed, for they consist almost universally of porphyry, quartz, serpentine and other stony substances which lie in large masses, composing the primitive mountains: their rounded shape too, like that of the pebbles on the sea shore, seems to intimate that they have been carried by the force of water to the places which they now occupy. Another circumstance which appears to point out the quarter whence they originally proceeded is, that, in proportion to their vicinity to the primitive mountains, is their size: a circumstance that might naturally be expected, since the further they were carried, the more would they be rounded and comminuted. Still, however, there is a difficulty attending this hypothesis; namely, by what means could these rounded pebbles have been forced

forced across the many deep valleys that intersect the mountains in all directions, without first filling up the valleys themselves? And, if this was the case, by what means were the valleys so entirely cleared of them afterwards, as they appear now for the most part to be? The difficulty, however, I think, is more apparent than real; for it seems highly probable, that at the time when these slate mountains were formed under the water, there were no valleys, but the whole mass was one uniform bank, the valleys being afterwards formed by the rivers as the water subsided: by this not improbable supposition, as it appears to me, the objection is wholly removed. On descending from the slate rocks to the limestone and the derivative hills, the marks of submerision are more numerous and unequivocal; sand, pebbles, shells, and other marine exuvie, being found in considerable abundance. Following the example of most mineralogists, I might indeed have mentioned the existence of lime as of itself a sufficient proof of submerision: it appears to me, however, that there is by no means evidence sufficient to support the large assertion, that all the lime which forms so considerable a part of the surface of the earth, has been actually produced by the process of animalization. The only fact that I recollect, which has any reference to this question, would lead one to draw a directly contrary inference; it being well known that the eggs of hens are without shells, when care has been taken in the feeding of the bird to hinder its having access to mortar or any other substance that contains a large quantity of calcareous matter. These tokens of the presence of water on the tops of mountains that are now 2000 feet above the level of the sea, naturally lead me to enquire into the cause of this phenomenon, in explanation of which two hypotheses have been started; one, that the continents were forcibly elevated to their present height above the sea by successive explosions, as some of the Lipari islands are known to have been formed; the other, that the sea has gradually, or suddenly, subsided to its present level, but that no alteration has taken place in the position of the mountains. The extreme scarcity of acknowledged volcanic productions seems to render the first supposition highly improbable; and it may also be objected to it, that an immense power, to which we see at present scarcely any thing analogous, is called into existence to accomplish that which the operation of common allowed causes would effect just as well. The grand difficulty is to account for the disappearing of so great a quantity of water, nor indeed have I ever seen this explained in a tolerably satisfactory manner: it is easy to imagine vast chasms in the earth into which the waters have retired; but of this there is no proof whatever: it is also contrary to the gradual decrease of the sea, which, from the present appearance of the earth, and from historical records, appears probable.

IV. The Welsh primitive mountains in mass contain no metals; copper, however, is found in several of the hornstone stratified mountains, of which the Parys mine, and those at Llanberis and Pont-aber-glaslyn are examples. In these mines the ore is for the most part yellow sulphuret of copper: the green and blue malachites, or carbonates of copper, are found in limestone, as at Orme's-head and Llanymynech-hill; nor have I heard any instance of these two last mines furnishing copper in any state but that of carbonate. Carbonated copper is also found in the calcareous cement of sand rocks, as has been already mentioned to be the case at Pym-hill and Hawkstone in the plain of Shrewsbury. Lead and calamine, I believe, are not to be found in North Wales, in any of the primitive stratified rocks. These metals are most frequently found in slate, with a matrix however of calcareous spar,

spar, as in the vale of Conway, at Llangynnog, and the Snailbeach mines; they are frequently also found in limestone as at Llanymynech and Holywell. Respecting the formation of the above-mentioned metals, it is not easy to give a tolerably probable opinion; it appears, however, that carbonate of copper is of considerably later formation than the sulphuret, the former probably originating from the decomposition of the latter, and deriving its acid from the carbonate of lime, in which it is found. It is not likely that the lead found in limestone was originally formed elsewhere, because lead, even in slate rocks, lies in a matrix of calcareous spar; and especially because it does not form thin strata between the strata of lime, as is the case with copper, but it traverses, in a stream, the several strata without any alteration in the line of its direction; to which may be added, that sulphuret of lead is the general slate in which the metal is found, both in the slate rocks and limestone, the carbonate being equally rare in both situations.

There is no coal found in North Wales between the primitive mountains and the slates: a very small quantity is procured between the slates and limestone; but by far the most extensive beds are between the limestone and the sand rocks, as about Wrexham or Coalbrook Dale, or between these last and the alluvial hills, as round Wolverhampton.

V. From the above-mentioned circumstances it would appear, that at some former period the sea covered the whole of North Wales, of the present plain of Salop, and of Cheshire, except a line of islands consisting of the Snowdon chain, another to the south consisting of the present Cader Idris chain, and a few detached rocks, several leagues to the east, which now form the tops of the Wreakin, Caer Caradoc, and Stiperstones. Under this primitive sea, and prior to the existence of animals or vegetables, the vast banks of slate appear to have been formed. My reasons for thinking that animals and vegetables had as yet no existence are, because there was no soil upon these hard insulated rocks for the growth of plants upon which the animals might feed, and because we meet with no impressions or remains of organized bodies in the primitive mountains or banks of slate. By slow degrees the water subsided; being possibly in part absorbed by the earth, in part fixed in a solid form in the slates, and in part decomposed, forming oxygene and hydrogen: the former of which constituted the oxygenous base of the lower atmosphere; the latter, by its superior levity, rising above the atmospherical air, according to the opinion of some philosophers, forms a vast stratum many miles above the surface of the earth; whence originate meteors, the aurora borealis, and other similar appearances.

The water having for the most part retired from the beds of slate, the greater part of Wales, and the secondary hills of the English counties west of the Severn, would form one or more considerable islands, separated from the small part of England then above water by a wide channel occupying the flat part of Cheshire and Shropshire, and the present vale of the Severn from Coalbrook Dale to the Bristol Channel. At this period I imagine the secondary limestone hills to have been formed. The desiccation of the water still continuing, the tops of the limestone ridges themselves would begin to appear above the surface; and then the plain of Salop, the flat part of Cheshire, and the southern extremity of Lancashire, would form one vast bay; into which the Severn, Dee, and Mersey emptied themselves, flowing into the sea by an united stream, filling the present mouths of the two latter rivers, and the intermediate space, the hundred of Wirral. Into this bay or estuary a large quantity of sand would be constantly poured in by the violent western winds, and
the

the currents of the three rivers not being able entirely to clear it away, banks of sand would be by degrees formed, constituting the present sand and free-stone rocks extending from Nefeliff eastward by Pym-hill and Grinshill to the hills of argillaceous schistus round the Wreakin. This accumulation of sand would prevent the free egress of the waters of the Severn into the main bay, which by degrees, or more probably at once, urged by a strong west wind, and swelled unusually by rains or snow, broke through the lime-stone rock at Coalbrook Dale, and rushed into the Channel, which it has ever since flowed in. The banks of sand, that almost entirely shut out the Severn from the bay of Cheshire, prevented the Dee from deviating from its original course, and the further decrease of the water added constantly to the difficulty. A number of particulars might be mentioned in confirmation of the foregoing hypothesis, derived from the appearance of the hills, the soil, and other circumstances, were it my intention to enter minutely into the subject; but this could not be done without the aid of charts and engravings, in a manner capable of interesting the attention of any, except those who have visited and carefully observed the tract in question.

The sea, however, has not been uniformly receding; for some time past it appears to have been advancing upon the Welsh coast: a brief enumeration of the proofs of this will conclude the subject.

The coast of Cardigan from Aberystwith northward, if it does not furnish any direct proof of the advance of the sea, yet shews at least that the water is not retreating, from the circumstance of there being no beach at high tide, and the many caverns and recesses in the slate rocks on the coast that are every day filled by the sea. The southern part of Merionethshire exhibits certain proofs of the progressive state of the sea, in the vast banks of peat already mentioned, which extend along the shore to Tovyn, and stretch to an unknown distance into the water. From near Harlech a long range of sand and gravel, including Traeth-mawr and Traeth-bychan, runs twenty-two miles into the sea, being called at present Sarn Badrig, or the Ship-breaking Causeway; the whole of which tract, formerly called Cantrer Gwaelod, or the Lowland Hundred, was about the year 500 overwhelmed by an inundation, occasioned by the carelessness of those who kept the flood-gates, as is mentioned in an extant poem of Taliesin. Northward of the town of Abergeley in Denbighshire, a vast extent of inhabited country is said to have been destroyed by the sea; in proof of which an epitaph without date or name in Abergeley church-yard is cited, signifying that the person to whose memory the monument was erected lived three miles to the north. A more decisive evidence is furnished by Mr. Pennant in his *Snowdonia*. "I have observed," says he, "at low water, far from the clayey banks, a long tract of hard loam, filled with the bodies of oak trees, tolerably entire, but so soft as to be cut with a knife as easily as wax." Finally, I have observed on the Lancashire coast, a few miles north of Liverpool, the beach overspread with trunks and branches of oak-trees; the whole shore to a considerable distance inland, being a peat moss, now for the most part covered with sand; the extent of the moss along the shore is very evident by the almost blood-colour of the beach, occasioned by the boggy iron ore with which the water that oozes out of the peat is highly impregnated. From these facts it may, I think, be fairly inferred, that most of the present sands which border the coast of North Wales and Lancashire, were formerly forests or cultivated land; and that the sea is at present, and for these twelve or thirteen centuries past has been, gaining upon the shore.

VII.

An Account of the Fata Morgana; or the Optical Appearance of Figures, in the Sea and the Air, in the Faro of Messina. With an Engraving.

As when a shepherd of the Hebrid' Isles
Placed far amid the melancholy main,
(Whether it be lone fancy him beguiles,
Or that ærial beings sometimes deign
To stand, embodied, to our senses plain)
Sees on the naked hill, or valley low,
The whilst in ocean Phœbus dips his wain,
A vast assembly moving to and fro:
Then all at once in air dissolves the wondrous show.

THOMSON.

VARIOUS philosophical writers and travellers, and among them our English travellers Brydone and Swinburne, make mention of a very striking phenomenon which occasionally appears in the Straits of Messina, and is known by the name of Fata Morgana, or, as some render it, the Castles of the Fairy Morgana. The accounts differ from each other, as well with respect to the appearances, as the concomitant circumstances which are supposed to be necessary for producing them. How far the effects themselves may be subject to variation, or to what extent the imagination of the narrators, who speak of the exhibition as calculated to produce astonishment, may be subject to irregularity, would admit of discussion; but the general certainty of the events is matter of universal notoriety, and admits of no doubt. I have not had the good fortune to meet with any of the authors who treat on this subject expressly from their own knowledge and observation, till lately that the Dissertation of Minasi* was lent me by the Right Honourable Sir Joseph Banks, Bart. &c. In this treatise the facts are related with much simplicity and precision, and the philosophical reasoning of the author is kept distinct from the narrative. I have therefore chosen to collect the present account from this author. The engraving, plate X. is copied from the same work.

His first chapter contains a description of the phenomenon. "When the rising sun shines from that point whence its incident ray forms an angle of about forty-five degrees on the sea of Reggio, and the bright surface of the water in the bay is not disturbed either by the wind or the current, the spectator being placed on an eminence of the city, with his back to the sun and his face to the sea;—on a sudden there appear in the water, as in a catoptric theatre, various multiplied objects, that is to say, numberless series of pilasters, arches, castles well delineated, regular columns, lofty towers, superb palaces, with balconies and windows, extended alleys of trees, delightful plains with herds and flocks, armies of men on foot and horseback, and many other strange images, in their natural colours and proper actions, passing rapidly in succession along the surface of the sea during the whole of the short period of time while the above-mentioned causes remain.

* Dissertazione prima sopra un Fenomeno volgarmente detto Fata Morgana: O sia apparizione di varie, e successive, lizzarre immagini che per lungo tempo ha sedotti i popoli, e dato a pensare ai dotti. A sua Eminenza il signor Cardinale de Zelada. Del P. Antonio Minasi Domenicano. In Roma 1773.

"But if, in addition to the circumstances before described, the atmosphere be highly impregnated with vapour, and dense exhalations not previously dispersed by the action of the wind or waves, or rarefied by the sun, it then happens that in this vapour, as in a curtain extended along the channel to the height of about thirty palms, and nearly down to the sea, the observer will behold the scene of the same objects not only reflected from the surface of the sea, but likewise in the air, though not so distinct or well defined as the former objects from the sea.

"Lastly, if the air be slightly hazy and opaque, and at the same time dewy and adapted to form the iris, then the above-mentioned objects will appear only at the surface of the sea, as in the first case, but all vividly coloured or fringed with red, green, blue, and other prismatic colours."

The author therefore distinguishes three sorts of Fata Morgana: that is to say, the first at the surface of the sea, which he calls the Marine Morgana; the second in the air, called the Aërial Morgana; and the third only at the surface of the sea, which he calls the Morgana fringed with prismatic colours.

In a note in this chapter P. Minasi enquires into the etymology of Morgana. After various remarks, he thinks the opinion of those who derive this word, which is so foreign to the Roman idiom, from *μῆγος* tristis and *γάρβος* letitia afficio, is not far from the truth, considering the great exultation and joy this appearance produces in all ranks of people, who on its first commencement run hastily to the sea, exclaiming Morgana, Morgana! He remarks that he has himself seen this appearance three times, and that he would rather behold it again than the most superb theatrical exhibition in the world.

In the second chapter the author describes the city of Reggio, and the neighbouring coast of Calabria; by which he shews that all the objects which are exhibited in the Fata Morgana are derived from objects on shore.

In his third chapter, consisting of physical and astronomical observations, he affirms that the sea in the Straits of Messina has the appearance of a large inclined speculum; that in the alternate current or tide which flows and returns in the Straits for six hours each way, and is constantly attended by an opposite current along shore to the medium distance of about a mile and a half, there are many eddies and irregularities at the time of its change of direction; and that the Morgana usually appears at this period. Whence he enters into considerations of the relative situations of the sun and moon, which are necessary to afford high water at the proper time after sun-rise, as before described. It is high water, that is to say, the northern current ceases, at full and change, at nine o'clock. There is probably a small rise and fall, though the annotation to a large chart before me affirms that there is none.

In the fourth chapter and subsequent part of the work, the author collects the opinion and relations of various writers on this subject, namely, Angelucci, Kircher, Scotus, and others; and he afterwards proceeds to account for the effects, by the supposed inclination of the surface of the sea, and its subdivision into different plains by the contrary eddies. The aërial effects are referred to considerations of saline and other effluvia suspended in the air, which I forbear to abridge, because it seems difficult to make any clear or productive statement either from the narrative or the reasoning.

What I seem to collect upon the whole from the several relations, and the curious print
which

which I have copied, is: 1. That by the situation of the Faro of Messina, the current from the south, at the expiration of which this phenomenon is most likely to appear, is so far impeded by the figure of the land, that a considerable portion of the water returns along shore. 2. That it is probable the same coasts may have a tendency to modify the lower portion of the air in a similar manner, during the southern breeze; or, in other words, that a sort of basin is formed by the land, in which the lower air is more disposed to become motionless and calm than elsewhere. 3. That the Morgana Marina presents inverted images below the real objects, which are multiplied laterally as well as vertically; and that there are repetitions of the same multiplied objects at more considerable vertical intervals. This I gather from the appearance of the dome and other objects in the plate. 4. That the Aërial Morgana is not inverted, but, as I am disposed to conjecture, is more elevated than the original objects. 5. That the fringes of prismatic colours are produced in falling vapours, similar to many appearances which have been described by authors, but not accurately explained by the general principles of refraction through spheres of water. The ship is referred to by the author as an object surrounded by these fringes: whence it appears that the colours apply to the direct rays from objects, as well as to those of the Marine Morgana. 6. Various other objects in the drawing, as well as in the description, afford matter for question and conjecture, but none perhaps which it may be proper to enlarge upon, until the theory be better known. 7. It seems at all events more probable that these appearances are produced by a calm sea, and one or more strata of superincumbent air differing in refractive, and consequently reflective power, than from any considerable change in the surface of the water, with the laws of which we are much better acquainted than with those of the atmosphere. 8. By attentive reflection upon the facts and reasonings in Mr. Huddart's paper *, we may form a theory to account for the erect and inverted images: the polished surface of the sea may perhaps account for the vertical repetition; but for the lateral multiplication we must have recourse to reflecting or refracting planes in the vapour, which appear nearly as difficult to deduce or establish, as those which have been supposed on the water.

VIII.

A Memoir containing some Results arising from the Action of Cold on the Volatile Oils, and an Examination of the Concretions found in several of these Oils. By Cit. MARGUERON, Member of the Société des Pharmaciens at Paris.

[Concluded from page 186.]

An Examination of the Concretions found in several Volatile Oils.

THE volatile oil of fennel-seed, which had been long made, had deposited a concretion of a lamellated form. The oil was of an amber-colour, and smell scarcely perceptible. It was fluid, and no longer capable of assuming the concrete state at the temperature of four or five degrees below congelation. The concrete matter being separated was yellowish, and had the taste and smell of fennel. When exposed to the air it became dry and friable

* Philosophical Journal, I. 145.

between the fingers, like a resin. When placed between two capsules of glass, and exposed to the hot wood-ashes, it melted and sublimed in needles. On suspending the operation there remained in the capsule a brown matter, which by cooling crystallized in a striated mass. The needles obtained by sublimation were white and silky, with the taste and smell of fennel. On exposure to the flame of a candle they liquefied without taking fire, and by cooling they assumed a concrete transparent state. They were insoluble in the nitric acid; but their solution in alcohol reddened the tincture of tournsole; and when submitted to evaporation it left a crystallized matter in silky needles.

Volatile oil of wormwood, not rectified, and preserved several years in a bottle with the distilled water of the same plant, had deposited a yellowish matter; the oil had little fluidity; its colour was of a deep green, and its smell considerably pungent; the deposited matter was yellowish, and emitted the smell of wormwood; when applied to the tongue it communicated the peculiar taste of the plant; it adhered to the fingers, like turpentine; exposure for some days to the air did not augment its consistence; when it was afterwards presented to the flame of a candle, it took fire, and was covered with small needles. This matter crystallized by cooling, and became dry and brittle, like resin.

The water on which the oil had been preserved very speedily reddened the tincture of tournsole.

The volatile oil of sage, distilled long before, had deposited a concretion which lined the bottom of the bottle in which it was preserved. This concretion was white, and when examined by the magnifier appeared brilliant, crystallized, and laminated. Before I proceeded to examination I compressed it between several folds of the paper Joseph, to deprive it of the oil which might be interposed between its parts. This concretion, exposed to the air, became dry and friable; its solution in alcohol became white by the addition of water. Part of the solution, subjected to spontaneous evaporation, left in the capsule a whitish covering, and some small needles. Exposed to the flame of a candle it did not take fire, but liquefied, and became firm by cooling, and of a resinous appearance. When placed between two capsules, and subjected to a gentle heat, it melted, but only a few particles sublimed. When it was treated comparatively with the camphor of the shops by the nitric acid, and with the same degree of heat, the concretion was liquefied, assumed the consistence of turpentine, and became much coloured: the camphor, on the contrary, entirely dissolved in the nitric acid, and was recovered again with its properties by the addition of water. The nitric acid which I heated with the concretion, did not afford the same result when treated with a like quantity of water.

From these experiments we may conclude, that the concretions observed in the several volatile oils, approach rather to the nature of resins with a superabundance of acid, which form a kind of salt similar to the flowers of benzoin* rather than camphor, to which they

* In 1792, Citizens Deyeux and Vauquelin informed us, that the concretions deposited by cinnamon-water had the properties of the acid of benzoin. M.

That the concretions were not camphor, is no doubt ascertained by the different action of nitrous acid; but how far they may agree with the acid of benzoin, seems to require further proof. It is generally agreed, from the experiments of Lichtenstein and others, that the acid of benzoin is soluble in the vitriolic and nitrous acid, and separable by the addition of water, without alteration of its properties, and that it is not readily altered by digestion or abstraction of the latter acid. N.

have

have frequently been compared. For, if we compare the effects of the nitric acid on these concretions, the foregoing observations shew that its action upon them is very different from its action upon camphor.

IX.

On the Cold Winds which issue out of the Earth. By Professor DE SAUSSURE and others; with Observations.

IN the fifth volume of De Saussure's *Voyages dans les Alpes*, page 342, lately published, the author, after having ascertained by experiment that the water at the bottom of the Alpine Lakes is much colder than temperate, namely, about 4 degrees of Reaumur, while that of the surface indicated about 15°, whereas the temperature of the water at 886 feet and at 1800 feet depth in the Gulf of Genoa, near the land, proved to be above temperate, namely, 10, 6 degrees, while at the surface the heat was 16½ degrees, proceeds to make remarks on a phenomenon probably of the same nature, that there are many subterraneous cavities out of which winds issue, which are colder than the mean temperature of the earth. The former effects are considered by this author as very difficult to be accounted for; and indeed they appear to depend on the principle of the almost perfect non-conducting power of fluids with regard to heat, which has scarcely, if at all, been treated by any author but Count Rumford in his seventh essay, published a few days ago. The latter he thinks may more easily be explained. I shall here relate the facts by a translation nearly close, and afterwards give the substance of his reasonings, with such remarks as may present themselves.

He begins his account with the caves of Mont Testaceo near Rome, which were the first that fixed the attention of an accurate observer. The Abbé Nollet observed them in his travels into Italy*, and found their temperature was 9½ degrees on the 9th of September 1749, in the afternoon, while the thermometer in the open air stood at 18 degrees; and he remarks with reason, that their coolness is so much the more astonishing, as they are not deep; that the entrance has scarcely any descent, and that the sun shines for the greatest part of the day upon the door of the entrance.

M. De Saussure found them cooler than the Abbé Nollet did, though he visited them in hotter weather; the reason of which, as he remarks, may be deduced from the explanation of the phenomenon. On the first of July 1773, the external air in the shade was at 20½ degrees; that of one of the caves was at 8; of another at 5½; and of a third at 5¼. These caves are made in the side of the mountain, and occupy nearly its whole circumference. Those he entered were on the west side. The walls at the bottom have perforations through which the cold air enters.

The air itself comes through the interstices left between the pieces of broken urns, amphoræ, and other vessels of pottery of which this small mountain appears to be entirely composed. He went to its summit, which is not above two or three hundred feet in

* Acad. des Sciences, 1749, page 486.

height, and every where observed these fragments, which he does not doubt were formerly collected in this place by some public order. At present the police maintains them; for these caves are so useful and valuable, and there is so much apprehension lest their quality should be altered, that every excavation, and even the cultivation of the ground on this small mountain, is prohibited. And it is truly a very singular phenomenon, that in the midst of this district, of which the air is so hot and stifling in summer, there should be found a small isolated hill, from the base of which currents of air of extraordinary coolness should issue on all sides.

It is not less singular, that under a still more southern climate, and in an island like that of Ischia, which is entirely volcanic, and every where abounding with hot springs, a cold subterraneous wind should be found like that here described. Sir William Hamilton assured our author, that there is a similar grotto at Ottiano, at the foot of Vesuvius. These grottos have even an appropriate name; they are called Ventaroles. That of Ischia is called Ventarola della Funera. It is beneath a small chapel dedicated to St. Anthony, which is itself beneath the Casa Monella. On the 9th of March 1773, the thermometer in the open air in the shade out of the grotto stood at 14 degrees, and that which M. De Saussure placed at the bottom of the grotto stood at 6 degrees; and he was assured that in the summer, during the great heats, he would have found it much lower.

The cold caves of St. Marino are at the foot of a hill of grit-stone, on which the capital of this small republic is built. On the 9th of July 1773, about three in the afternoon, the thermometer in the open air stood at 13 degrees; and in the caves at 6. The floor of these caves lies between 320 and 330 toises above the level of the sea.

The caves of Cesi are situated in the town of the same name, which is six miles north of Terni, in the Ecclesiastical State. That inspected by M. De Saussure was in the house of Don Giuseppe Cesi. The cold of this cave, like the preceding caves, does not proceed from its depth, but from a cold air which issues from the crevices of a rock against which it is built. This air issued out with so much force at that time, that it almost extinguished the flambeaux which enlightened them; and the proprietor assured our author, that the wind would have been much stronger, if the weather had not been cold, as it was for the season. In the winter, on the contrary, the wind rushes violently in, and the more so the colder the weather. This is expressed in the Latin verses which the master of the house shewed him:

Abditus hic ludit vario discrimine ventus,
Et faciles miros exhibet aura jocos:
Nam si bruma riget, quaecumque objeceris haurit;
Evomit aëstivo cum calet igne dies, &c.

The master of this house derives great advantage from the coolness of this cave, not only by preserving wines, fruits, and provisions of every kind, but likewise by conducting this cool air by pipes into the apartments. Cocks, at the extremity of these tubes, emit the cold air in such quantities as may be desired. This refinement is carried so far as to convey the air under certain stands, the foot of which is pierced; so that bottles placed on these stands are continually cooled by the wind which issues forth. On the day when M. De Saussure measured the temperature of this subterraneous wind at the entrance of the

small cavern whence it issued, he found it at $5\frac{3}{4}$ degrees, while the external air was at $14\frac{1}{2}$ degrees. This was on the 4th of July 1773, in the afternoon; whence, in fact, it appears that the day was very cold for that climate and season.

The Cantines, as they are called in Italian Switzerland, or the cold caves of Chiavenna, are likewise situated against a rock to the south-east of the town. The cold air enters into the caves through the crevices of this rock, of which the composition is an indurated steatites, intermixed in various places with asbestos and flexible amianthus. On the 5th of August 1777, at noon, the thermometer stood in these caves at 6 degrees, while in the open air it was at 17.

On this occasion our author remarks, that the stones which compose the mountains whence the cold winds issue, are very different in their nature; which, as he observes, affords an answer to the question of the Abbé Nollet, relative to the caves of Mont Testaceo, "Is pottery of a nature to be more difficultly heated than other materials; or do the influences of the atmosphere cause refrigeration, which would not take place elsewhere?" It is certain, adds M. De Saussure, that this phenomenon does not depend on the nature of the pottery; for the cold winds of Cesi issue from a calcareous mountain, those of St. Marino from grit-stone, those of Chiavenna from a steatites, &c.

The caves in which M. De Saussure found the air coldest, and which he most carefully observed, were those of Caprino, near the lake of Lugan, and opposite that agreeable small town of Italian Switzerland. These caves are situated at the foot of a calcareous mountain, whose very rapid slope terminates near the lake.

Before your entrance, you are desired to remark the cold wind which issues from the key-hole of the door, which is sensible even at seven or eight inches distance. On your entrance, the sensation of cold is very striking, so as even to produce an apprehension of inconvenience; and on coming out, you seem to enter an oven. In the first visit to these caves (June 29, 1771), the author found the thermometer at the bottom of the cave to be $2\frac{3}{4}$ degrees; while in the shade in the open air it was at 21. The second time that he visited them, namely, on the 1st of August 1777, the thermometer descended no lower than $4\frac{1}{2}$ degrees; while in the air it stood at 18.

What he thinks remarkable in these caves is, that they are not deep, nor hollowed into the earth. Their floor is on a level with the ground; the external wall and the roof are entirely exposed to the open air. There is only the wall at the bottom, and part of the lateral walls, which are within the foot of the mountain. This foot is every where covered with angular fragments of the same mountain, and it is from among these fragments that the cool air issues. By a fortunate chance, he saw one of these caves constructed. The mason who overlooked the work, affirmed that he was in possession of the art of finding proper situations; and that it was necessary to seek for those places whence the wind issued out, and to bore apertures corresponding to those places. It is by these ventiges that the caves are cooled, as may be easily perceived by applying the hand; and against these it is that the thermometer must be placed to find the lowest temperature.

It is asserted that the country is indebted to the sheep for this discovery. A shepherd observed, that during the great heats his sheep all repaired to certain places in preference, and applied their noses to the earth. He endeavoured to ascertain the reason of this preference,

preference, by applying his hand to the ground. The cool air, which, even without any construction, is perceptible, induced him to build a cave.

In the cave which M. De Saussure saw built, there was only the interior wall raised, so that its external face was, at that time, absolutely in the open air; nevertheless his thermometer, placed at the entrance of these ventiges, stood at 4 degrees. He plunged his thermometer to the depth of 8 inches in the ground of this open cave; it then stood at 7 degrees; and at 8, when merely laid on the ground; but on the floor of a close cave it stood at 5. The thermometer, as already remarked, stood at 18 degrees in the open air.

This cold air has no sensible quality different from pure atmospheric air cooled to the same degree. It has neither smell nor taste; but, as the author observes, it would certainly be curious to analyse it.

The constructor of these caves, who appeared to be an intelligent man, expressed his belief to M. De Saussure, that the cold air comes from the inside of the mountain, and issues from clefts concealed beneath the rubbish: but that nevertheless there was no knowledge existing of any cavern or natural repository of ice in this mountain, in which the snows might accumulate during the winter. The mountain is not high enough to preserve visible snow during the summer. It is necessary however, according to M. De Saussure, that the cause of this phenomenon should be very extended; for he was certainly informed that these caves exist as far as Capo di Lago, at the distance of eight miles from Caprino, and even at Mendrisio, which is a league farther off. There are some also on the opposite shore of the lake. It is affirmed that there are several on the banks of the lake of Como; which M. De Saussure more readily believes, because he found the water of the intermitting spring at the Villa of Pliny, situated on the banks of the lake, to be as cold as 7½ degrees.

The last caves described by De Saussure, from whence cool streams of air are emitted, are those of Hergisweil. These were the only ones he saw on that side of the Alps. At Winckel, a village at the distance of one league from Lucerne, he embarked on the lake; and in less than half an hour he was opposite Hergisweil, a village belonging to the canton of Underwald, situated at the bottom of a small bay, and surrounded with meadows and vineyards, in a position highly rural and romantic. At the distance of ten minutes from the village, at the foot of the mountain, are the cold caves, which are nothing but huts of wood except the bottom wall. This wall, like those at Lugan, is applied to the accumulated fragments at the foot of the mountain. The stones of the wall are not bound with mortar; and it is through the interstices between them that the cold wind enters, which issues from the fragments of the mountain.

On the 31st of July 1783, at noon, the thermometer stood at 18,3° in the shade, and 3,3° at the bottom of the cave. The master of the house assured him that milk could be kept in the cave for three weeks without change, flesh meat a month, and strawberries from one year to the other. Near this hut there was another of the same kind, in which snow is kept for sale at Lucerne in the summer; but there was no snow in the cave where he observed the temperature. Close to the hut, under the same roof, a fire was kept for domestic purposes, without any apprehension of its affecting the temperature of the cave. In winter it freezes in these huts rather later than in the open air, but afterwards, as they affirm, more strongly; no doubt, says M. De Saussure; by reason of the returning current of air into these subterraneous cavities.

The mountain which overlooks these caves is calcareous, its steep side lying to the north, against which the huts are placed. Its name is Renq, and its foot advances into the lake of Lucerne, where it forms a promontory. It is one of the bases of Mount Pilate, of which it forms part. M. Peyffier informed M. De Saussure, that the lake is very deep near the rock.

It appears that the cold wind issues at many places; for at the foot of the mountain in the neighbourhood, if the earth be any where removed which covers the rubbish, the cold wind which issues forth may be felt against the hand *.

The theory by which M. De Saussure explains the cold of these caves is as follows:

It must be supposed that the air which cools these caves was included in subterraneous cavities, not sufficiently deep to be inaccessible to the heat of summer, and the cold of winter, but yet enough so to admit of a variation of a few degrees from these changes. It must also be supposed, that after this air has been somewhat condensed by the winter's cold, and the summer's heat begins to cause it to issue forth by dilatation, it is again cooled by evaporation during its passage through humid clefts, or the interstices of wet stones.

He thinks that the existence of such reservoirs, accessible to the cold of winter and the heat of summer, is not hypothetical, but an immediate consequence of the facts universally attested by the possessors of these caves, namely, that the air issues out in the summer with so much the more force as the weather is hotter, and returns again by the same apertures in proportion to the intensity of the cold.

Though the cooling effect of evaporation admits of no doubt, he thought it proper to make an experiment resembling what he conceives to happen in this phenomenon. He took a tube of glass of one inch in diameter, which he filled with fragments of wetted stone. Through this tube he forced the blast of a large pair of bellows: the air issued out of the bellows at the temperature of 18 degrees, and its passage through the tube caused it to descend to 15. He had the same result when he used a chemical receiver with two necks, half filled with small flints wetted; but when he directed the wind of the same bellows against the ball of a thermometer surrounded with a wet cloth, the refrigeration was four degrees. A still greater refrigeration, namely, of nine degrees, was produced by inclosing the ball of the thermometer in a wet sponge, and whirling it round in the air. But, as this candid observer justly remarks, the air in this last case is continually renewed; so that the circumstances are not accurately parallel to those in which the air is supposed to become continually more and more loaded with moisture.

* To these instances may be added the caves of Roquefort, of which a description is given by Chaptal in the *Annales de Chimie*, IV. 31, 45. From the description of the caves themselves, which is less precise than those of M. De Saussure, it appears that the air issues from among the fragments of a calcareous mountain. These caves are used in the manufacture of a peculiar and highly esteemed cheese. M. Marcorelle in the month of October found the thermometer of Reaumur descend in these caves to $5\frac{1}{2}$ degrees; while it stood at 13 degrees in the open air; and Chaptal on the 21st August 1787, with a good thermometer, which stood at 23 in the shade in the open air, found the temperature of a rapid current in one of the caves to be 4 degrees. He was informed that the thermometer had been seen in that exposition as low as 2 degrees above zero. The hotter the external air, the cooler the caves are found to be, because the current is then stronger.

Mr. Chaptal supposes, in his explanation, which is very concise, that the external air enters the earth, and is cooled by evaporation; but he does not point out any cause why the current is produced, and varies in its velocity in proportion to the external heat. N.

He thinks, therefore, that evaporation would not suffice to explain the refrigeration of seven or eight degrees below temperate, such as is observed in the caves of Lugan, but that it would suffice to explain a cold of five or six degrees below this term, such as that at Cesi, Ischia, and Mont Testaccio. In fact, he supposes a great subterraneous reservoir of air to exist sufficiently near the surface of the earth, that the cold of the winter may cause it to descend three degrees below temperate, or the tenth degree, and that the heat of summer may cause it to rise as much above that term.

When the cold shall have penetrated this reservoir to its maximum, the temperature will be seven degrees.

Afterwards, when the heat of spring begins to dilate it, its temperature will rise, suppose to eight. It will begin to issue out; and the evaporation, diminishing its heat three degrees, will reduce it to five; and this will be its term of the greatest cold. In proportion as the heats of summer shall penetrate the reservoir, the heat of the air which issues out will increase also: it will not however rise above temperate, because the greatest heat of the reservoir will be 13, and evaporation will reduce it to 10.

The comparison of his experiments with each other, as well as with those of the Abbé Nollet, proves, in fact, that the heat of these cool winds increases as the season advances. For the Abbé Nollet found the caves of Mont Testaccio at 9,5 on the 9th of September, whereas M. De Saussure observed them at 5,3 on the 1st of July. In the same manner he found the caves of Lugan at 4½ on the 1st of August, and at 2½ a month earlier, namely, on the 29th of June.

When the air has obtained its highest degree of heat, it must remain for a certain time in a state of stagnation; after which the reservoir begins to cool, and absorb the external air. In this situation the coolness of the autumn, and the frosts of winter, are sufficient to preserve the cold state of the caves.

The author remarks, that from the original supposition of the medium temperature being at 10 degrees, and the cold produced by evaporation 3 degrees, it is impossible to explain a degree of cold which in summer is lower than five degrees. For if the reservoir be supposed nearer the surface, such for example as that the cold of the winter would cause it to descend to 5, it is true that the air with the additional cooling by evaporation would issue out in the spring at the temperature of two degrees. But it will likewise follow, that the reservoir would rise in its temperature proportionably higher by the heat of summer.

If, therefore, it be required to explain a greater degree of coolness than 4 or 5 degrees, such as that of Lugan and Hergisweil, and it be not supposed that evaporation in these circumstances can produce more refrigeration than three degrees, it will be necessary to suppose the mean temperature of the reservoir to be lower than 10 degrees; a supposition, as the author observes, by no means forced, at least for the vicinity of the Alps; where alone observations have been made of caves possessing so low a temperature.

M. De Saussure anticipates the objection against his theory, that if the air in these caverns be already saturated with humidity, it cannot produce evaporation, nor consequently cold. But he remarks, that caverns are not all humid; that the caverns here supposed must be of vast extent, in order that the dilatation caused by a few degrees shall afford considerable currents of air through the whole summer; and consequently, that a great quantity of cold dry air must enter in with the winter, which will be very desiccative when expanded by

by heat, and will dry the sides of these caverns. Whence he thinks that the air may be considered as sufficiently dry to produce an evaporation which may cool it three degrees.

After having given so interesting and accurate a detail of facts on this curious subject, from the work of a philosopher to whom the world is so greatly indebted, I have thought it a point of justice to relate his theory in a manner scarcely less copious, and that the more particularly, as the notions which occur to me on the subject have led me to differ from him in opinion.

In the first place I must remark, that we have no actual information of the existence of such vast caverns, especially so near the surface of the earth. 2. By the author's own experiments it appears, that at very inconsiderable depths, compared with the thickness required for the roofs of such caverns, the influence of the seasons is scarcely perceptible. He found the greatest difference at the depth of 29½ feet, in compact ground, during three years, was only from 8,95 degrees to 7,75 degrees, or 1,2 degrees in the whole. 3. If the roof were ever so thin, it appears from Count Rumford's experiments *, that heat, which in air, and very probably in all fluids whatsoever, is scarcely transmitted but by the ascent of the heated parts or descent of the parts cooled, would not pass downwards, because the rarefied parts could not descend; consequently the expansion, even in this most improbable situation, could not take place. 4. From the experiments of Duvernois, related in the *Encyclopédie Méthodique*, article AIR, page 686, which are also to be found in the first volume of the *Annales de Chimie*, it appears that common air dilates rather more than $\frac{1}{3}$ th part of its bulk by the first twenty degrees of Reaumur, and therefore scarcely more than $\frac{1}{8}$ th part by the difference of temperature supposed by M. De Saussure. This cave must therefore contain near the surface of the earth, to be within the reach of the seasons, eighty times as much air as flows out of all the apertures during the whole summer. For want of data, that is to say, the apertures and velocities of emission, it is impossible to institute a calculation; but it seems utterly improbable that such a volume of air should have been reserved in an appropriate vessel. At all events, it is not in the smallest degree likely, that Mont Testaceo contains, or communicates with, such a reservoir. 5. Lastly, it seems a gratuitous supposition, that the reservoir should be always dry, and the passage through which the air issues always wet.

After this undisguised examination of the considerations of M. De Saussure, I shall myself present a few notions respecting the cause of this phenomenon; with the perfect wish to see them refuted, if existing facts, future discoveries, or undetected errors should render it necessary.

The whole, then, appears to me to depend upon the simple circumstance of the cavity between a considerable mass of stones or other substances not being capable of speedily changing its temperature, by any other means than the contact of the air, or other fluids, which may pass through it. Let us suppose, for example, the cathedral of St. Paul's in London, the dome of which is near 400 feet in height, to be filled with fragments of broken pottery. This large mass might be supposed at first to possess the temperature of the air at the time when it was accumulated. From the imperfect conducting power of pottery and most earthy substances, the effect of the sun's rays, or of the actual heat of the surrounding air, would pene-

* Experimental Essays, VII.

trate to a very little depth. From several known facts, it does not seem probable that the variations from these causes would extend to the depth of three feet. Suppose now the external air to be cooled ten degrees; the whole body of air contained in the interstices of the pottery, being lighter than that without, would ascend through the upper openings of the mass, in consequence of the pressure from the denser air into the lower apertures. This process would continue until the contents of the edifice had acquired the common temperature; that is to say (without attending to the capacities of pottery and of air for heat, and the relative bulks of the interstices to the solid parts, but supposing these to be equal), it would follow that a quantity of air near one thousand times the bulk of the edifice must pass through the interstices before the common temperature would be restored. As the winter advanced, the current would continue to flow in at the doors, or near the base of the heap, and out from the superior parts. This would continue so long as the temperature of the external air continued to be colder than the internal parts of the heap; and the velocity would be greater the greater the difference between these temperatures. But when the winter had reduced the whole mass nearly to the freezing point, or perhaps below it, the return of spring, rarefying the outward air, would suffer the internal cold dense air to flow out below. The warm air would necessarily press in above, become cooled, and flow out again beneath in that state of diminished temperature. And in this case also the velocity of the current would be greater the hotter the external air.

It is easy to suppose a prodigious variety of cases. If the mass be very large, the extreme variations will be more considerable, and the effect more permanent and steady. If the fragments be large, the current will be swifter, but the refrigeration less. And if among these fragments there be a constant cause of humidity, it may be inferred that this will render the descending current colder by evaporation than it would otherwise have been.

I was led to mention the cathedral of St. Paul's, from having very frequently in hot weather met a strong cool blast issuing out of the doors and vaults of that edifice, produced, as I suppose, by the refrigeration of the air against the ponderous masses of masonry within its area. As the probable cause did not occur to me till the present investigation offered itself, I have not examined all the circumstances of this last fact. Currents of this nature are however very common. The well-known experiment of holding a candle first at the top and then at the bottom of a door, is of this kind. If the air within the room, or the walls, be warmer than the external air, the flame will be blown outwards at the top, and inwards at the bottom of the door; but the contrary will happen if the walls of the room or its contents be colder. In this case, the lower current resembles the cooling blast of the caves. There is no doubt, if two holes were bored in the door of a cellar in summer, the one near the top and the other near the bottom, that the upper hole would draw in the air, and the other emit a cold stream, until the walls had acquired the heat of the external air.

X.

The Combustion of Phosphorus in the Vacuum of the Air Pump. By DR. MARTINUS VAN MARUM*.

I. HAVING proposed in December 1794 to exhibit in one of the lectures on the Teylerian foundation the combustion of phosphorus in oxygen, and its combination with that

* *Annales de Chimie*, XXI. 158.

substance in a glass vessel, after the manner of M. Lavoisier; but not being able to avail myself of the burning-glass to set fire to the phosphorus, I supposed I might succeed by electric sparks. I attempted therefore to give fire to the phosphorus by electrical sparks or small discharges. Neither of these being attended with success, I attached to a small piece of phosphorus a little cotton scarcely weighing three quarters of a grain, which I sprinkled with a little finely powdered resin. I afterwards placed this cotton, which rose about one-fourth of an inch above the phosphorus, in such a position between the extremities of two conducting wires within the glass receiver, that the electric spark might pass through it.

In this manner, on the 4th of December 1794, I attempted for the first time the inflammation and combustion of phosphorus; without any other design, however, in this first experiment than to ascertain, before the lecture, that there should be no fault in the apparatus or method of making the experiment; and the time I could give on that day to this experiment being already expired, I did not rarefy the air in the receiver, more than till the mercury in the barometer gauge was about one inch lower than in the standard barometer beside it. I then passed the oxygene gas from the gasometer into the receiver: I set fire to the phosphorus after the receiver was thus filled, and the experiment was made without any difficulty.

II. As I thought I could now repeat the experiment in my lecture without any risk of failure, I made the attempt a few days afterwards. I placed another small piece of phosphorus in a small crucible of platina, which hung in the centre of a glass receiver thirteen inches in diameter, having first surrounded the upper part of the small stick of phosphorus with a little cotton powdered with resin. I then proceeded to rarefy the air as much as possible; but an unexpected appearance prevented me. The candles had been removed, for the better observation of the light at the surface of the phosphorus. We saw the light increase very perceptibly in magnitude and strength, when the height of the mercury in the barometric gauge still differed one inch from that in the barometer. This light increased in proportion as the air was more rarefied. I was far from supposing that the phosphorus would take fire, for which reason I continued to work the pump; but, contrary to all expectation, we saw the phosphorus take fire when the mercury was about half an inch lower than that in the barometer.

[To be continued.]

MATHEMATICAL CORRESPONDENCE.

QUESTION V. Answered by J. F. R.

THE bulk or volume of a chemical compound being very seldom, and perhaps never, equal to the sum of those of its component parts before their union, it follows that the relative quantities of its ingredients cannot be inferred from its specific gravity by the mere rule of alligation; but recourse must be had to the results of actual experiment. Perhaps M. Baumé's experiments on the specific gravities of different solutions of common salt in water, preparatory to the construction of his *pese-liqueur* (an account of which will be found in this work, p. 38.), with the appreciation of the scale, deduced from the observation of M. D. Morveau, will afford us the best data for the solution of the present question. The specific

specific gravity of the solution given being 1.1213, we shall find, from the table in p. 39, that it contained about 0.1581 of salt, or 444 grains in the whole; being near 29 grains less than what would result from mere computation.

* * The above was the answer sent previous to the appearance of the Editor's note, p. 192. E. was sufficiently aware that the abovementioned table was not to be relied on to indicate the specific gravities of all possible solutions of common salt in water; which indeed he conceived to be too obvious as to preclude the necessity of more restrictive terms. It is however certainly true, that, if Baumé's construction of the instrument, and De Morveau's observation, were both correct, the tabular specific gravity answering to 15° would be accurately that of the solution of 0.15 of salt used by the former; and that the errors of the table, for all inferior degrees of saturation, would be those only arising from the hypothesis of equal graduation on which it is calculated; which, as the Editor observes, p. 192, are probably not very considerable. According, therefore, to this mode of reasoning, the table ought to be perfectly correct about that degree, and serve well enough for giving nearly the specific gravities of all aqueous solutions of common salt, in which its proportion does not exceed 0.17 or 0.18. The great difference between this table and that of Dr. Watson (p. 192.), which gives upwards of 0.18 of salt, instead of 0.15, for the specific gravity of 1.114, seems only to be attributable to the reasons assigned p. 38. Perhaps M. Baumé had either exposed the salt to a considerable heat before he dissolved it, or suffered the solution to evaporate before he used it for the instrument which fell into the hands of M. De Morveau; or possibly even its scale had been transferred from another whose stem bore a smaller proportion to its bulb. The latter table is probably the most to be depended on in all cases.

NEW MATHEMATICAL QUESTION.

QUESTION IX. *By TRIGONOMETRICUS.*

THE angles of elevation of a terrestrial object situated above the horizon, taken at three given stations in a horizontal plane, being given; it is required to determine from thence its perpendicular height.

NEW PUBLICATIONS.

Count Rumford's Experimental Essays, Political, Economical, and Philosophical. Essay VII. Of the Manner in which Heat is propagated in Fluids. Of a remarkable Law which has been found to obtain in the Condensation of Water with Cold, when it is near the Temperature at which it freezes; and of the wonderful Effects which are produced by the Operation of that Law in the Economy of Nature. Together with Conjectures respecting the final Cause of the Saltness of the Sea. Octavo, 108 pages, with two plates. Price 2s. stitched. Cadell and Davies, 1797.

THIS essay, which contains important discoveries and applications of the doctrine of heat, will demand particular and more ample notice in the present work than can be here given. The author had before proved that the communication of heat through air is almost totally effected

effected by the circulation of its parts. He has since ascertained, that water, and thence probably all other fluids, conduct that quality or matter by a similar process; and from the general facts observed by him, he concludes that they are non-conductors of heat. The experiments contained in this essay are very striking and ingenious, and the author has availed himself of them in forming a set of deductions of great use in the explanation of the economy of heat on the surface of the planet we inhabit.

Traité de la Fonte des Mines, &c.; or, A Treatise on the Fusion of Ores by Pitcoal, and the Construction and Use of Furnaces proper for the Fusion and Refining of Metals and Minerals by means of Pitcoal. Together with the Method of rendering this Coal fit for the same Uses as Charcoal of Wood. By Mr. De Genssane, of the Royal Academy of Sciences at Montpellier, &c. Sold at Paris at the Veterinary Library of M. R. Huzard, Rue de L'Eperon-Saint-Andre-des-Arcs, No. 11. 2 volumes in quarto; the first containing 400 pages, with 34 plates, and the second 534 pages, with 42 plates. Price 20 francs for the two volumes in boards, and 12 francs for the second separately.

The first volume of this work appeared in 1770, and the second, which was retarded at first to verify certain facts, and afterwards for other reasons, was printed in 1776. Other circumstances, unnecessary to be detailed, have prevented the proprietor from offering the work regularly to sale until lately.—*Journal des Mines.*

Anfangsgrunde der Chemie zum Grundrisse Academischer Vorlesungen nach dem neuen Systeme, &c.; or, Elements of Chemistry, to serve as the Plan of Academical Lessons according to the new Theory. By M. G. Fr. Hildebrandt, Professor of Medicine and Chemistry at the University of Erlang. Printed at Erlang 1794, 3 volumes, large octavo.

The celebrated L. B. Guyton, in the *Annales de Chimie*, XXI. 333, speaks very highly of this work, for its order and perspicuity. The author begins by exhibiting the general principles of mixture, solution, affinity, precipitation, &c. together with the operations of chemistry. In the next place he treats in separate sections on caloric, light, oxygene, azote, atmospheric air, hydrogen water, the earths, alkalis, acids, and compound salts. These subjects occupy his first volume.—The second volume treats of metals, their alloys, and combinations.—And in the third the chemistry of organized substances is explained.

Notizie, &c. An Account of the Siliceous Incrustations of the Thermal Baths of Italy; and certain remarkable Products found under the Lava which buried Part of the Town of Torre del Greco in 1794. By M. Tompson. Octavo. Naples, 1795.

The author has observed siliceous stalactites in the mountain of Santa Fiora in Tuscany, in the Euganean mountains, in the isle of Ischia, and in the crater of the Solfatara or Pozzuolo. In most of these places there are likewise soda, humid and hot vapour, and sulphur, either in substance or in the state of sulphuric acid. The solution of the flix may therefore be attributed to the combined action of these three chemical agents, particularly the soda. A kind of liquor silicum is formed. The author has discovered that the *arundo donax* of the family of the bamboos contains sea-salt, and consequently soda; and it is known that the knots of the bamboo contain siliceous concretions. The celebrated Black ascertained the presence of mineral alkali in the Geyser spring in Iceland; round which siliceous incrustations are formed.—*Edinburgh Transactions*, Vol. 2 and 3.

By digging into the houses of the town of Torre del Greco, which were buried under the lava in 1794, among other things were found: 1. Glass changed into Reaumur's porcelain.

porcelain. 2. Iron mineralized, augmented in volume to thrice its former bulk; crystallized within in grains or plates, some of them three lines in length, with the argentine brilliancy; other portions in red rosettes of specular iron ore, more or less participating of the nature of sulphate of iron. 3. Silver coin fused in the same degree of heat which copper money resisted. 4. A candlestick of brass (*chandelier de laiton*) entirely metamorphosed, the outside crystallized into blende, intermixed with octahedrons of copper, more or less red: the fracture of the candlestick towards the centre presents very fine cubes of red copper. 5. Lead converted in some instances to litharge, and in others to minium, which is solid, compact, and of the most beautiful red colour.—Pichet, *Bibliothèque Britannique*.

An Inaugural Dissertation on the Chemical and Medical History of Septon, or Azote.

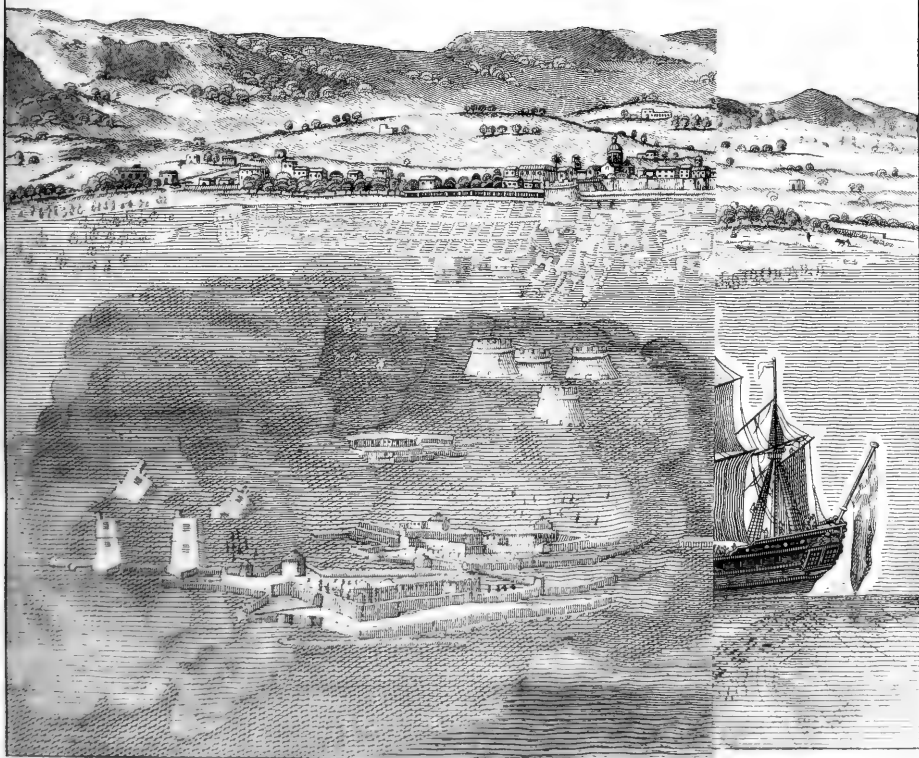
By Winthrop Saltonstall. Octavo. New York, 1796.

I find this work announced in the 22d volume of the *Annals of Chemistry*, No. 64, for last April, with the following observations:

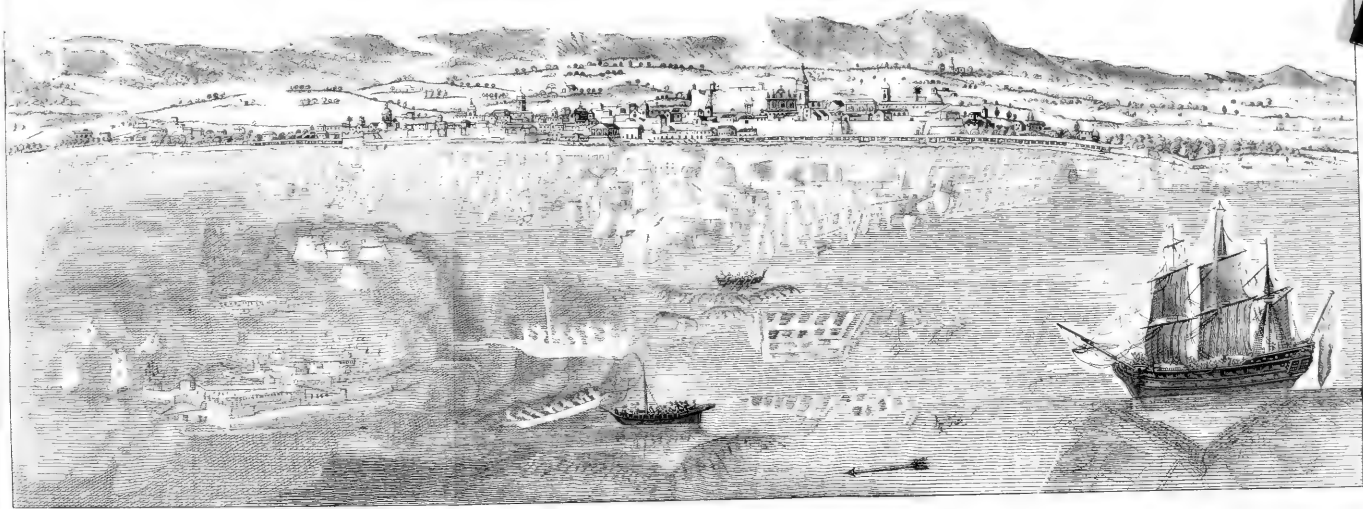
It is known that azotic gas forms 73 parts out of 100 in the atmosphere. Cavendish made the important discovery, that 68 parts of oxygene, with 32 of azote, produced nitrous gas. Berthollet has proved that 6 parts of azote and 1 of hydrogen form ammoniac. Deimann and Van Troostwyk have lately proved that 37 parts of oxygene, chemically united to 63 of azote, form that gaseous oxide called dephlogisticated nitrous air by Priestley, which has the singular property of supporting combustion, though it destroys the life of animals which respire it. So that we know of four degrees of oxidation or combustion of azote: 1. The dephlogisticated nitrous air of Priestley, called by the modern chemists oxide of azote. 2. Nitrous gas. 3. Nitrous acid gas. 4. Nitric acid.

This author thinks that the putrefaction of animal substances has an epocha in which the azote, at the instant of its disengagement, coming into contact with oxygene, may combine with it without requiring a very elevated temperature. The oxide of azote thus formed may become a very active poison; and the cancer, with the whole family of corroding ulcers, has probably no other origin. The exhalations of azotic substances which putrefy in marshes, in prisons, and in damp hot countries, form, according to the hypothesis of the author, a certain chemical combination of azote with oxygene, which becomes the cause of contagion, and of various endemic and epidemic disorders. This destructive oxide may introduce itself into the animal system by the lungs, and produce putrid fevers and asthmatic symptoms; it may penetrate with the aliments into the stomach, and become the leaven of epidemic bilious disorders. Does not the yellow fever of America depend on the same morbid gas, since we know that it is azote which gives the more or less intense orange colour to wool, silk or skin, when it is abundantly contained in those substances, and combined with a small portion of oxygene? Lastly, The oxide of azote may act on the absorbent vessels of the animal, and produce pestilential bubos and sores.

Here, say the editors, are conjectures not entirely without some appearance of truth; but which require many experiments and observations before they can be admitted in the extensive degree proposed by this author. For example, it is so far from being demonstrated, as the author thinks, that the orange colour of wool, &c. is owing to azote, that it rather appears to arise from an excess of carbone. The same may be remarked of the oxide of septon of this author, or dephlogisticated nitrous acid. This gas appears to be nitrous vapour mixed with oxygene gas, which last does not unite with the surplus of nitrous gas in the acid.



Coast View of the



A View of the City of Reggio, with a Sketch of the Phenomenon called Fata Morgana.

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AND
THE ARTS.

SEPTEMBER 1797.

ARTICLE I.

*Experiments and Observations made with the View of ascertaining the Nature of the Gaz produced by passing Electric Discharges through Water; with a Description of the Apparatus for these Experiments. By GEORGE PEARSON, M. D. F. R. S.**

IN the Journal de Physique for the month of November 1789, were published the very curious and interesting experiments of Messrs. Paets Van Troostwyk and Deiman, with the assistance of Mr. Cuthbertson, on the apparent decomposition of water by electric discharges.

The apparatus employed was a tube 12 inches in length, and its bore was 1-8th of an inch in diameter, English measure; which was hermetically sealed at one end, and, while it was sealing, $1\frac{1}{2}$ inch of gold or platina wire was introduced within the tube, and fixed into the closed end, by melting the glass around the extremity of the wire. Another wire of platina, or of gold, with platina wire at its extremity, immersed in quicksilver, was introduced at the open end of the tube, which extended to within 5-8ths of an inch of the upper wire, which, as was just said, was fixed into the sealed extremity†.

The tube was filled with distilled water, which had been freed from air by means of

* Communicated by the Author. This paper was read before the Royal Society, and an abstract of the same published in the Transactions for 1797. N.

† In another part of Mr. Van Troostwyk's memoir it is stated that the distance was $1\frac{1}{2}$ inch from the end of the upper wire to the top of the lower wire; and that the distance between the insulated ball and prime conductor was at first 3-4ths of an inch, but that afterwards it was increased to one inch. Although the wire fastened into the top of the tube was said to be $1\frac{1}{2}$ inch in length, it is observed, that when a column of 3-8ths of an inch of air was collected, it was almost at the extremity of the upper wire. From these and other inaccuracies, it will be made appear, that no one, from the account published, has been able to repeat the experiment.

Cuthbertson's last improved air pump, of the greatest rarefying power. As the open end of the tube was immersed in a cup of quicksilver, a little common air was let up into the convex part of the curved end of the tube, with the view of preventing fracture from the electrical discharges.

The wire which passed through the sealed extremity was set in contact with a brass insulated ball; and this insulated ball was placed at a little distance from the prime conductor of the electrical machine. The wire of the lower or open extremity, immersed in quicksilver, communicated by a wire or chain with the exterior coated surface of a Leyden jar, which contained about a square foot of coating; and the ball of the jar was in contact with the prime conductor.

The electrical machine consisted of two plates of 31 inches in diameter, and similar to that of Teyler. It possessed the power of causing the jar to discharge itself 25 times in 15 revolutions. When the brass ball and that of the prime conductor were in contact, no air or gas was disengaged from the water by the electrical discharges; but on gradually increasing their distance from one another, the position was found in which gas was disengaged, and which ascended immediately to the top of the tube. By continuing the discharges, gas continued to be disengaged, and ascend, till it reached nearly to the lower extremity of the upper wire; and then a discharge occasioned the whole of the gas to disappear, a small portion excepted, and its place was consequently supplied by water.

The residuary portion of gas being let out after each experiment, and the discharges being continued in the same water, this residuary gas was left in smaller and smaller quantity; so that after four experiments, probably made on the same day, it did not amount to more than 1-80th of the bulk of gas which had been produced. If it had been possible to pass electric sparks through this very small quantity of gas a second time, or oftener, it was supposed it would have been diminished still more. But when the tube had been left for a night only filled with water, the residuary gas was in greater quantity than after the last experiment the preceding day*.

It was concluded that the gas produced by the electrical discharges was oxygen and hydrogen gas, from decomposed water:

1. Because no other gas hitherto known instantly disappears on passing through it an electric spark.
2. The gas obtained must have been the oxygen and hydrogen of decomposed water, because they were in exactly those proportions in which by combination they reproduce water; the trifling residue being considered to be merely a portion of air which had been dissolved in the water.
3. Liquids which are not compounded of hydrogen and oxygen, as sulphuric and nitric acids, afforded gas by the electric discharges, but which did not disappear on passing through it an electric spark; but which did disappear on adding to it nitrous gas over water. Mr. Schurer also asserts, on the authority of Mr. Van Troostwyk, that even liquid

* In at least fifty experiments I have never seen the residue of gas less than 1-40th of the gas produced, although the water had been freed from air by the most effectual means. But Mr. Schurer (*Annales de Chimie*, tom. v. p. 276.) testifies that he saw Mr. Van Troostwyk make the experiment; and that after it was repeated many times, on the same parcel of water, there was no residue at all. I have very good grounds for believing, that this is one of the number of inaccuracies in the account published of this subject.

muratic acid, which contains a very large proportion of water, affords hydrogen gas only, the oxygen being absorbed by the muriatic acid, and becoming oxy-muriatic acid.

From much experience I can safely affirm, that it is scarcely possible for the student, or even the proficient, to institute the above experiment with success from the explanation published. Hence, during the six years which have elapsed since its publication, no confirmation has been published except the experiment repeated by Mr. Cuthbertson, for my satisfaction, as related in my work on the chemical nomenclature; but I have heard of many persons, and some of whom were experienced electricians and chemists, who made the attempt.

Since Mr. Cuthbertson came to reside in London, I have learned from him the circumstances requisite to the success of the experiment; and I have received from him also very great assistance in continuing a process with the objects I had in view, the tediousness and even difficulties of which can only be conceived by those who have been engaged in the same pursuit.

I am very sensible that it would be unnecessary for me to explain the importance of a process which may at last afford demonstration of the composition of water, by the fullest and unequivocal evidence of its analysis and synthesis; a demonstration which no other single process but the present promises to afford.

I propose therefore in this paper:

1. To give such a description of the experiment of rendering water into gas by electric discharges, as shall enable any person who is versed in pneumatic chemistry, and acquainted with the theory and practice of electricity, to repeat it with success. By this description, also, I apprehend I shall make known more generally the very elegant, and frequently most satisfactory, mode of decomposing and compounding bodies, by means of the fire of the electric discharge.

2. It is proposed to relate the additional evidence which I have already obtained from this process, concerning the composition of water. For although it seems most probable that water is really decomposed in Mr. Van Troostwyk's experiment, it must be confessed that it does not make appear a single unequivocal and decisive property of hydrogen and oxygen in the gas produced. The disappearance of this gas by combustion, or in some other way, instantly on passing through it an electric spark, it is true, is a property known only to belong to the mixture of oxygen and hydrogen gas; but it is well ascertained, that things of totally different species may agree in one or more properties. And there is at least a possibility, that electric discharges may produce various other kinds of gases in water, beside hydrogen and oxygen from decomposed water; and which may have the property of instantly disappearing on passing through them an electric spark.

3. I shall attempt to resolve the phenomena of the process into a general law of the action of fire, or of the joint action of caloric and light.

SECTION I.

Of the Manner of conducting the Process.

ELECTRIC discharges may be employed in two different manners to decompose water. One of these is by what has been termed *the interrupted explosion*; which was the method,

thod, although not so explained, of Mr. Van Troostwyk. And the other method is by means of the uninterrupted or complete explosion; for which there are two different kinds of apparatus. These were invented by Mr. Cuthbertson in the course of my investigation of this subject.

To succeed by the method of the interrupted explosion, the following are the necessary parts of the apparatus to be used, and the circumstances to be attended to:

1. *The electrical machine must possess sufficient power.* I do not think any cylindrical machine can be made to answer in this process if a large quantity of gas be required; because they cannot be made to act with due regularity, constancy, and force. Inequality of the surface of the cylinder is unavoidable, which causes undulation. A cylindrical machine never continues in full force above five or six minutes, without fresh amalgam. Hence, from the repeated amalgamization, the discharges will be so variable that the tubes must be frequently broken.

I used plate machines of a peculiar construction by Cuthbertson. These machines do not require fresh amalgamization oftener than once in eight hours, and they possess superior powers of acting, in point of regularity, force, and duration. A plate of 24 inches in diameter, with a jar containing 150 square inches of coating, afforded an adequate discharge every second or third revolution, for several hours; and for a still longer time every third or fourth revolution, with one application of amalgam. A 31 inch plate-machine afforded a due discharge at first every revolution, and afterwards every second revolution for many hours, with one application of amalgam. The most useful and expeditious machine was that with two plates, each 24 inches in diameter, and similar to that of Teyler. It produced 25 discharges every 15 revolutions for an hour or two; and for four or five hours longer a discharge was produced by less than two revolutions, with one amalgamization.

2. *The Leyden jar must have a sufficient quantity of coated surface;* without which the discharge will not be sufficiently powerful to produce the gas required. The proper quantity, as found by experience, was about 150 or 160 square inches, with an usual proportional prime conductor.

3. *The distance between the insulated ball, and the prime conductor, must always be less than the distance between the extremities of the wires.* Not the least notice of this circumstance has been taken; yet without attention to it the experiment can never succeed, or only for a very short time. Accordingly, as the distance between the extremities of the wires within the tube answered best when it was 5-8ths or 7-8ths of an inch, the distance between the insulated ball and prime conductor was seldom more, but frequently less, than 5-8ths or 6-8ths of an inch. The eye must be kept upon the sparks within the tube, and by practice a person may become a judge of their force by their vividness; which will direct him to bring the receiving ball nearer to the prime conductor, when there appears danger of the tube being broken; and on the contrary, to remove them to a greater distance from one another when the sparks do not produce gas duly from the water. When the discharge is of the most productive force, both ends of the wire within the tube will be illuminated by a spark; but, when it is weaker, one end only of the wire will be illuminated; and when this is the case, there is no risk of the tube being fractured, but gas will rise from the end of one wire only instead of two.

4. *The extremities of the upper and under wire within the tube must be at a certain distance from*

from one another. If they be too near one another, the points of them will not be illuminated; and provided the insulated ball be as near to the prime conductor as the two wires are to one another, the tube will be broken, because there will be a complete explosion. But if the wires be at too great a distance from one another, the electric fluid of the discharge will be so diffused through the water that no gas will be produced. If the Leyden jar contain, as above stated, 150 or 160 square inches of coated surface; and the ball of the prime conductor and of the insulated ball be about three inches in diameter, the distance between the wires which generally answered best, was about $\frac{5}{8}$ ths or $\frac{7}{8}$ ths of an inch, as above said. The narrower the bore of the tube, the greater may be the distance between the two wires; accordingly, the distance may be one inch with a tube $\frac{1}{14}$ th of an inch wide.

5. *The upper wire fixed into the closed extremity of the tubes must be of a proper length and thickness.* If this wire be too long, either the discharge will not be carried through to the end of the lower wire in sufficient quantity to produce gas; or, if it be in sufficient quantity to produce gas, the tube will be fractured. The smaller the diameter of the tube, the longer may be the upper wire, for a reason to be given under the next head. I generally found that the discharge requisite to produce gas fractured the tube, if the upper wire was more than $\frac{6}{8}$ ths or $\frac{7}{8}$ ths of an inch in length, within a tube of more than $\frac{1}{8}$ th of an inch in diameter. But with very narrow tubes, such as those of $\frac{1}{16}$ th of an inch in diameter, I frequently succeeded when the upper wire was $\frac{1}{4}$ th of an inch in length. It is obvious, that the shorter the upper wire the more readily will gas be produced: the process, however, will be rendered still more tedious in those cases in which a quantity of gas is to be collected in a reservoir for examination; on account of the time consumed in transferring such small parcels of gas by each experiment.

The diameter of the upper wire cannot, perhaps, be too small; for the greater its superficies, the more electric fluid will be parted with to the surrounding water. Hence platina wire of the finest sort, as that of $\frac{1}{24}$ th of an inch in diameter, may be used with superior advantages. This sort of wire also cannot be melted while it is soldering to the glass, which can hardly be prevented with fine wire of other metals. However, I found that copper, brass, or gilt wire, of about $\frac{1}{8}$ th or $\frac{1}{10}$ th of an inch in diameter, could be soldered to the glass, and answered perfectly. I did not find that any of the metal wires were affected by the discharge; but iron or steel ones are not proper, on account of their being so soon oxidised by the water, and consequently extricated from it hydrogen gas. I did not find any advantage from using several small wires twisted together, but separated at the end within the tube; for gas was extricated generally, at the point of one of them only, namely, the undermost. I think care should be taken to fix the upper wire so that it shall be in the middle of the tube; as in that case the tube will be less liable to be broken. As to the under wire, the diameter of it seems to be of little importance; for, if it be a thick one, as much gas will be extricated as if it were a small one; because the electric discharge will take the first point of the surface of the wire where the gas is produced.

6. *The tubes must be of a proper length and diameter.* If they be shorter than seven inches, the discharge will be liable to pass over the outside; and if they be longer than twelve inches, they will be of an inconvenient length. I found the most convenient length to be from nine to ten inches, exclusive of the curved part. The curved part was found very useful in preventing air ascending, which was accidentally let into the tube, by which the product

product of gas from the experiment would have been contaminated. Such curved extremities were however less convenient than straight ones, on account of the greater difficulty of transferring from the former than from the latter. See Fig. 1 and 2, Plate XI.

The diameter of the tubes should not be more than $\frac{1}{8}$ th or less than $\frac{1}{12}$ th of an inch. At least in my experiments these tubes answered best. If they were wider, the discharge requisite to produce gas broke the tube. If the tubes be narrower than just mentioned, the experimenter will find it difficult and tedious to transfer the gas, through the curved part, into a reservoir, in those cases in which a large quantity is wanted for examination. Where however the object is merely to shew the production of gas by means of the electrical discharge passed through water, between two wires; and the instant disappearance of gas so produced by passing through it an electric spark, the narrowest tubes are most eligible; as with them the experiment can be made in a shorter time. But there was one inconvenience experienced from very narrow tubes, namely, the bubbles of gas were very apt to hang near the end of the upper wire instead of ascending; and they were apt to form large bubbles, with water between each; in which situation the discharge frequently fractured the tube, or made gas to disappear by combustion in the course of the experiment. To possess at once the advantages of the narrow and wide tubes; about three to six inches of narrow tube were joined by fusion to a bottom part of wider tube (see Fig. 3); into the curve of which tube the gas produced was let up from time to time; so that this part frequently contained the products of ten or more experiments before the gas was transferred into the reservoir. The vast number of tubes which were broken in this experiment induced me to try various different kinds of them. I experienced no advantages from annealing tubes; but their being seemingly rendered more brittle, and harder, by this treatment, was an effect which I least of all expected. Tubes with thin sides answered just as well as with thick ones. Bohemian green glass tubes were found excellent for this experiment, as they were much less apt to be cracked by the discharges than any kind of English glass. To save time and trouble in so frequently letting out gas produced in very narrow tubes, a small bulb was made at the sealed end (see Fig. 4). In this case, however, as the upper wire must be shorter than other narrow tubes, it was more difficult to regulate the explosion.

Although common atmospherical air is an electric, and water is a conductor of electricity, it appears that the discharge passes with more resistance from wire to wire through water in the above experiment, than through common air under otherwise the same circumstances. The reason of which is this: air being an elastic and very rare fluid, it more readily gives way to the electric discharge than water; and it can therefore pass through a longer and thicker column of air between two wires, without breaking the glass tubes, than it can through water. For although water is a conductor, yet in a very small quantity it is a very indifferent one; so that its density and defect of elasticity more than compensate for its conducting power. Hence also, and on account of the conducting power of water, the reason of the upper wire in this experiment being shorter in proportion as the tube is wider; and on the same account will be seen the reason of the advantages of a small upper wire over thicker ones.

It will be necessary to add, that the tubes with curved extremities can only be filled by setting them in water under a receiver, and exhausting the air from the receiver, tubes, and water; then, by letting in the air again, the water will be forced up into the tubes. Sometimes, however, I have filled the tubes by setting them in Papin's digester.

These are the directions for making the experiment; but the rationale of it cannot be understood unless the nature of the *interrupted explosion* be explained; because I believe books on electricity do not contain the necessary information. It must be considered, in the above experiment, that if in place of water the tubes be filled with air, the whole of the charge of the Leyden jar will pass, at each explosion, from the upper to the under wire, and no interruption in the discharge will happen; but if they are filled with water, then an *interrupted discharge* may be caused; by which is meant that a part of the charge only passes at each explosion through the water, from wire to wire, and with much diminished velocity. The residuary electricity in the Leyden jar is nearly one half; as may be accurately demonstrated from the difference in point of density, elasticity, and conducting power of the medium of water and air as already observed. It must be added, that although water in large quantity is a good conductor, and air is not; yet water being here in very small quantity, it proves a bad conductor, as is the case with the very best conductors. A cubic foot of water is only just capable of receiving, or letting pass through it, a full discharge from a jar of one foot of coated surface; and the quantity of water employed in this experiment not being $\frac{1}{100000}$ th part of a cubic foot, it is a very imperfect conductor; so that an interrupted discharge only can pass through the tube, without dispersing the whole of the water. But if the discharge be not seemingly as strong as the tube can bear without breaking, the gas is not produced from it; and on this point hinges this extremely delicate process.

The situation of the different parts of the above described apparatus is shewn by Fig. 5.

To succeed by the method of the *complete or uninterrupted explosion*, the following apparatus must be used, and rules observed:

1. A tube (Fig. 6.) is employed, about four or five inches in length, and 1-5th or 1-6th of an inch in diameter. One end is mounted with a brass cap (Fig. 7.), and the other end is sealed at the lamp, with a wire about 1-40th of an inch in thickness fixed into it, as above described; which extends into the brass cap, so as to be almost in contact when the explosion is made. If the wire touches the brass cap there will be no explosion. The tube being filled with, and set in, a cup of water, the discharge may be made into it as in the above described process; but here the insulated ball must be placed at a greater distance from the prime conductor, and a Leyden jar, with only 50 square inches of coating, will answer the purpose. In this way of making the experiment, gas is produced by each discharge in the brass tube, and in much greater quantity, and with much less frequent accidents, and less trouble, than in the former method with the interrupted discharge. But the gas obtained with this apparatus always contains a larger proportion of atmospherical air, on account of the quantity of water, and more immediate and extensive communication of it with the atmosphere. By repeated discharges there is an impression made in the brass tube, in the part where the discharge passes through it, and at last a small hole is made in that part. On this account the same mounted tube cannot serve for producing a large quantity of gas.

The other sort of apparatus invented by Mr. Cuthbertson is represented by Fig. 8. At first it consisted of a glass tube half an inch wide and about five inches in length, mounted at one end with a brass funnel, and inverted in a brass dish; but afterwards the tube was blown funnel-wise at the end, as shewn by Fig. 9. The other end must have a wire about

about 1-40th of an inch thick, scaled into it at the lamp; which wire extends to nearly the bottom of the brass dish in which the tube stands.

The exact distance between the end of the wire and brass dish must be found by trials; that which generally answered in my experiments was about 1-20th of an inch. If it be properly arranged, gas will be produced at each discharge.

The Leyden jar used with this apparatus must contain about 150 square inches of coating.

The distance between the insulated ball and the prime conductor, at which the experiment succeeded, was commonly about half an inch.

If experiments be proposed in which electrical discharges must be passed through water or other fluids, for even a much longer time than was consumed in performing those referred to or related in this paper, it may be an object to employ the wind, or perhaps the power of a horse, to turn the electrical machines, the expence of labourers being considerable.

[To be continued.]

II.

Analysis of four Specimens of Steel; with Reflections on the new Methods employed in this Analysis.
By Citizen VAUQUELIN.

[Continued from page 217.]

SECTION III.

STEEL NO. 864—LARGE PIECE.

EXPERIMENT I. 1152 grains, or 61,14 grammes, of this steel, broken into small particles, were dissolved in the sulphuric acid diluted with five parts of water. There remained at the bottom of the solution 5,5 grains, or 0,292 grammes, of black powder of the carburet of iron. In another experiment, the same quantity of this steel afforded 6,4 of carburet of iron; a quantity, the medium of which is 5,95, and consequently the residue forms a fraction of the dissolved mass equal to 0,0051.

Experiment II. 100 grains, or 5,3 grammes, of the same steel, dissolved in the sulphuric acid, afforded 121 inches, or 2420 cubic millimetres of hydrogen gas. The black residue weighed scarcely 0,75 grains.

Experiment III. The 5,5 grains of carburet of the first experiment being subjected to heat under a muffle, took fire, and left three grains of a grey yellowish mass, which, when treated with boiling muriatic acid, assumed a white colour, while the acid became yellow. It was reduced to 1,66 grains, which amounts to 0,0014 of the mass of steel. This substance possessed all the characters of flux.

The muriatic acid used in this boiling contained a perceptible quantity of iron, which appeared to be the cause of the yellow colour it possessed previous to the operation.

Experiment IV. The solution of 1152 grains of iron in the sulphuric acid having been treated as in Experiment III. Section 2, afforded 58 grains of phosphate of iron, which makes

makes about 0,05 of the weight of the steel employed. The fluid likewise deposited by ebullition 96 grains of a deep red matter, which did not perceptibly contain phosphate of iron.

From this result it appears that the present steel contains nearly three times the quantity of phosphorus exhibited by No. 864, the small piece. For 576 afforded only 19 of phosphate of iron, whereas 1152, or only twice the quantity, afforded 58 grains. This steel must consequently be more brittle when cold, if, as there is no reason to doubt from the numerous experiments of Bergman and other chemists, that phosphorus is the essential cause of the cold short quality of iron and steel.

Experiment V. 288 grains of steel No. 864, the large piece, dissolved in sulphureous acid, as before mentioned, left 6,5 of carburet of iron, which, supposing it to contain as much sulphur and iron as that of No. 864, the small piece, would give 3,71 of carbone, or about the 0,010th part, or rather more than one-hundredth part of the steel employed.

Experiment VI. This steel was subjected to all the proofs calculated to shew the presence of manganese, but without any exhibition of that substance. It may therefore be concluded that this steel, as well as the other examined before, does not contain any perceptible quantity of manganese, at least so far as the present methods of chemistry are capable of shewing it*.

SECTION IV.

STEEL NO. 977.

Experiment I. 144 grains, or 7,631 grammes, of this steel, dissolved in the sulphuric acid, diluted with five parts of water, afforded 164,94 cubic inches of hydrogen gas, and left 0,941 grains, or about 0,05 grammes, of carburet of iron, or nearly 0,007 of its mass.

Experiment II. 504 grains, or about 26,7 grammes, of the same steel, dissolved in the sulphuric acid, diluted with five parts of water, the solution being diluted with a large quantity of water, and its excess of acid saturated with carbonate of pot-ash, deposited, a few instants afterwards, a large quantity of a white matter, slightly inclining to grey, which acquired a straw-colour by contact of the air. This matter was phosphate of iron, and weighed 22 grains, or about 1,16 grammes.

Experiment III. The liquid whence the phosphate of iron had been precipitated by the carbonate of pot-ash was submitted to ebullition, and again deposited 22 grains, or 1,16 grammes, of matter, in which, however, there was scarcely any phosphate of iron. So that 504 grains of the steel No. 977 contained 23 or 24 grains of phosphate of iron; that is to say, 0,047 of its mass.

Experiment IV. These 24 grains of phosphate of iron, treated by long ebullition with caustic soda, assumed a deep red colour, and after washing, and drying in the air, weighed only 15,33 grains. They lost therefore 8,67 grains of phosphoric acid, which afforded 17,54 grains of crystallized phosphate of soda.

* The oxide of iron formed with this steel, strongly oxidized by the action of fire, and afterwards treated with the nitric acid and fugar, afforded an oxide which threw down a very beautiful blue with the prussiate of pot-ash. The same oxide fused with borax afforded a greenish globule; but when heated with the external flame at the extremity of a pair of forceps it acquired a slight purple colour, which disappeared as the globule became cold.

Experiment V. This steel, dissolved in the sulphureous acid, afforded very nearly the same quantity of carbure of iron as No. 864, the small piece.

SECTION V.

STEEL NO. 1024.

Experiment I. One hundred grains, or about 5,3 grammes, of filings of this steel afforded 108 cubic inches of hydrogen gas during their solution in the sulphuric acid.

I have remarked that the hydrogen gas produced by this steel had an extremely fetid smell, infinitely stronger than that of the gases afforded by the other steels, which appears to arise from the greater quantity of phosphorus it contains*.

Experiment II. 288 grains of filings of the same steel being dissolved in the sulphuric acid, diluted with five parts of water, the carburet of iron separated, the solution diluted with water, and the excess of acid saturated with carbonate of pot-ash, afforded a very considerable white deposition of phosphate of iron, which, when well washed and dried, remained white, and weighed 26 grains, or about 0,09 of the mass of steel made use of. These 26 grains of phosphate of iron, treated with caustic soda, afforded 22 grains of phosphate of soda, and the ferruginous residue weighed 17 grains.

Experiment III. 288 grains of steel, dissolved in the sulphureous acid, left a carbonaceous residue, which, treated with pot-ash in the manner described Section II. and dried, weighed 3,5 grains. But on account of the minute losses which it is impossible to avoid in a numerous series of manipulations, the quantity of this substance may be estimated at four grains; and that the more reasonably, as the other kinds of steel gave nearly the same results. The quantity of carburet of iron in this steel was therefore somewhat less than 0,014 of the mass of the steel.

Experiment IV. This steel, subjected to the proofs requisite to discover the presence of manganese, presented no trace thereof.

SECTION VI.

Table of the Quantities of Hydrogen Gas afforded by each Kind of Steel.

By uniting the different quantities of hydrogen gas afforded by the decimastic quintal of each of the steels dissolved in the sulphuric acid, we have the following results for each 100 grains.

	Cubic inches.
Steel, No. 864, small piece	108
No. 864, large piece	121
No. 977	114
No. 1024	108

* The smell of putrid garlick emitted by iron and steel during its solution in acids, appears to depend immediately on the presence of phosphorus in the metal, of which a portion is dissolved by the hydrogen gas. For this odour is more strong in proportion to the quantity of phosphorus. This observation proves that the proportion of this substance to the iron which contains it cannot be accurately determined by the process of solution in acids which emit hydrogen gas. The sulphureous acid may answer this purpose.

Table of the Quantities of Carburet of Iron, contaminated with Silice, afforded by each of the Steels, converted into Decimal Parts.

Steel, No. 864, small piece	—	—	0,015
No. 864, large piece	—	—	0,013
No. 977	—	—	0,012
No. 1024	—	—	0,015

As it was ascertained by several experiments made upon the carburets of iron extracted from the different kinds of steel, that this substance contains at a medium 0,526 carbone, and 0,474 siliceous iron, it follows:

1. That the 15 thousandth parts of No. 864, small piece, are composed of

Carbone	—	—	0,00789
Siliceous iron	—	—	0,00711

2. That the 0,013th parts of No. 864, large piece, are composed of

Carbone	—	—	0,00683
Siliceous iron	—	—	0,00616

3. That the 0,015th parts of No. 977 are formed of

Carbone	—	—	0,00789
Siliceous iron	—	—	0,00711

4. That the 0,012th parts of No. 1024, are composed of

Carbone	—	—	0,00631
Siliceous iron	—	—	0,00568

I do not think it necessary in this place to ascertain the proportion between the iron and the silice which exists in the carburet of iron, because this earthy substance being to all appearance accidentally in the steel, must vary according to a great number of different circumstances.

The following, however, are the mean results obtained from several experiments on the carburet of iron:

1. Carbone	—	—	0,53
2. Iron	—	—	0,26
3. Silice	—	—	0,21
			1,00

Table of the Quantities of Phosphate of Iron afforded by the four Kinds of Steel.

By comparing the quantities of phosphate of iron obtained from the four kinds of steel here submitted to examination, very remarkable differences appear; as follows:

No. 864, small piece	—	—	0,0210
No. 864, large piece	—	—	0,0504
No. 977	—	—	0,0474
No. 1024	—	—	0,0900

Table of the Quantities of Phosphorus contained in each of the Samples of Steel.

Analysis having shewn me that phosphate of iron is composed of 0,58 of oxide of iron, and 0,42 of phosphoric acid, and this last being formed, according to Lavoisier, of 0,39 phosphorus, and 0,61 oxygene, it follows that the steel

No. 864, small piece, contained phosphorus	—	0,00345
No. 864, large piece	—	0,00827
No. 977	—	0,00791
No. 1024	—	0,01580

I cannot venture, however, to affirm that there really exists so great a difference between the quantities of phosphorus contained in each of these pieces of steel as is here given by experiment. For so little is required to change the results in this respect, particularly when small quantities are subjected to trial, that two or three thousandth parts may soon be obtained either way. Nevertheless, I cannot suppose, that, with the same processes and equal care, the difference between the maximum and minimum of these quantities will prove to be the consequence of uncertainty in the methods of analysis.

A Synoptic Table of the Proportions of Principles contained in the four Kinds of Steel before examined.

Steel, No. 864, small piece	1. Carbone	—	—	0,00789
	2. Silice	—	—	0,00315
	3. Phosphorus	—	—	0,00345
	4. Iron	—	—	0,98551
				1,00000
— No. 864, large piece	1. Carbone	—	—	0,00683
	2. Silice	—	—	0,00273
	3. Phosphorus	—	—	0,00827
	4. Iron	—	—	0,98217
				1,00000
— No. 977	1. Carbone	—	—	0,00789
	2. Silice	—	—	0,00315
	3. Phosphorus	—	—	0,00791
	4. Iron	—	—	0,98105
				1,00000
— No. 1024	1. Carbone	—	—	0,00631
	2. Silice	—	—	0,00252
	3. Phosphorus	—	—	0,01520
	4. Iron	—	—	0,97597
				1,00000

SECTION VII.

Reflections on the Causes of the variable Quantities of Carburet of Iron afforded by Steel dissolved in the Sulphuric Acid.

MANY circumstances may influence the carburet of iron by the same steel; and it is not easy to obtain equal masses of this substance, in two different experiments, from steel of the same nature.

Bergman obtained from 0,002 to 0,008 of carburet of iron from different steels. Is it probable that there was really so great a difference in the proportions of a substance to which the steels owe their qualities, without there having been likewise a very sensible difference in their usual properties?

Among the causes of uncertainty which may occasion the results to vary, we may principally remark the following: 1. The greater or less concentration of the acid employed to dissolve the iron. 2. Its mixture with water at or before the time of the experiment. 3. The more or less considerable division of the metal. 4. The length of time during which the solution is effected. 5. The time during which the deposition of carburet remains in contact with the liquor after the solution is effected.

1. It is evident that the more concentrated the sulphuric acid, the more rapid the solution, and the greater the quantity of caloric developed in a given time.

Now experiment proves that the solution of carbone by hydrogen is in the direct proportion of the caloric which penetrates it, or at least that it follows some progressive ratio of increase with the temperature. It follows, therefore, that the more elevated the temperature during the solution of steel in acids, the less of carbone will remain. It also follows that it is advantageous not to use an acid too concentrated:

2. As the too great concentration of the acid is inconvenient for the solution of steel from which the carburet is to be extracted, it is also equally inconvenient to dilute it too much. It is in fact known, that when acids are too much diluted with water, they exert a less affinity on other bodies; and this is the case with respect to very diluted sulphuric acid, and steel in a high state of division. The solution then takes place very slowly; part of the steel decomposes the water by its own attractive power, and passes to the state of the black oxide, which cannot be dissolved by diluted vitriolic acid. Hence, instead of carburet of iron, the residue consists of a mixture of this substance with the black oxide of the metal.

3. The duration of the experiment is likewise of considerable importance. It may arise from two causes. The first is the concentration of the acid. In this case, the sulphate of iron which is formed absorbs for its crystallization the small quantity of water which remains, with which it falls down, and entirely stops the solution. The second is the too considerable weakness of the acid, the particles of which being separated too far from those of the metal by the water which retains them, cannot in this case act but very slowly.

I am well convinced that whenever the carburet of iron remains in contact with the metallic solution, particularly when it contains disengaged acid, it is subject to remarkable changes. Its black colour becomes yellow, or yellowish grey, and it diminishes in volume by the actual loss of part of its mass. From these observations it follows, that in order to obtain

obtain the greatest possible quantity of carburet of iron from any kind of steel, the acid must be neither too much nor too little concentrated. I have observed that the best proportion between the acid and the water was one of the former to five of the latter, and that coarse filings or turnings of steel were preferable to fine filings or pieces of considerable size.

Most of the inconveniences here mentioned do not exist with regard to the muriatic acid, at least in so evident a manner; because it is never so concentrated as the sulphuric acid, has a stronger affinity for the oxide of iron, and forms with this metal a very soluble salt. But it cannot be employed to determine, in one and the same operation, the proportions of carburet of iron and of phosphorus; because it decomposes the phosphate of iron, and renders it necessary to use a caustic alkali to precipitate it by double affinity, which may be attended with some inconvenience.

The method we have proposed to determine the quantity of carbone contained in steel, is therefore preferable to this in every respect.

SECTION VIII.

Reflections on the Means proposed by Bergman to discover the Presence of Phosphorus in Iron and Steel.

BERGMAN, in his Dissertation on the Analysis of Iron and Steel, does not speak of the presence of phosphorus or siderite, which he found only in cold short iron.

It is nevertheless very unlikely, that, among the very numerous samples of iron and steel which he examined, there should have been none which contained phosphorus, since the four steels which constitute the subject of this memoir all presented quantities very considerable, though they appeared of sufficiently good quality for the uses in the arts. I suspect, therefore, that this chemist's not having found it was owing to a want of attention to the probability of its existence, or else, that the means he made use of were not adapted to detect its presence.

The following is the method he proposes for this purpose, in his Dissertation on the cause of the brittleness of cold short iron:

He takes a bottle A, of the capacity of about 12 cubic inches, into which he puts 16 loths of crude iron, which affords the cold short iron. Upon the iron he pours six cubic inches of distilled water, and half an inch of concentrated sulphuric acid. As soon as the effervescence is ended, he filters the liquor, and receives it in another bottle B, of the same contents as the first. He washes the residue in the bottle A, until the filtration fills the bottle B. This being done, it is observed that the fluid in B, which at first was clear, becomes turbid and white. The powder does not settle until after several hours. He pours on the iron which remained in the bottle A a second quantity of water and acid equal to the first; when the effervescence is over, he pours the liquid into a third bottle C. He repeats this manœuvre a third and fourth time, &c. and collects the liquors in the vessels marked D, E, &c. The phenomena were the same in each of these operations; but the fifth solution in F remained clear for several weeks, at the end of which it formed a slight cloud. The sixth solution deposited nothing in the same space of time, though much iron remained undissolved.

I repeated this process on the steels treated in this memoir, and did not discover the least trace of phosphate of iron, though they would have afforded considerable quantities by the method I have before proposed. This justifies the assertion that Bergman did not find this substance in the different irons he analysed, because his method was insufficient.

I prove its insufficiency as follows:—Iron and phosphorus united together, and brought into contact with acidulated water, both tend to unite with oxygen by virtue of a predisposing attraction between the acid which is added, and that which is formed, towards the oxide of iron, whence the result is sulphate and phosphate of iron.

This last is insoluble in water; but it has an affinity for acids, by virtue of which it combines, and becomes soluble in water. This affinity of the phosphate of iron for acids is sufficient to impede and even suspend the solution of new quantities of iron; so that it is not without difficulty, and after a length of time, that the phosphate of iron can be completely separated from the sulphuric acid by means of metallic iron.

It happens therefore in every case when iron or steel which contains phosphorus is dissolved in an acid, that the acid divides itself into two parts, one of which unites with the oxide of pure iron, and the other with the phosphate of iron as it is formed; and that when the acid is saturated by the oxide of iron and the phosphate of iron, the solution is almost totally stopped, though the liquor reddens blue vegetable colours. It may be hence inferred, that the sulphuric acid, united with the phosphate of iron, no longer exerts an equal power on the oxide of iron; and that the metal does not decompose the water, nor unite to the oxygen of this substance but by its own peculiar force, which occasions a retardation or even a complete suspension of the solution. Now, if in such a solution the phosphate of iron should be small in quantity, no sign of this metallic salt will appear, even by the addition of a great quantity of water: and this is what happens in the process of Bergman; whereas, by adding an alkaline carbonate to this solution until it ceases to effervesce, the alkali unites with the sulphuric acid; and the phosphate of iron, whatever may be its quantity, falls down in the form of a white powder.

SECTION IX.

Reflections on the Insufficiency of the Methods hitherto proposed to discover and separate Manganese from Iron.

BERGMAN is the first who found manganese in iron; and almost every kind of iron which he examined afforded quantities more or less considerable. The maximum of this quantity in steel, according to this chemist, amounts to 0.39, and the minimum to 0.005, a latitude extremely great, and which appears scarcely probable, as we shall proceed to shew.

In order to detect manganese in iron or steel, the professor of Upsal proposes two methods. The first, having for its object to ascertain the presence of this metal, and being speedy, cheap, and easy to be performed, may be called explorative. It consists in throwing into five parts of fused nitrate of pot-ash, one part of filings of iron or steel, and to bring the mass into strong fusion after the detonation has taken place. If the crucible when cold exhibits towards its superior edge a vitreous circle of a greenish blue colour, it is, according to him, a sure sign of the existence of manganese in the iron. The other method may be called

called determinative, because it is not used until it has previously been ascertained that the iron contains manganese, and that nothing remains but to find its quantity. It is performed by dissolving in the nitric acid a known weight of iron or of steel, containing manganese, and, after having evaporated the solution, by strongly calcining the ferruginous residue, which is treated with more diluted nitric acid, into which a certain quantity of sugar has been put. By this means (says Bergman) the manganese will be dissolved, and the oxide of iron will remain untouched.

These processes, carefully repeated, did not afford me the same results. I must even say that they are false, and capable of leading to erroneous conclusions. In fact, with regard to the first, it is known that nitrate of pot-ash, decomposed in a crucible of pure earth or fine silver, leaves the pot-ash in the form of a mass of a greenish or blueish colour. It is in vain to object, that this colour is owing to the manganese contained in the pot-ash of commerce; for this alkali, when purified by alcohol, presents the same phenomena. And even supposing it to be a certain sign of the presence of manganese, it would also prove a source of error, because the nitrate of pot-ash, and pot-ash alone, exhibit this colour by fusion.

Pure iron itself might produce deception in this respect; for, when it has been oxidized by the nitrate of pot-ash, it combines by the assistance of the alkali with the siliceous matter of the crucible, and forms a glass which has often a greenish colour. Common crucibles also are not without inconvenience; for they may contain the oxide of manganese, which is well known to be often mixed with earthy matters.

The second method of Bergman is attended with still more danger of error than the first, by fixing the opinion of the existence of manganese, and producing a decision with regard to its supposed quantity.

Bergman, who was persuaded that the nitric acid mixed with sugar does not dissolve (the oxide of) iron, must frequently have attributed appearances to manganese which really arose from iron only; for we have proved, that the vegetable acid which is formed in this operation dissolves a considerable quantity.

III.

Extract of a Letter from Mr. HUMBOLDT to Mr. BLUMENBACH, containing new Experiments on the Irritation caused by the Metals with Respect to their different Impressions on the Organs of Animals.*

MR. HUMBOLDT is one of the philosophers who has made the most numerous observations on the phenomenon discovered by Galvani concerning the irritability produced by the contact of different metals with the parts of animals in which the principle of life is apparently extinguished. As long ago as the year 1795 he observed that the animal irritability was augmented by the oxygenated muriatic acid. Not having discontinued his attention to this object, the perusal of the physiological writings of Reil, and his correspondence with Scarpa and Volta, afforded him indications for new enquiries, of which he has occasionally had the courage to make himself the subject.

* This Letter forms part of Gren's (German) Journal of Natural Philosophy for the month of October last. The extract was read to the Institute of France, by Citizen Guyton.

"In a conversation," says he, "with M. Scarpa, at Pavia, on the effects which Galvanism produced upon myself, nothing surprised him more than the appearance of a lymphatic and ferous humour on my back. 'What can be the nature,' said he, 'of this stimulant, which in a few instants changes the nature of the vessels to such a degree as to cause them to prepare humours, which, the instant they touch the epidermis, excite inflammation, and mark their course by a redness which lasts for whole hours?'" M. Humboldt promised to repeat the experiment; and the account he gives of the facts constitutes one of the most interesting articles in his letter.

For this purpose he caused two blistering plasters to be applied on the deltoid muscle of both shoulders. When the left blister was opened, a liquor flowed out which left no other appearance on the skin than a slight varnish, which disappeared by washing. The wound was afterwards left to dry up: this precaution was necessary, in order that the acrid humour which the Galvanic irritation would produce might not be attributed to the idiosyncrasis of the vessels. This painful operation was scarcely commenced on the wound, by the application of zinc and silver, before the ferous humour was discharged in abundance: its colour became visibly dark in a few seconds, and left on the parts of the skin where it passed traces of a brown inflamed red. This humour having descended towards the pit of the stomach, and stopped there, caused a redness of more than an inch in surface. The humour, when traced along the epidermis, left stains, which after having been washed appeared of a bluish red. The inflamed places having been imprudently washed with cold water, increased so much in colour and extent, that Mr. Humboldt, as well as his physician Dr. Schallerer, who assisted at these experiments, entertained some apprehension for the consequences.

Mr. Humboldt has not undertaken to determine the nature of the fluid which produces such astonishing effects; but he applies himself to circumscribe the phenomena in the real circumstances which produce them. He judiciously varies the preparations, and carefully notes all the results; being persuaded that the cause of Galvanism cannot be explored with success, but by observing the proportions in which the chain of metals either irritates or has no effect: and to extend still more this vast field of observation, he employs various means to raise or diminish the irritable capacity of the animal organs.

What is the sensation which the Galvanic irritation produces? Mr. Humboldt has discussed this question. "No one (says he) can speak more decidedly on this subject than myself, having made several experiments on my own person, the seat of which, in some instances, was the socket of a tooth which I had caused to be extracted; in others, certain wounds which I made in my hand; and in others, the excoriations produced by four blistering plasters." The following is his answer:

The Galvanic irritation is always painful, and the more so in proportion as the irritated part is more injured, and the time of irritation more prolonged. The first strokes are felt but slightly; the five or six following are much more sensible, and even scarcely to be endured, until the irritated nerve becomes insensible from continued stimulus. The sensation does not at all resemble that which is caused by the electric commotion and the electric bath; it is a peculiar kind of pain, which is neither sharp, pungent, penetrating, nor by intermissions, like that which is caused by the electric fluid. We may distinguish a violent stroke, a regular pressure, accompanied by an unintermitting glow, which is incomparably more active when

the wound is covered with a plate of silver, and irritated by a rod of zinc, than when the plate of zinc is placed on the wound, and the silver pincers are used to establish the communication.

When the communication is made by the contact of the epidermis, it produces no effect; it appears to insulate like glass, when interposed between the wound and the metal: but if the skin be removed, by making two wounds at eight inches distance, and a plate of zinc be placed on one of them, and on the other a leg of a frog prepared, this last is seen to contract itself when it communicates with the zinc by the silver wire; which proves that the Galvanic fluid then passes beneath the epidermis.

This fluid produced in some circumstances a very sensible acid taste. The two wounds of Mr. Humboldt having been covered, one with silver, the other with zinc, an iron wire of several feet in length, attached to the zinc, was conveyed between his upper lip and the spongy substance of the teeth, and thence to the tongue of another person. When the iron wire was made to touch the silver, a strong contraction of the scapular muscle took place, and at the same instant the person whose tongue formed part of the chain of communication perceived the sensation of acidity. There are also cases in which the fluid acts on the organs of taste without producing any sensible effect on the organs of motion: such is that where the epidermis serves as the conductor from zinc to the frog; for there is not then any contraction, but merely an acid taste on the tongue.

The author, having learned from Mr. Volta that he employed the solution of pot-ash (*solum tartari per deliquium*) in order to augment the conducting power, availed himself successfully of this means to raise the capacity of the animal organs. He moistened one of his wounds with this liquor, which produced little pain; but the Galvanic irritation was more violent, and accompanied with more heat; sparks appeared and disappeared before his eyes; the tongue moistened with the same distinctly perceived the acid sensation, although the communication was established only between zinc and zinc. The thigh of the frog, moistened with the alkaline solution and laid upon a plate of glass, without touching either metal or carbonic matter, fell of itself into violent convulsions, the antagonist muscles of the legs and toes being incessantly agitated. Irritability has been re-established by this application in the animal parts, where it had been extinguished by warm solutions of the oxide of arsenic. Lastly, the irritation (which does not commonly take place when the nerve and the muscle are armed with the same metal, the different metals being between the coatings) becomes manifest after this preparation; which seems to indicate that the alkali not only irritates the nerve, but likewise adds to its irritability.

The author applied this method to amphibious animals, which he roused from their winter's sleep, and in which he perceived a peculiar symptom of irritability.

These observations led him to distinguish two states of the animal organ. The first, of irritability naturally or artificially raised or excited; the second, irritability in a less degree. These two states, which he calls *positive* and *negative*, are merely, as he remarks, different degrees, and not phenomena absolutely distinct from each other.

In individuals naturally sensible, the effects produced by alkaline solutions, by the oxygenated muriatic acid, by the solution of oxide of arsenic, are very rarely of the same intensity.

In the case of increased irritability, muscular motions are observed without metal or carbonic

carbonic matter. They may be obtained with metals, though without communication between the nerve and the muscle; that is to say, without the regular connection or chain. They may be also obtained by forming the chain of similar metals.

Let the crural nerve of an animal naturally tenacious of life be placed upon glass. Let a small piece of fresh muscular flesh be fixed on a stick of sealing-wax, and then brought into contact with the crural muscle. The result will be a violent convulsion at the instant when the chain of communication is completed. The same thing happens if, instead of the small piece of muscular flesh, a detached piece of the crural nerve be fixed on the stick of sealing-wax. The connection is therefore formed of two things, nerve and muscular fibre. How in this simple case can the fluid which passes from the nerve into the muscle cause it to be contracted? Mr. Humboldt thinks that it becomes stimulant, merely because it returns from the nerve into the nerve by a foreign animal matter; that is to say, not organically connected with the nerve.

The disparity of the metals forming the chain has hitherto appeared as a necessary condition to produce Galvanic irritation. This hypothesis, however, is overturned by the experiments of Mr. Humboldt. If it be true that, in the state of less irritability, there is very rarely contraction with similar metals (as Volta affirms, contrary to Aldini), this circumstance becomes indifferent in the case of increased irritability. Mr. Humboldt put into a china cup some mercury exactly purified; he placed the whole near a warm stove, in order that the entire mass might assume an equal temperature: the surface was clear, without the appearance of oxidation, humidity, or dust. A thigh of a frog, prepared in such a manner that a crural nerve and a bundle of muscular fibres of the same length hung down separately, was suspended by two silken threads above the mercury. When the nerve alone touched the surface of the metal, no irritation was manifested; but as soon as the muscular bundle and the nerve touched the mercury together, they fell into convulsions so brisk that the skin was extended as in an attack of tetanus.

We ought not to be surprised at the precaution here taken by Mr. Humboldt to heat the mercury. This is required in consequence of the opinion which he announces, that the parity of the metals does not depend on the homogeneity of their chemical constituent parts, but of their heat, polish, hardness, and form.

Gold, placed between two armatures of zinc, produces irritation only when the gold is moistened by some volatile fluid, or by the moisture of respiration.

Lastly, Mr. Humboldt has attempted to include all the cases in the following formulæ:

1. In the State of increased Irritability.

Positive cases.	{	Frog—muscular flesh.
		Frog—zinc—zinc.
		Frog—zinc—muscular flesh—silver.
		Frog—zinc—silver—zinc.
		Frog—muscular flesh—silver—zinc.
		Frog—zinc—muscular flesh—silver—muscular flesh—zinc.

2. In the State of diminished Irritability.

Positive cases.	{	Frog—zinc—silver.
		Frog—zinc—muscular flesh—silver—zinc.
		Frog—zinc—muscular flesh—silver—muscular flesh—silver—zinc.

Negative cases. { Frog—zinc—zinc.
 { Frog—zinc—muscular flesh—silver.
 { Frog—zinc—muscular flesh—silver—zinc.

Mr. Humboldt finishes this letter by some observations which he has collected in the course of his experiments on the *sthenic* or *asthenic* virtue of chemical agents; that is to say, their energy or their inefficacy to produce irritation. Alkalis appear to be to the sensible fibres what acids are to muscular groups. The muriatic acid augments the irritability of the muscle while it extinguishes that of the nerves, which does not re-appear even after the acid has been saturated with alkali.

By continuing to bathe the nerve with an alkaline solution, an entire atony is at length produced by excess of irritation; but if a few drops of muriatic acid be let fall on the part, the irritability is re-established.

A thigh of a frog, irritated even to total relaxation by a warm solution of oxide of arsenic, has exhibited new convulsions, after having been immersed for two minutes in a solution of pot-ash.

The *sthenic* virtue of the oxygenated muriatic acid is not less remarkable. Thighs of frogs naturally flaccid, and weakened still more by the Galvanic process for seven hours, which afforded no sign of motion when silver served as a conductor between zinc and the nerve, exhibited violent contractions when the nerve was moistened with oxygenated muriatic acid. The author refers to this subject the experiment which he published in 1793, in his *Flora Fribergensis*, by which it is ascertained that ordinary muriatic acid retards the germination of plants, but that oxygenated muriatic acid had caused a plant to germinate in seven hours, which required thirty-eight in pure water in order to arrive at the same development. This fact appears to him to indicate some relation between the vegetable and animal organization.

A judgment may be formed from this extract of the number of important facts contained in this letter, and of the interest they will excite when they shall be collected, arranged, and amplified, in the large work which the author is preparing.

IV.

*Useful Notices respecting various Objects *.—Methods of closing wide-mouthed Vessels—Preservation of Gunpowder—Granulation of Shot—Precipitation of Magnesia.*

1. *Methods of closing wide-mouthed Vessels.*

THE means of closing the apertures of large jars and other similar vessels, so as to render them air-tight, has long presented, to persons who are engaged in philosophical pursuits, an object of some interest. Those in particular who are in the habit of preserving animal substances in spirit for anatomical or physiological cabinets, find that, notwithstanding all the care which can be taken, the evaporation of the liquor in which they are immersed is a subject which requires constant attention. Perhaps nothing with which we are acquainted is

* The whole of the present article was received from my anonymous correspondent, J. F—:—:—r, whose former communications have, no doubt, engaged the attention of the reader.

more convenient and proper for stopping small circular apertures, whose interior surfaces are smooth enough to admit of their use, than common phial-corks: for a certain time at least, where the contents of the vessel are not corrosive, they fit more closely than most of the glass stopples which are used for this purpose. Where however the area of the aperture is considerable, the porosity of cork and bladders, the necessity of using vessels whose mouths are elliptical, and the unavoidable irregularities in the form of the ground glass covers, which are besides of considerable expence, are circumstances which render the solution of this problem less easy than might be wished. The closeness of texture which characterises metallic substances seems to afford the best means of removing these difficulties; and a quantity of an amalgam of tin is accordingly in some museums spread over the edges of the covers, by which means the mischief is in a good measure prevented: but perhaps no method of effecting this end is so applicable to general purposes, as that which consists in the use of tin foil, which is also in frequent practice. It may be applied in two ways: a piece of bladder, soaked in warm water, having been stretched tightly over the mouth of the jar, and tied, a piece of the thickest tinfoil, previously examined by interposing it between the eye and the sun, in order to detect the small fissures which are frequently found in it, is to be laid smoothly over it with the palm of the hand, without stretching, and, that being also tied, a second piece of bladder again stretched over it: or, where the vessel may require to be sometimes opened, the foil may be laid smoothly over the surface of a bung, and, a piece of bladder being stretched over it, the whole applied in the usual way.

The following seems to be another effectual method of closing a jar:—Let *ab* (Plate XII. Fig. 1.) be a section of its mouth; *cd*, that of a circular rim of tin, *A*, an inch high, whose internal diameter is half an inch larger than the exterior diameter of the mouth of the vessel; *ef*, that of a common tin cover, *B*, whose diameter is a quarter of an inch less than that of the tin rim, *A*. The rim being cemented round the neck of the jar with sealing-wax, or the common electrical cement, and the interval between it and the neck filled to the depth of two-tenths or a quarter of an inch, with olive-oil*, it is clear, that if the jar be placed horizontally, and the cover *B* put on, it becomes hermetically sealed, except in the event of an elastic fluid making its way through the oil. The only care necessary will be, not to put so much oil into the tin rim as to occasion it either to flow over or into the jar, on a change taking place in the pressure of the atmosphere. The mouth of the vessel should also be stopped, to prevent any of the oil from being thrown into it on removing the cover.

In the laboratory, a thousand uses of these modes of preserving different substances must occur; and it seems not improbable that, if they were extended to the purposes of ordinary life, and glass vessels substituted for the porous earthen-ware at present in use, several articles of culinary preparation, such as pickles, preserved fruits, potted meats, and the like, would receive less injury from the effects of time than they are now found to do.

* Lavoisier used mercury for a somewhat similar purpose, but its depth was considerably greater. It seems, from some experiments of Priestley's, in which he found his gases contaminated with atmospheric air, though the tubes had been immersed to the depth of an inch or more in this fluid, that, in a thin stratum, oil would be more effectual. F.

If this construction, as I understand it, be such as to admit the oil into contact with the resinous cement, this last will soon become soft by the process of solution. To prevent this effect, I suppose it would be convenient to fasten the rim by a mixture of plaster of Paris and white of egg. N.

2. *Preservation of Gunpowder.*

GUNPOWDER, by reason of the nitre which enters into its composition, having been partially deprived of its water of crystallization, and the known attraction of charcoal for humidity, is always somewhat disposed to deliquesce; and although it does not actually liquefy, or become unfit for some of the purposes to which it is applicable, yet, for those of the sportsman, to whom the quickness of its communication is of the highest consequence, it is generally in a state very inferior to that in which it would be found, if a greater degree of care was taken in its preservation. It is only when it has received but a very slight injury from damp, that the mischief is capable of a remedy: when once it has become at all concentered, drying it will no longer restore its power: the nitre will be found, on examination with a magnifier, to have crystallized, and the strength and quickness of the powder are considerably and permanently impaired; probably even before this symptom has appeared. It is evident that no vessel is sufficiently close to prevent this circumstance from taking place, but such as is perfectly air-tight. There cannot perhaps be a much stronger proof of the insufficiency of the packages in general use for this purpose, than the opinion of a considerable dealer in this article, to whom the matter was lately mentioned. He said he was convinced that powder would be found to "give" in some states of the weather, though the vessel which contained it was ever so close: a notion which may perhaps have contributed to prevent the adoption of more effectual means. He added, that it is found to do so in the tin canisters, as much as when packed in brown paper. The remedy is however extremely easy. Nothing more is necessary, than to cut off the communication with the atmosphere: any vessel in which salt of tartar can be preserved dry, will of course keep gunpowder in the same state of perfection as when first enclosed. For a quantity not exceeding a few pounds, which is not intended to be frequently removed from place to place, common ten or twelve-ounce phials answer extremely well; and if half a dozen of them be put into a case, there cannot perhaps be a more convenient magazine. They should be filled as full as possible, and the powder well corked up at the mills, the corks being tied over with bladder and tinfoil. As, however, there might be some danger of explosion from the accidental fracture of one of these, if this method were to be adopted for large quantities, it would in that case be necessary to use some other material than glass; and if, instead of the slider now inserted into the tin canisters, a turned brass or pewter neck, like that of a common phial, and capable of being likewise stopped with a small cork, were soldered into the top, they would also answer as well. A projection would perhaps render them inconvenient for package; and it would therefore be proper that the neck should be sunk into the top; and, in order to get out the contents, that it should be let into a semi-cylindrical hollow in the side of the canister. When corked up, the top of the cork might be cut off, and the whole aperture covered with a plaster of thick drying paint, or wax and turpentine, spread on a piece of tinfoil. None of the flasks, the best of which are those of copper or tin, are fit for preserving the powder longer than whilst they are in use, during which the charger should be kept corked; a precaution the effects of which will be found considerable.

There are some, perhaps, who may not conceive these remarks to be very materially conducive to the general interests of philosophy; but he to whom it has frequently happened to miss an excellent cross shot, from his powder hanging fire—*quaque ipse miserrima vidi*—will scarcely consider this as the least important article in the September Journal.

3. *Granulation of Shot.*

THE manufacture of common fowling shot consists merely in causing the fused metal to fall in equal spherical drops into water. The lead is melted with the addition of a small proportion of arsenic, which, being reduced to a metallic state, by means of grease stirred in during the fusion, renders it less fluid. An oblong shallow vessel of iron, perhaps 10 inches wide, 14 long, and $2\frac{1}{2}$ deep, called a *card*, whose bottom is pierced with holes proportionate to the intended size of the shot, is placed at the height of from one to three inches, over the surface of a tub of water, covered with a thin film of oil. The card is previously heated to the temperature of the metal by immersing it in the cauldron; and a stratum of soft dross or scoriae, which are found on the surface of the fused alloy, is then placed on its perforated bottom, and, being slightly pressed down with the ladle, forms a kind of filter, which partly chokes up the apertures, and prevents the metal from flowing through them in continuous streams. The fused metal is then poured by ladlefuls into this vessel, and appears notwithstanding to run through it with considerable velocity; so that it seems difficult to believe that it falls in separate drops, till convinced by taking up a quantity of shot from the bottom of the water.

The shot thus made is not without considerable imperfections. The exterior coat of the lower part of the drop becoming suddenly fixed by the contact of the water, its superior portion, which is still liquid, as it also cools and contracts, necessarily pits, like the surface of metal in the channel of a mould, so that the greater part of the shot are somewhat hollow and of an irregular form; consequently too light for the purpose to which they are destined, and liable to unequal resistance in their passage through the air. These defects are remedied in the patent shot, the manufacture of which differs only from that of the preceding kind in the addition of a larger portion of arsenic, which varies according to the quality of the lead; *in dropping it from such a height that it becomes solid before it enters the water*, which is from 40 to 100 feet; and in some subsequent operations, which are as follows: It is first dried and sifted. It is then *boarded*, which consists in scattering it on several polished slabs or trays of hard wood, with rims, in the form of a Π , except that the sides converge towards the lower part, to which a slight inclination and alternate motion in their own planes are given by boys employed in the manufacture. The shot whose form is imperfect are detected by the sluggishness of their motion, and remain behind, whilst the others roll off from the board. The last operation is the polishing; which is performed by agitating it, with the addition of a very small quantity of black lead, not exceeding two spoonfuls to a ton, in an iron vessel, turning on an horizontal axis, like a barrel-churn. It does not appear that any higher degree of perfection than that which is thus attained remains to be desired. The argentine brilliancy of the shot when newly made, the beautiful accuracy of its form, and the curious instance of inanimate tactics which it presents when scattered on a plate, render it even an agreeable object of contemplation.

4. *Precipitation of Magnesia.*

THE most striking characters of common magnesia, and those which are most relied on by a purchaser who has not the means of analysis at hand, as indicative of its purity, are its levity and impalpability: it is therefore a matter of some importance to those who deal in
this

this article, that it be made to possess these qualities in as high a degree as is possible. It had long since occurred to the writer of this, that the effects which are attributed, by all who treat of the subject, to a very small quantity of other earths in the alkali which is used in the precipitation, were somewhat disproportionate to the assigned cause, and that a part of them were probably rather owing to a deficiency of carbonic acid *. An accidental piece of information which he received lately from a practical man, that magnesia was always to be obtained "beautifully light" by the addition of a small proportion of sal sodæ to the vegetable alkali employed, and a very loose experiment which he has since made with a view to this object, appear to corroborate such an idea. The magnesia contained in four ounces of Epsom-salt was precipitated with a filtered solution of common pearl-ash, washed, dried, and a portion of it then re-dissolved by vitriolic acid, and again precipitated with the same alkali, with the addition of one-fourth of carbonate of soda. The powder was certainly more light and impalpable after the second precipitation. An addition of carbonic acid to the alkaline solution employed, will probably operate in two ways: it will not only render the magnesia lighter, but in some degree actually purer, by precipitating the aluminous and siliceous earths before held in solution by the pot-ash in a more caustic state. In this respect, and in this only, perhaps, if a sufficiently small quantity of water be used, the aqua kali of the present Pharmacopœia is inferior to the oil of tartar per deliquium of the old ones. There is possibly a limit to the proportion of this ingredient, which can be admitted into the process with a due regard to economy; perfectly neutralised carbonate of magnesia being by no means insoluble. If an alkali, in an highly effervescent state, be added to a weak solution of any magnesian salt, it is well known that no precipitation whatever will take place. What remained in the supernatant liquor might, however, if thought of sufficient value, be afterwards precipitated with a caustic alkali, and reserved for calcination; or indeed would of itself subside during the subsequent evaporation for obtaining the vitriolated tartar. The best process in all respects may be easily ascertained by experiment, and the matter appears to deserve it.

V.

On the Elastic Fluid contained in the Air-Vessels of Fish.

DR. Francis Rigby Brodbelt, of Jamaica, in a letter to Dr. Duncan †, gives the following account of some observations and experiments which he has made on the gas contained in the air-bladder of the sword-fish:

"I will relate to you a few experiments which I made during my passage to this island. I had often wished to determine what is the nature of the gas which is contained in the air-

* I find, on examination of the common magnesia with a deep magnifier, that its levity proceeds from an actual radiated crystallization, like that of snow: its form may be advantageously seen when just separating into flocks in a glass tube. Hence perhaps it would be improved by the addition of the alkali in very small portions, at intervals. Mere washing, by destroying the ramifications of the crystals, considerably augments the specific gravity of magnesia: an effect which would perhaps be best prevented by performing this operation carefully with distilled water, previously boiled on a portion of it more than sufficient for its saturation. F.

† Annals of Medicine, by Drs. Duncan, for 1796, p. 393.

bladder of fish; and I was perhaps prevented from finding it out, by hearing Dr. Monro in his lectures say, it was natural to suppose it fixed air. However, although this authority prevented me from putting it to the test of experiment for some time, yet one day, on our voyage, having caught a very large sword-fish, I collected the contents of all the air-bladders; for in that fish the bladder appeared divided into innumerable cells, which had no communication with each other. They afforded so much air that I collected a quart bottle full. My surprise was great to find that the gas contained was oxygene. A flame was brightened, an ignited stick was made to re-ignite, and it was so strong and pure, that the common experiment of a piece of steel wire, heated and put into it, succeeded well, and threw out a most vivid light when melting. I have committed to writing my thoughts on this subject at greater length, and I wish to infer that this pure air is to serve the purposes of life when the animal is far below the surface of the water."

The preceding discovery of oxygene is, I believe, perfectly new. Dr. Priestley, in his *Experiments in Natural Philosophy* abridged and methodized, vol. ii. page 462, mentions the commencement of a course of experiments on the state of the air which is contained in the bladders of fishes. He remarks, that when these are taken out of the fish, the air cannot be got from them by pressure through any existing aperture, but that he was always obliged to cut or burst them. The first time that it occurred to him to examine the air contained in these bladders, he found it in a great number of them to be not at all affected by nitrous air. But at another time he found air from the same kind of fish, namely, roaches, to be slightly affected by that re-agent. It appears, therefore, from these trials, that he seldom met with oxygene, and then in small quantity; but what the other portion of air might consist of was not ascertained by his experiments.

Fourcroy afterwards made experiments on the air contained in the air-vessel of the carp, which, at certain seasons, he affirms, may be had very cheap and in abundance at Paris. It was perfectly pure azotic gas, for the most part, though sometimes it contained a small quantity of carbonic acid gas. He thinks, from the nature of this fluid, that the air in the bladders of fishes is produced in the stomach. (*Ann. de Chim.* I. 47.) The observations of Dr. Brodbelt seem to render this general conclusion at least doubtful.

VI.

Account of certain remarkable Changes of Colour and Direction of the Clouds during a Thunder-Storm.

AMONG other circumstances enumerated by Dr. Priestley in the description of the clouds, in a thunder-storm, in his *History of Electricity*, mention is made of a certain luminous appearance, evidently independent of solar reflection. I have always supposed this expression to denote the opaque whiteness of the upper or arched outline of certain thunder-clouds, contrasted with others apparently in contact with them, but of a dull leaden hue; and accordingly I was disposed to conclude that the whole was an optical delusion, arising from the position of the spectator, who imagined, though falsely, that the latter clouds were as much exposed to the sun's direct light as the former. But the storm which happened on Sunday morning, the 30th of July last, exhibited facts which seem to shew that the transition of electricity may cause the clouds to emit a steady permanent light, very different from the sudden flash called lightning.

I was called at five o'clock in the morning. The sky was then covered with clouds, not very dense except to the south, and flying with great rapidity to the W. by S. or W. S. W. It lightened very frequently in the N. W. and S. W. quarters, by doubled and trebled flashes of a bright illumination (for the actual flash was not seen), with very loud thunder, usually at the interval of 11 or 12 seconds after the flash. The lower prominences, or ragged extremities of the clouds, were constantly tinged with red, and I was informed that they had been very much redder before I got up.

At about ten minutes after five, no rain having fallen, but a few heavy drops, a sudden darkness came on, and the dust rose in Newman-street, where I reside, beginning at the south end, about 250 yards distant from my house, and proceeding to the north. It was very dense, and rose to the height of about sixty feet by estimate, which is much higher than the houses in the street. At about a quarter past five, the darkness being then greatest, the houses on the opposite side of the street appeared as if seen through a deep blue glass, particularly the white stone work above the windows; and upon looking upwards, the clouds were seen of a deep leaden blue colour, and moving swiftly in a direction precisely opposite to that before observed, namely, to the E. by N. or E. N. E. Soon afterwards, the lightning and thunder continuing during the whole of these changes, there fell a heavy shower, which beat against the western face of the house, and the darkness gradually went off. At half past five the clouds were much higher, and moved with a moderate angular motion to the north, while the smoke of a chimney opposite my window was gently driven to the south.

We have yet much to learn concerning the theory of thunder-storms. It is well known by experiments with the electrical doubler*, that almost all bodies possess a certain degree of electrization, which is variable from a considerable number of circumstances. It is also known from Franklin's experiments of the can and chain, as well as from numerous other facts, that the intensity of the electric state will be augmented by diminishing the surfaces of bodies. In this way, as well as from other causes, it is inferred that clouds become highly electrified during the progress of their condensation; so that flashes of lightning pass between them and the earth, and between each other. From other observations and deductions, it has been also rendered highly probable that the long range of clouds in a storm of this nature does serve as a conductor, through which flashes of electric fire are conveyed from one part of the earth to another part in a different state. The facts above described seem uncommon, and, if collated with other more usual events, may afford some instruction concerning this class of phenomena. I do not highly esteem the conjectures which present themselves to me on this occasion; but shall communicate them, because they tend to point out future objects of research.

The singularity of this thunder-storm appears to have arisen from the mass of aqueous vapour having been much too small to afford a favourable communication between the two opposite states of electricity on the surface of the earth. If the mass of clouds be supposed to have been at first near the eastern portion of the earth, and to have become electrified and repelled, they would, on the common principle of bodies in that state, be repelled, and pass swiftly to the western part of the surface to deposit their electricity as soon

* New Experiments on Electricity. By the Rev. A. Bennet, F.R.S. octavo. London.

as they came within the striking distance. In this situation, it may be imagined that the supply or communication might continue to be made by the eastern part of the cloud acting in the manner of a point, while the western part emitted flashes; a supposition which is rendered more probable by the consideration that most thunder-clouds are ragged or pointed on one side, and round or swelled on the other; and also that the disposition of any conductor to receive electricity without explosion is much greater than to give silently, even when the terminations are alike. I conjecture, therefore, that the lightning came from the east, and passed through the clouds to the west; that the posterior extremities of the clouds were illuminated as points usually are; that the electric motion of these low clouds at first caused an easterly wind; that some change in the general state of electricity, or perhaps the mere exhausted state of the clouds, caused them to pass rapidly back to the original reservoir, as in the most common experiments of electricity; that this return produced first a strong eddy in the lower air, which threw up the dust, and afterwards a contrary stream of wind, by which the rain was beaten against the west front of the house. But why the clouds should have been illuminated with a red colour during their western course, and afterwards with blue, does not seem deducible from any facts I know of. It may perhaps be analogous to the colours of the aurora-borealis. The contrary lower current, when the clouds were moving to the north, seems to have been a natural consequence of their remoteness. It is probable that the returning current of the air, which must have been driven before the mass of clouds when they were moving very near the earth, took place entirely in the upper part of the air, where, in that case, there was more room.

VII.

The Method of making excellent Bread without Yeast; as practised at Debretzin in Hungary.*

By ROBERT TOWNSON; LL. D. F. R. S. Edin.

LIGHTER, whiter, and better-flavoured bread than that made here I never ate, nor did I ever see elsewhere such large loaves. Were I not afraid of being accused of taking advantage of the privilege of travellers, I should say they were near half a yard cubed. As this bread is made without yeast, about which such a hue and cry is often raised, and with a substitute which is a dry mass, that may be easily transported, and kept half a year or more, I think it may be of use to my country for me to detail the Debretzin art of making bread. The ferment is thus made: Two good handfuls of hops are boiled in four quarts of water: this is poured upon as much wheaten bran as can be well moistened by it: to this are added four or five pounds of leaven; when this is only warm, the mass is well worked together to mix the different parts. This mass is then put in a warm place for twenty-four hours, and after that it is divided into small pieces, about the size of a hen's egg, or a small orange, which are dried by being placed upon a board, and exposed to a dry air, but not to the sun; when dry, they are laid by for use, and may be kept half a year. This is the ferment, and it may be used in the following manner: For a baking of six large loaves, six good handfuls† of these balls are taken and dissolved in seven or eight quarts of warm

* Travels in Hungary, 4to, London, 1797, page 242.

† I suppose broken into fragments; the balls themselves being too large to be measured by handful. N.

water. This is poured through a sieve into one end of the bread-trough, and three quarts more of warm water are poured through the sieve after it, and what remains in the sieve is well pressed out. This liquor is mixed up with so much flour as to form a mass of the size of a large loaf: this is strewed over with flour, the sieve, with its contents, is put upon it, and then the whole is covered up warm, and left till it has risen enough, and its surface has begun to crack: this forms the leaven. Then fifteen quarts of warm water, in which six handfuls of salt have been dissolved, are poured through the sieve upon it, and the necessary quantity of flour is added, and mixed and kneaded with the leaven: this is covered up warm, and left for about an hour. It is then formed into loaves, which are kept in a warm room half an hour; and after that they are put in the oven, where they remain two or three hours, according to the size. The great advantage of this ferment is, that it may be made in great quantities at a time, and kept for use. Might it not on this account be useful on board of ships, and likewise for armies when in the field?

VIII.

Description and Use of an Eudiometer with Sulphuret of Pot-Ash. By Citizen GURTON.*

NATURAL philosophers and chemists have long been desirous of possessing an eudiometer which might accurately shew the quantity of oxygene mixed in any kind of gas. Citizen Berthollet has clearly shewn, in his late lessons to the Normal School †, that the eudiometer of Scheele, which he with justice considers as the best, has nevertheless great defects; because the absorption requires several hours, and because there is a decomposition of water towards the end, which consequently disengages hydrogen gas, and renders the measure of absorption doubtful.

This consideration induced me to seek a material which might immediately, and with convenience, afford a result more to be depended on than those obtained by nitrous gas, hydrogen gas, phosphorus, and the mixture of sulphur and iron; the only substances which have been, as far as I know, hitherto used or proposed for that purpose.

The sulphuret of pot-ash appeared to me to deserve trial in this respect. I well knew that at the ordinary temperature it is only susceptible of a combination still more slow and insensible than the mixture of sulphur and iron moistened; but I presumed that by raising the temperature, merely by the approach of a small taper, the action of chemical affinity might be so much favoured as to determine rapidly an absorption which in that case would not be affected by any foreign circumstance.

The effect has completely justified my conjecture; so that nothing more remains than to describe the apparatus required to form this eudiometric instrument. I thought that the reversed retort, or inflected receiver (recipient cornu) as I have named it in the article *Air* of the *Encyclopédie Methodique*, I. 706, would unite simplicity, convenience, and every desirable advantage. The experiment was made in the laboratory of the third division of the Polytechnic School, according to the following description:

* *Journal de l'Ecole Polytechnique*, II. 166.

† *Séance des Ecoles Normales*, &c. tom. v. p. 73.

A B, Plate XII, Fig. 2. represents a very small glass retort, with a long neck, its whole capacity being from 12 to 15 centilitres (between seven and nine solid inches English). It must be chosen of such a curvature, that, when the neck is set upright, the bulb may form at its lower part a cavity to retain the matters introduced.

The extremity of the neck of this retort is ground with emery to enter the glass tube CD, which is open at both ends, and about 20 or 25 centimetres in length (eight or nine inches English). The retort then closes the tube in the manner of a ground stopper, and intercepts all external communication*.

A cylindrical glass vessel, F, is provided, of the form of a common jar, in which the glass tube CD may be entirely plunged beneath the level of the water.

Lastly, the sulphuret of pot-ash is prepared and broken into pieces sufficiently small to be introduced into the retort. These are to be enclosed, dry and even hot, in a bottle for use.

These constitute the whole apparatus and preparation of materials.

When it is required to examine an æriform fluid, by separating its respirable part, two or three pieces of the sulphuret, of the size of a pea, are put into the retort. It is then filled with water, taking care to incline it so that all the air may pass out from the bulb. The orifice of the retort is then to be closed, and inverted into the pneumatic tub, in order that the gas proposed for examination may be transferred into it in the usual manner.

By an easy manœuvre of alternately inclining the retort in different directions, all the water is made to flow out of the bulb in which the sulphuret remains.

When this is done, the retort is placed in the vertical situation, and its extremity introduced into the tube of glass CD, which must always be under water. A small lighted taper is then to be placed under the bulb.

To support the retort in its position, the jar is provided with a wooden cover, in which there is a notch to receive it.

The first impression of the heat dilates the gaseous fluid so much that it descends almost to the bottom of the tube, which is disposed expressly for its reception; otherwise the partial escape would prevent an accurate determination of its change of bulk.

But as soon as the sulphuret begins to boil, the water quickly rises, not only in the inferior tube, but likewise in the neck of the retort, notwithstanding the application and even the increase of the heat.

If the fluid be absolutely pure vital air, the absorption is total. In this case, to prevent the rupture of the vessel by too sudden refrigeration, the ascent of the water must be rendered slower, either by removing the taper, or inclining the retort †; which will not pre-

* Citizen Chauffier had before constructed, for eudiometric experiments with phosphorus, an apparatus little different from this, composed of a long tube of a single piece, one end of which is bended and blown into a bulb, and has at *c* a projecting tube, which is closed with a cork, after having caused the water to rise within the tube to about one-third of its height. This instrument may also be applied for experiments with the sulphuret of pot-ash. I must observe, however, that the practice is not so easy as it appears at first sight: besides which, if the projecting tube renders it very convenient for operating on the air of the atmosphere, it is not the same with regard to the other gases, which cannot be introduced but by transferring them. G.

† It does not appear that inclining the retort would diminish the rapidity of ascension. If the perpendicular height were increased, by partly raising the tube above the water, or if the aperture were partly closed, this effect would follow. N.

vent the absorption from continuing while any gas remains which is proper to support combustion.

If the fluid be common air, or vital air mixed with any other gas, the quantity of water which has entered the retort must be accurately measured after the cooling. It represents the volume of air absorbed. Care must be taken to enclose the remaining gas under the same pressure, by plunging the retort to the level of the line at which the enclosed water rests, before the orifice is stopped.

This operation of measuring, which is very easy when measuring vessels are at hand, may be habitually performed by a slip of paper pasted on the neck of the retort, upon which divisions are drawn from observation, and which must be covered with varnish, to defend it from the action of the water.

IX.

Description of an improved Electrometer, in which the Sensibility of the Gold-Leaf is considerably augmented, and the Intensities are distinguished by numerical Graduation.

IT is scarcely to be supposed that any philosopher who is conversant with electricity can be unacquainted with the electrometer of Bennet, in which two pendent slips of gold-leaf are substituted in the place of the pith-balls of Canton, and serve to indicate the nature and quality of very minute intensities of the electric state. There are two particulars in which this excellent instrument appears capable of improvement: the first, to render it portable, without danger to the gold-leaf, and the second, to express its various degrees of electrization by a scale of divisions.

I have reflected much on the probable means of securing the gold-leaf from fracture by carriage, but hitherto with little prospect of success. There was some hope that a single slip of this gold might be preserved in a sheath or box, with its sides very nearly in contact; but when I placed such a slip upon a gilded piece of wood of the same superficial dimensions, to which it was fastened at one end, its flexibility was such that the leaf very readily slid along the surface of the wood, and became full of folds, by inclining the fastened end a very few degrees lower than the other extremity. There was still less immediate expectation that the slips could be actually and repeatedly confined between two leaves or cushions, as in the book of the gold-beaters, without their being broken by occasional agitation. To this, however, my attention will probably be directed when I may again resume this object. In the mean time, I recommend it to other philosophers, as a very desirable improvement in the mineralogical apparatus, and should rejoice to be anticipated by their successful researches.

The weight of one slip of gold-leaf, in the electrometer of Bennet, is about one-six-hundredth part of a grain; but this, as well as the sensibility of the instrument, must vary, not only from the figure and dimensions of the piece, but the nature and thickness of the gold itself*. It seemed, therefore, unnecessary to endeavour to render two of these instruments comparable with each other. All that could be done was, to distinguish the different intensities as shown by the divergencies of the leaf; or, as I have taken it, the distances at

which they strike a pair of insulated metallic bars. In Plate XII. Fig. 3, A represents the insulated metallic cap, from which, at C, depend the two narrow pointed slips of gold-leaf. BB is the glass shade, which serves to support the cap, and defend the leaves from the motion of the surrounding air. DD are two flat radii of brass, which open and shut by means of one common axis, like a pair of compasses. By a contrivance of springs, they are disposed to open when left at liberty; but the micrometer screw E serves to draw a nut, which has two steel bars, with a claw at the end of each, that enters into a correspondent slit, in two small cylindrical pieces, to which the radii are fixed respectively. This apparatus is seen in another position in Fig. 4. KL represents a piece of brass, which serves as the frame for the work, and fits the lower socket of the electrometer, FF, Fig. 3. In this the letters IH indicate the cylindrical pieces which carry the radii, and are seen from beneath. On the side of the nut G, one of the steel drawing pieces is seen; the other being on the opposite side, and consequently not visible. Towards L appear the two re-action springs. The other parts require no verbal description.

In the common construction of the gold-leaf electrometer, there are two pieces of tinfoil pasted on opposite parts of the internal surface of BB; against which the gold-leaf strikes when its electricity is at the maximum. If the radii DD be left at the greatest opening, our instrument does not then differ from that in common use. But if the divergence produced by the contact of an atmospheric conductor, or any other source of electricity, be so small as to render it doubtful whether the leaves be electrified or not, the radii may then be brought very gradually together by means of the screw, until the increased divergency from their attractive force be sufficient to ascertain the kind of electricity possessed by the leaves. In this and all other cases, the division on the micrometer head, which stands opposite the fixed index, at the time the leaves strike the radii, will shew the greater or less degree of intensity.

X.

*The improved Process of Tanning. By Citizen SEGUIN *.*

IN consequence of our knowledge of the multiplied researches and important discoveries of Citizen Seguin respecting astringent substances, and the happy application of those discoveries to simplify and perfect the art of tanning, we invited him to visit the public laboratory, for the purpose of communicating his processes and observations. Far different from those selfish manufacturers, who carefully conceal under the cloak of pretended mystery, operations in themselves simple, frequently transmitted by oral tradition, and which have cost them neither trouble nor expence, the Citizen Seguin did not hesitate to comply with our wishes, and has devoted to this object two sittings, at which all the pupils, and the greatest part of the instructors and agents of the school, were present. He not only exhibited without reserve all that experience and meditation had discovered to him, but he likewise came attended by skilful workmen, with proper instruments to execute all the processes. Lastly, he furnished gratuitously the skins, and a considerable quantity of tan,

* Reported by Citizen Chauffier; being part of the Transactions of the Polytechnic School of France. From their Journal, IV. 678.

in order that each of the pupils might himself repeat the experiments, follow the detail of all the processes, and render himself completely master of the method. We could have wished to render an exact account of the two lectures delivered by Citizen Seguin at the school; but circumstances oblige us to confine our narration to the principal objects.

Want is the parent of the arts. Though man in the possession of all his forces is formed to support the difference of seasons and climates; though the parts of his body which are subjected to pressure, as in quadrupeds, are so disposed as to acquire by habit and exercise a degree of density and compactness, which renders them little sensible to the action of foreign bodies: yet accidents, and circumstances which it is easy to suppose, have determined him to seek the means of securing his feet from the impression of an unequal, stinty, or humid soil. The shepherd must have first made use of soft and flexible bark, mats, and similar fabrications of different kinds; the hunter must have taken a piece of the fresh skin of such animals as served for his support, which he must have fashioned, modelled upon his foot, and retained with straps*. These simple means are sufficient in a climate which is usually dry; but they would be of little advantage on wet ground, or in a climate subject to the alterations of wet and dry weather.

Skins swell up, and become soft, by moisture, which renders them permeable to water. Hence they are easily destroyed by the putrid process which ensues, and they become dry and brittle when the moisture is evaporated. Accident, no doubt, occasioned the discovery of the means of preventing these inconveniences by the use of certain vegetable substances, particularly the bark of oak. It was seen that skins prepared with these substances acquired new properties; that without losing their flexibility they became less permeable to water; more firm, more compact, and in some measure incapable of putrefaction. These observations gave birth to the art of the tanner.

This art, no doubt of high antiquity, because founded on one of the earliest wants of man in society, comprehends a succession of processes which was executed by habit and imitation, without a knowledge of the essential objects. The preparation of skins accordingly required several years, and frequently, in spite of the care, expence, and slowness of the operation, the tanning was incomplete; the skin formed a soft and porous leather, which was soon destroyed by moisture. These defects essentially sprung from ignorance of the true principles of this operation, because no discovery had been made respecting the action of tan upon the skin, and the circumstances or conditions which might accelerate or retard the process.

To arrive at this knowledge in an accurate manner, it is necessary to consider, first, the nature and properties of tan, and secondly, the structure and composition of the skin.

We shall not enter into the detail of such precautions as are requisite in the choice of oak bark, the time and manner of separating it from the tree, preserving it, or pulverising it. It will be sufficient for our object to remark, that water poured into a vessel upon tan acquires, after some hours infusion, at the common temperature of the atmosphere, a brown

* According to the relations of travellers, these usages are still to be found among certain nations. Sparman affirms, that the Hottentots make their shoes with a piece of fresh skin, the edges of which they raise up, and tie with straps; the hairy side is outwards. No other preparation is made than to beat and moisten the skin. If it be strong and thick (such is the skin of the buffalo, for example), it is left for some hours in cow-dung, which renders it soft and flexible.

colour, an astringent taste, and becomes charged with the most soluble substances contained in the tan; that by drawing off the water, and adding a similar quantity to the tan repeatedly, the whole of the soluble parts may be successively extracted, the water ceases to acquire colour, and there remains in the tub a mere fibrous matter, or parenchymatous texture, insoluble in water, and no longer adapted to promote the operation of tanning. This residue is therefore always rejected in the manufactories as useless. It is only used by gardeners for their hot-beds; but might probably be advantageously applied in the fabrication of coarse paper.

It is therefore in the water of infusion, or the lixiviations of tan, that we must seek for the soluble substances which alone are efficacious in tanning.

On examination of the water of the last filtration, it is found to be not only clearer, less impregnated, and less acrid than the water of the first lixiviation, but likewise that it possesses all the properties of the gallic acid. It reddens the infusion of tournsol, acts upon metallic solutions, and more particularly it precipitates a black fecula from sulphate of iron, &c. And it is also found that a piece of fresh skin, divested of its fat and sanguine humours, and macerated in this liquor, instead of becoming compact, is softened and swells up.

The liquor of the first lixiviation exhibits a very different character. It is more coloured and astringent; it not only exhibits the properties of the gallic acid, by the alterations it causes in the blue colours of vegetables, and the black precipitate it forms with the sulphate of iron; but it likewise possesses the remarkable quality of forming, with animal gelatin, or glue, a yellowish abundant precipitate, insoluble in water, not putrescible, which becomes hard and brittle by drying; and if a piece of skin properly prepared be immersed in this fluid, it becomes gradually more compact, and is converted into leather.

There exist, therefore, in the same fluid, two very different substances: the one, which precipitates a black matter from iron, is the gallic acid or principle; the other, which precipitates animal gelatin or glue, is called the tanning principle, on account of its efficacy in the preparation of leather.

To leave no doubt on this important point, it was proved, by a number of experiments easy to be repeated,

1. That the liquor of the last lixiviation, though coloured, and of an astringent taste, affords no precipitate with glue; a fact, which seems to shew that the gallic acid contained in the bark is less soluble than the tanning principle. In fact, as has already been remarked, when water is successively poured on the tan, an infusion is at last obtained which no longer precipitates glue, though it precipitates sulphate of iron very well.

2. The liquor of the first lixiviation, after having been saturated with glue or animal gelatin, and forming an abundant precipitate with that substance, is entirely deprived of the tanning principle. It no longer differs from the liquor of the last filtrations, and contains merely a portion of the gallic acid. Hence the addition of sulphate of iron affords a new precipitate with this liquor.

3. As the tanning principle has a strong attraction to the animal gelatin, with which it always forms an insoluble precipitate, this property affords a very convenient reagent to ascertain its presence immediately in any fluid, and to determine with precision its quantity. Accordingly, the infusion of tan poured into milk, whey, serum, broth, &c. forms, with

these liquors, a precipitate more or less abundant, according to the quantity of gelatin they contain.

This peculiar property of the tanning principle affords an application which may become of great importance in the art of treating diseases, to determine the nature of urine, and to ascertain some of its changes. In the healthy subject, all whose functions are duly exercised, the urine does not contain gelatin, nor afford a precipitate with the infusion of tan: on the contrary, in all the gastric affections, the urine is more or less charged with gelatin, and forms, with the infusion of tan, a precipitate more or less abundant. The same observation is applicable to acute and chronical diseases, in which the assimilating or digestive forces are troubled, deranged, or perverted.

4. The gallic acid, or, if other terms be preferred, the principle which precipitates the sulphate of iron, is often found alone, or at least without being accompanied by the tanning principle. Thus quinquina, crude or torrefied coffee, the roots of the strawberry-plant, scrofularia, milfoil, arnica, the flowers of Roman camomile, and all the multitude of plants vaguely comprised under the title of astringents, contain the gallic acid only. All these form with the sulphate of iron a precipitate more or less coloured and abundant; but none of them produce the slightest change in the solution of animal glue. On the contrary, the tanning principle has never been found alone, but always united or combined with the gallic principle. It was long supposed to exist exclusively in the oak, the nut-gall, and sumac, the only substances used at the tan-works; but it is found more or less abundantly in the siliquastrum, the rose-tree, the larix, several species of pines, the acacias, the lotus, the squill, the roots of bistort, of rhubarb, of parella, and several other plants, of which we shall hereafter give a list. We have also found this principle in the products of distillation of different vegetable substances, where it was in some measure formed during the operation.

From these different considerations, founded on experiment, the following general principles may be deduced: 1. Every substance of which the infusion is capable of precipitating animal jelly, possesses the tanning property. 2. Every substance which possesses the tanning property, likewise precipitates the sulphate of iron black. 3. Every substance which precipitates the sulphate of iron, but not the solution of glue, does not possess the tanning property*.

We shall dismiss the consideration of the tanning principle, by shewing some of the remarkable changes to which its infusion is subject.

1. A few days after its preparation a yellowish precipitate is spontaneously formed, which is more abundant in proportion as the liquor is more saturated, and the time of infusion longer. This precipitate, when separated, is converted by drying into a very fine, light, ash-coloured powder, totally insoluble in water, oil, alcohol, and ether, even at the temperature of ebullition. When thrown on burning coals, it readily takes fire, and emits a thick smoke. By destructive distillation it affords a considerable quantity of carbonic acid gas, an aqueous acid, and a brownish oil, leaving in the retort a light spongy coal.

* The solution of glue is a convenient re-agent to ascertain the presence of the tanning principle. It is therefore advantageous to have it in readiness in the laboratory. To prevent the speedy putrefaction to which animal substances are liable, we have added one-twentieth part of alcohol, which answers the purpose very well, without altering the properties of the fluid. C.

As this precipitate by its insolubility approaches to the nature of wood, and as it falls down even when the lixivium of tan is preserved in well closed vessels entirely filled, it might be supposed that it is merely a gradual and successive deposition of the remains of the woody and parenchymatous parts of the bark disseminated and suspended in the fluid; it might be imagined that the contact of air has nothing to do with this phenomenon. But we must observe, that this precipitate is found in the most limpid solution, even after it has been filtered; that it is most abundant when the infusion has been prepared from bark ground long before, and preserved without care; and that it is more readily deposited when the liquor presents a great surface to the contact of the air. In this case the surface of the fluid becomes tarnished by an extremely fine pellicle, which covers it, and becomes thicker and firmer the longer the fluid remains thus exposed and at rest. If it be divided by agitating the liquor, it is again renewed, and in this manner produces the precipitate we speak of. We may determine, in some respect, at pleasure, the formation of this pellicle and precipitate, by exposing the liquor to the contact of oxygen gas, or by pouring into it the oxygenated muriatic acid. It is sufficient even to bring a bottle containing the oxygenated muriatic acid into the vicinity of the infusion of tan. A pellicle is instantly formed, resembling a slight gauze, which floats and spreads instantly over the whole surface, assuming in a regular order the most beautiful colours of the iris, and at last becomes brownish. If the experiment be continued for a few minutes, the pellicle thickens, and the precipitate begins to fall. The formation of this precipitate, and the properties it acquires, depend, therefore, on the combination of oxygen, which operates either during the infusion and filtration of the lixivium, or after its having been drawn off clear with the contact of atmospheric oxygen. Hence are shewn the necessity and advantage of using tan recently prepared *, and of preserving the tan as well as the infusions defended from the contact of the air.

These phenomena are common to all the preparations of vegetables made by means of water, whether by infusion, decoction, or even distillation. We have seen these precipitates gradually formed in the infusion or decoction of nut-gall, sumac, gentian, quinquina, and even of the most insipid as well as of the most aromatic and acrid plants. The expressed juice of fresh plants, such as hemlock, sorrel, anil, &c. presents the same phenomena, and affords, more particularly when the action of air is promoted by agitation, a coloured pulverulent deposition which has long been distinguished by the name of *fecula*. Lastly, all the distilled waters which are kept for several years become turbid, by the formation of filaments and whitish flocks, more or less abundant, but always insoluble. It is to a combination of oxygen, as Fourcroy in his excellent analysis of quinquina has shewn, that we ought to attribute the formation of these precipitates, the insolubility they acquire, and their approach to the nature of the ligneous fibre.

Nevertheless, the spontaneous precipitation which is effected by the contact of atmospheric oxygen in the lixivium of tan does not change its properties, at least in any sensible manner; and as in a lixivium considerably saturated the precipitate is most abundant on the first days, it might perhaps be of advantage to wait until this first precipitate was formed before

* These observations are applicable to all vegetable substances which are reduced to powder. They not only lose their peculiar aroma, but likewise change their nature by the contact of light and atmospheric air. Their properties being thus considerably altered, the dose at which it is proper to employ them becomes uncertain. The physician ought not therefore to prescribe in his formulae any powders but such as are recently prepared. C.

immersing the skins. For this substance, when deposited, attaches itself to the surface of the skins, where it forms a coating more or less thick, which soils them, closes their pores, and retards in a small degree the direct action of the tan.

2. The lixivium of tan, particularly when highly concentrated, and prepared in hot weather, acquires after a few days a vinous smell, which seems to announce a commencement of fermentation, and might lead to an apprehension that its progress might impair the tanning properties. In order to ascertain how far these notions might be well founded, different vegetable acids were mixed with the tanning lixivium, but none of them sensibly altered its properties. And still more we may add, that for near two years a bottle of the infusion of tan has been kept in the laboratory, and, in spite of the alterations of temperature to which it has been exposed, and the change necessarily produced by the contact of air, its properties appear the same, and it is still daily used with advantage as a re-agent to ascertain the presence of gelatin.

As a knowledge of the properties of tan affords observations of importance, which constitute the basis of Seguin's method, we have not been apprehensive of dwelling too long on the subject; but we shall confine our remarks on the structure and chemical composition of the skin to a few general considerations.

This membrane, as is shewn by anatomists, is essentially formed of a great number of laminae or fibres, which are white, broad, short, and closely adherent, but interwoven, and disposed in different directions, so as to leave between them an infinity of small spaces or pores. From this construction it exhibits a dense but soft spongy texture, and is susceptible of extension and contraction. In the midst of this substance are found a great number of nerves and vessels of different kinds, the very fine and multiplied ramifications of which serve to support and maintain the lamellated structure of the skin, and convey into the vacuities between the fibres those fluids which serve for the support and nutrition of this membrane. The external part is the epidermis. This thin transparent membrane is of a very different texture. It contains neither the apparatus of vessels, nor the disposition of fibres or laminae, which by their intersection form a kind of spongy net-work. It is in some measure a simple uniform covering, which presents no distinct organization, but adheres strongly to the skin, entering into its folds and numerous porosities. Lastly, the hairs are implanted in the skin by a sort of bulb or oval root, interspersed with small vessels filled with a kind of mucus.

The composition of the skin, the changes it undergoes by different preparations, by chemical agents, and the properties it acquires in these several states, present other considerations.

If the fresh skin of an adult animal be macerated for some hours in water at the temperature of the atmosphere, and if, to accelerate the effect, agitation and pressure be used, a separation is made of the blood, the juices, and the different soluble substances contained in the vessels or vacuities; and by examining the nature of the matter thus extracted, it is found that a small portion only consists of gelatin, which has little consistence or tenacity. Subsequent macerations, at the same temperature, afford no more gelatin, the fibrous texture remains insoluble, and undergoes no further loss. It therefore appears, that in the natural state the skin contains but a small portion of gelatin perfectly formed, and soluble. It may even be apprehended, that the small quantity obtained by the first maceration was merely included in the pores of the skin adherent to the surface of the fibres; and that the vital force had not yet time to assimilate and convert it to the state of fibre.

But if the temperature be raised to ebullition the fibrous texture is altered, becomes soft, and successively dissolves. It assumes the character of gelatin, and may be entirely converted into a viscid and tenacious glue. The fibre which forms the solid structure of the skin does not therefore essentially differ from gelatin, but in its texture, its concretion, and its insolubility in cold water; and as it is observed that substances capable of absorbing oxygene deprive the fibrous matter of its solidity, and hasten and determine its conversion into gelatin, we are authorized to conclude that these distinct qualities of the fibre depend only upon a proportion of oxygene which the vital action and progress of life combine with the gelatin. Multiplied experiments appear to leave no doubt on this point. "They have proved to me (says Seguin) that the fibre is oxygenated glue, which in that state cannot combine with the tanning principle, but which acquires that property by passing to the state of gelatin in consequence of the loss of a portion of oxygene."

The essential point in the operation of tanning is therefore to ascertain, and to direct in a precise and invariable manner, the circumstances and conditions which determine the transition of fibre to the state of gelatin, and to seize the instant in the process to effect a proper combination with the tanning principle. This object is too important not to recall some of the observations made by Seguin.

Glue, as has before been remarked, possesses a great attraction for the tanning principle. It immediately forms with this substance an insoluble matter, which is not subject to putrefaction; but it must be well remarked that the precipitate is dry and brittle.

Hence it is evident that a skin, the pores of which might contain gelatin ready formed, or of which the fibrous texture should have been altered and converted by certain preparations to the gelatinous state, would tan very speedily, but would afford a kind of leather which would be harsh, brittle, and disposed to crack or peel in the wear. On the contrary, the operation will be long, incomplete, and will afford only a soft, spongy, and putrescible leather, if the fibre preserves its state of oxygenation, if it do not pass by successive degrees into the state of gelatin, or if the tanning principle do not penetrate its thickness in proportion to the conversion of the fibre into glue.

These inconveniences are avoided, and the proposed object is obtained with precision, by the use of the lixivium of tan. This fluid has been shewn to contain two very different principles, of which the union and action are alike necessary for the success of the operation. On the one side the gallic principle, which, as is known, readily seizes the oxygene of metallic solutions, and reduces them or brings them nearer to the metallic state, acts nearly in the same manner upon the fibre; it unburns it, or deprives it of oxygene, and converts it into the state of gelatin. On the other hand, the tanning principle, which also exists in the solution, exercises its action as soon as the fibre is sufficiently reduced to the state of gelatin. The tanning process is not, therefore, instantly effected, like the precipitation of animal glue; but it operates gradually, and in succession, by strata from the surfaces of the skin to the centre; and as the action of the tanning principle immediately follows that of the gallic acid, the fibre is surprised in its position, the felted texture of the skin is totally preserved, and its composition alone is changed. Accordingly, the leather prepared by this process has the advantage of being supple, flexible, insoluble, imputrescible, more durable, and less disposed than any other to imbibe humidity.

The new method of Seguin for the preparation of leather is founded upon this series of observations

observations and experiments. We shall not in this place enter into the detail of the changes and improvements he has made in the different branches of the tanning art; but we must add, that the processes have been repeated in all the laboratories of the Polytechnic School, by the greatest number of the pupils; that the operations on different kinds of skins were all concluded in eight, ten, twelve, or fourteen days at most; that the leather thus manufactured was completely saturated with tan, and of a quality superior to the leather prepared by the old method which is to be met with in the market; and lastly, that to these advantages the method of Seguin unites simplicity, facility, and certainty of success. A discovery of this importance, in an art so necessary to our wants, entitles the author to the esteem and gratitude of the public; and these sentiments are more especially due to the citizen who sacrifices his individual interest to the general prosperity, and is desirous of communicating the fruits of his researches. The advantage of public utility constitutes his recompense, and enables him to disregard the clamours which envy, ignorance, and the prejudice of old habits never fail to raise against every useful innovation.

After the two sittings employed by Citizen Seguin to explain the nature and properties of tan, the order adopted in the course of vegetable chemistry was resumed in the school. The alkalis, the ligneous part, and the colouring matter were successively examined; the last of which naturally led to an explanation of the principles of the art of dyeing, the different processes for fixing colours, and ascertaining their qualities. In treating of these different objects, some considerations were presented respecting the new properties which stuffs acquire by tinctorial processes.

The process of dyeing ought not to afford simply a colour to please the eye, but should effect a kind of colouring tannage capable of adding to the properties of the web, and to render it less soluble or putrescent. This object is generally neglected or little known, but it cannot be indifferent. It is not only of importance with regard to economy, but we do not hesitate to say, equally so for the preservation of health. This may easily be conceived if attention be paid to the circumstances, that cloths of the same kind and fabric differ considerably according to the dye; that some are dry, brittle, of little durability, not adapted to preserve or retain caloric; others are soft, spongy, capable of retaining damps and exhalations of every kind; and by the successive action of light, air, and caloric, may either undergo a sort of oxidation that destroys their texture, or else pass to a kind of putrefaction, which forms a continual atmosphere round the body, and more or less affects the health. It is in the midst of armies under tents, in camps, where a great number of men are exposed to the same kinds of fatigue and intemperance, and differ only in the colour of their clothing, that we may ascertain the truth of these observations. It is in the magazines of the military hospitals, where the clothing of men is deposited, that a very marked difference in the smell and porosity of the stuffs is observed, according to the colour with which they are charged.

After a successive examination of the different products of vegetables, the attention of the school was directed to the alterations they undergo after death, by the action of light, caloric, air and water. These subjects led to the theory of the vinous and acetous fermentations, and the production of alcohol and ether; and lastly, the course was terminated by an examination of the phenomena of putrefaction, which restores to the atmosphere and to the earth those principles which the action of vitality had before extracted.

During

During the concluding months of this course, the pupils continued to operate in the particular laboratories appointed for their use, either in repeating the principal experiments which had been exhibited in the lectures, or in making others which had been pointed out or imagined by themselves. Their attention was principally directed to the application of chemical knowledge to the progress of arts and manufactures. With this view they successively prepared different kinds of soap, varnishes, and pigments, and they executed in small the different processes of dyeing, tanning, &c. Some of the pupils attended particularly to the experimental research of substances proper to form the oxalic acid; others engaged in the experimental enquiry after those vegetables which contain the tanning principle, and might be used as a substitute for oak bark. From these researches a table has been formed, which is already very ample, wherein those vegetables which afford a precipitate with the sulphate of iron alone, are distinguished from those which afford a deposition with the solution of glue. This table is already very interesting; but it may easily be imagined that it still presents vacancies to be filled up by time and experiments. This will form the object of the work of another year; and when it shall be finished, we shall hasten to present the labours of the pupils of the school.

XI.

The Combustion of Phosphorus in the Vacuum of the Air Pump. By DR. MARTINUS VAN MARUM.

[Concluded from page 237.]

BEING desirous of ascertaining the cause of this singular inflammation in air so rarefied, I put, on another day, a small stick of phosphorus, wrapped in the same manner in cotton powdered with resin, under a receiver containing about four hundred cubic inches, on the plate of the air pump, in order to observe the phenomenon with accuracy a second time. The temperature of the place where I made the experiment was nearly the same as on the former occasion, namely 56 degrees of Fahrenheit's scale. To observe the degree of rarefaction more easily, I placed a short barometer gauge under the receiver. When the air was rarefied so that the mercury was supported in the gauge to the height of about an inch, the light began to be enlarged on the surface of the phosphorus, chiefly at the upper part of the small cylinder. This light increased by degrees during the subsequent exhaustion, and the inflammation took place when the mercury stood at the elevation of 5 lines.

The flame was much paler and weaker than is afforded by phosphorus when burned in an atmosphere of the usual density. I observed the flame to become weaker and weaker, and about two minutes afterwards the phosphorus ceased to exhibit any light.

IV. To ascertain whether the cotton powdered with resin, which was wrapped round the phosphorus, might be the cause which gave place to the inflammation, I placed under the same receiver two small sticks of phosphorus of the same size, one of which only was wrapped in the powdered cotton. These two small pieces began to shine at the same time, when the mercury stood at the height of about an inch in the short barometer gauge. Nevertheless, the piece which was surrounded by the cotton and powdered resin alone took fire when the rarefaction was more advanced.

I then

I then thought there was some reason to suppose that the resin might be the cause of the inflammation; and to determine this point, I put under the same receiver three similar small sticks of phosphorus, one of them simply powdered with resin, another wrapped in cotton without resin, and the third wrapped in cotton powdered with resin, in the same manner as in the former experiments. The light began to increase at the same time in all three, namely, as soon as the mercury had fallen to about one inch in the barometer. The phosphorus wrapped in cotton powdered with resin took fire first; a short time afterwards that which was wrapped simply in cotton took fire; and that alone which was simply powdered with resin did not take fire at all.

V. After these results, the question was, to know in what manner the cotton could cause inflammation in air so rarefied, in which every other combustible, though set on fire, would cease to burn; and still farther, how phosphorus could spontaneously take fire when the experiment was made in an atmosphere where the temperature does not exceed 56 or 58 degrees of Fahrenheit, though the phosphorus does not take fire in the open air unless heated to about 112 degrees of Fahrenheit. After some reflections and experiments, I found that this singular phenomenon could be very easily explained according to the principles of the modern chemistry. The increase of the light which precedes the inflammation when the air is rarefied to a certain degree, suggested to me this explanation. I shall therefore, in the first place, explain the evident cause of the increase of light in the phosphorus in the rarefied air.

VI. Exhalations continually rise from the surface of phosphorus when exposed to the atmosphere, as is proved by its speedy dissipation in that circumstance: but as soon as the air is rarefied to a certain degree, the exhalations cannot rise; for these particles will not be elevated but during the time that the surrounding air is heavier than themselves. Whenever, therefore, the air is rarefied to this degree, the exhalations must remain, and surround the phosphorus from which they came. The union of oxygen with these phosphoric exhalations must be then made only in the vicinity of the phosphorus, whence the light from the disengaged caloric must be seen there only. It is evident that this light will be much stronger when the phosphoric exhalations do not rise, because the disengagement of the same quantity of caloric is then made in a more confined space than when the phosphoric exhalations could rise and be dispersed in the receiver.

VII. The caloric which is disengaged from the oxygen, and is seen in the rarefied air round the phosphorus in the form of a stronger light than ordinary, must, also, on account of its greater density, give heat to the phosphorus. Hence may be clearly seen the reason of the combustion of phosphorus on rarefying the air when it is surrounded with cotton, as before described. Woollen and cotton stuffs have the property of preventing the dispersion of caloric*. The caloric which is disengaged round the phosphorus in the rarefied air is thus retained by the cotton, until its accumulation on the surface of the phosphorus becomes at length sufficient to set it on fire. When a piece of phosphorus is not enveloped in cotton or some similar substance, it does not take fire in the rarefied air, because the caloric which is disengaged near the phosphorus is so speedily dispersed when it is not arrested by the cotton, that the phosphorus cannot acquire the degree of inflammation necessary to inflame it.

* Because they impede the circulation of the air, by which the heat would else be conducted off.

VIII. Though this explanation appeared to me to be very evidently founded upon the knowledge we already possess, I was nevertheless desirous of shewing the truth by a direct experiment. I endeavoured to ascertain, by means of a thermometer, that the temperature near the surface of the phosphorus which is wrapped in cotton, is more elevated before the inflammation, when the light is perceptibly stronger. For this purpose, I used a thermometer, the bulb of which was about one quarter of an inch in diameter. I fastened the cotton with which the small stick of phosphorus was surrounded, as in the preceding experiments, to the ball of this thermometer, so that it was entirely surrounded with it, at the same time that its distance from the surface of the phosphorus was about half a line. The result of this experiment answered my expectations. I observed that the mercury rose, after the light had increased, and that from 52 degrees, which was the original temperature, it had risen to 67 of Fahrenheit before the phosphorus had taken fire.

The size of the ball of this thermometer appeared to me to be the cause why the mercury did not rise higher before inflammation. I had also admitted some distance between the bulb and the phosphorus, in order that its contact might not prevent the phosphorus from acquiring the requisite degree of heat which is necessary for its inflammation. I therefore resolved to repeat the experiment with a thermometer of the same kind as was used by Dr. Hunter in his observations on the heat of animals and plants, the ball of which was not more than one line in diameter. I fastened cotton to this in the same manner, which was attached to a small stick of phosphorus; but the ball of the thermometer was placed in contact with one of the ends of the stick of phosphorus, which was half a line in diameter, and four lines in length. I then saw that while the light round the phosphorus became stronger and stronger, the thermometer rose from 46 to 76 degrees before the phosphorus took fire. The sudden heat broke the ball of the thermometer; which prevented my using similar thermometers in the repetition of this experiment.

Though this thermometer did not mark the degree of heat which was necessary to set fire to phosphorus in the open air, it is seen, nevertheless, from the experiment, that the temperature rises very considerably at the surface of the phosphorus before it takes fire. The ball of the thermometer which I used for this experiment had its glass extraordinarily thick at bottom, which was probably one of the reasons why the thermometer did not indicate an higher temperature before the inflammation, as there is no reason to suppose that phosphorus will take fire in rarefied air at a less temperature than in the atmosphere*.

IX. Lastly, I examined whether the air could be rarefied to such a degree that phosphorus should be incapable of taking fire in it. I saw it take fire in air so rarefied that the mercury in the gauge stood at the height of one single line only. The phenomenon here described is hitherto, as far as I know, the only example of a true inflammation in air rarefied to the highest possible degree by the air pump. This fact, however, does in no respect prove that a true inflammation can take place in vacuo.

When the mercury in the gauge placed beneath the receiver stands at the height of one

* As the solidity of the ball of the thermometer was about half a cubic line, and the phosphorus little more than three quarters, it should seem that, as the heat was shared between the mercury and the phosphoric vapour, the thermometer could not be expected to rise to the point of combustion unless it had remained unbroken for a longer time. N

line, which indicates the utmost degree of rarefaction of the air which I have ever observed to take place, the rarefied air still possesses 1-300th of the density of the air of the atmosphere, in case the mercury in the barometer be supposed at 30 inches elevation.

It is certainly very singular, that the small quantity of oxygene gas which remains in air so rarefied should be sufficient for the inflammation of phosphorus; more especially as all other combustible substances are extinguished in air rarefied to a much lower degree. I have already explained the principal reason. There is another circumstance which probably favours this inflammation, of which I shall speak hereafter, when I shall have made some experiments.

X The phenomenon exhibited by the phosphorus in rarefied air, as here described, is certainly a real combustion, as is proved by the very remarkable diminution of weight of the phosphorus after the experiment has been made in a large receiver, or has been several times repeated. The phosphoric acid produced by the combination of oxygene with the substance burned is also found on the plate of the air pump.

XI. The combustion of phosphorus in air highly rarefied is accompanied with several very singular phenomena, which I shall here mention.

1. The phosphorus usually emits, a short time after the commencement of the inflammation, fiery jets, in the form of small ignited balls, which are dispersed on all sides in the receiver, and exhibit a very curious and surprising appearance. I have not hitherto been able to explain this phenomenon.

2. The flame which surrounds the phosphorus when it burns in the rarefied air, extends farther and farther in a globular form. Its light at the same time becomes paler and paler, and at length disappears. This enlargement of the flame, and diminution of the light, are probably to be attributed to the oxygene, to which the phosphoric exhalations may unite, being gradually exhausted. The light at last entirely disappears, when all the oxygene gas which existed in the rarefied air, and could be reached by the phosphoric exhalations, is combined with them; for, as soon as there is no more oxygene to which the phosphoric exhalations may unite, there ceases to be any further separation of caloric, which causes the flame and the illumination.

3. When a small quantity of atmospheric air or oxygene gas is suffered to enter the upper part of the receiver by a cock after the combustion or light of the phosphorus has disappeared, a pale light is then seen to disperse itself through the whole capacity of the receiver. This light must certainly be attributed to the combination of the oxygene, of the gas which has entered, with the phosphoric exhalations which exist in the receiver. The exhalations which the phosphorus emits after it has been heated by the inflammation, are apparently more subtle and light than those which preceded that phenomenon; and it is probably owing to this reason that they can support themselves in the gas which remained in the receiver, though very highly rarefied.

4. When the apparatus is left untouched for some time after the combustion of the phosphorus in the rarefied air has ceased, and the whole has become cooled, then the phosphoric exhalations descend in this rarefied air. In proof of this, when the experiment is made in a receiver, on the plate of the air pump, and air is suffered to enter by a cock beneath the plate, as usual, it is then seen that the light occasioned by the entrance of the air takes place only near the plate upon which the phosphoric exhalations have fallen.

XII. When the atmospheric air or oxygene gas enters the receiver by a cock from above, a little before or the instant after the light of the combustion has disappeared, and when care is taken to admit no more air or gas than shall be sufficient to raise the gauge one or two lines, the phosphorus usually takes fire a second time. In this manner the phenomenon may be made to appear repeatedly; and accordingly as a greater or less quantity of air is suffered to enter, the circumstances are found to vary in a very remarkable manner. This communication would be too long if I were to attempt to describe the variety of appearances which I have observed in these experiments. They are not entirely the same, in circumstances which appear perfectly similar. I have observed that pieces of phosphorus obtained by different operations have exhibited different phenomena. The fiery globules emitted by the burning phosphorus are in some cases larger and more numerous than in others.

MATHEMATICAL AND PHILOSOPHICAL CORRESPONDENCE.

QUESTION V. * Answered by the PROPOSER.

LET x denote the weight of the salt, and s its specific gravity; w the weight of the bottle of distilled water, and W that of the solution; the magnitude, or content, of the bottle, and the specific gravity of the water, being each considered as 1.

Then, since the magnitudes of bodies of the same kind are as their weights, $w : W - x ::$

$\frac{W - x}{w} = \text{magnitude of the aqueous part of the solution} : \text{and, because the magnitudes of bodies of different kinds are as their weights divided by their specific gravities, } \frac{w}{1} : \frac{x}{s}$

$:: 1 : \frac{x}{ws} = \text{magnitude of the salt. Whence } \frac{W - x}{w} + \frac{x}{ws} = 1$; from which equation

x is found $= \frac{s}{s-1} (W - w) = \text{weight of the salt. And since } s = 2.8, W = 2810 \text{ gr. and } w = 2506 \text{ gr. by the question, we shall have } x = \frac{2.8}{2.8-1} (2810 - 2506) = \frac{2.8}{1.8} \times 304 = 472\frac{2}{3} \text{ gr. the quantity of salt required.}$

And if the case be reversed, by supposing x to be known, the value of s will be readily found from the general formula; by which means we shall be enabled to make a proper allowance for the difference in the specific gravity of the salt, arising from chemical condensation, as was done in the proposing of the question, where it is taken as 2.8 instead of $2\frac{1}{2}$, which it is supposed to be in its separate state.

QUESTION VI. Answered by C. W.

LET m be put for the mean temperature at the equator, n for the difference between this temperature and that of the north pole, and l for the latitude of any place, radius being 1; then the mean temperature of that latitude will be $m - n \times \sin l$. For the heat

produced by the direct action of the sun, is as the sine of the sun's altitude; so that if the earth were equally heated at the equator and poles, the quantity to be added to the polar heat would be $n \times$ sine of the sun's altitude, radius being 1. But it has been ascertained from observation, that the heat on the earth's surface (*ceteris paribus*), and consequently the heat emitted by the earth itself, is always proportional to the direct solar heat; whence the quantity to be added must be $n \times$ the square of the sine of the sun's altitude: or, since the mean annual altitude of the sun, in any latitude, is equal to the complement of that latitude, the quantity to be added to the polar heat ($m - n$) will be $n \times \text{cof. } \frac{1}{2}l$; but $\text{cof. } l = \sqrt{1 - \text{fine}^2 l}$; consequently, $m - n + n \times \text{cof. } \frac{1}{2}l = m - n + n - n \times \text{fine } \frac{1}{2}l = m - n \times \text{fine } \frac{1}{2}l$. Hence, if the mean temperature of any two latitudes be known, the temperature under the equator, at the pole, and in every intermediate latitude, may be readily ascertained.

The mean temperature of lat. 40° is found to be 62.1, and of lat. 50° to be 52.9; but the square of the sine of 40° is .413, and the square of the sine of 50° is .586; whence

$$m - .413n = 62.1,$$

$$m - .586n = 52.9;$$

From which equations n is found = 53 nearly, and $m = 84$. So that, the mean annual temperature at the equator being 84° , that of the pole must be $84 - 53 = 31^\circ$; and the mean annual temperature of every intermediate latitude will be $84 - 53 \times$ the square of the sine of that latitude.

NEW MATHEMATICAL QUESTION.

QUESTION X. By W. THOMSON.

IT is required to find at what time of the longest day it is the hottest in London, supposing the heat to be as the sine of the sun's altitude, and the time of its continuance above the horizon.

PHILOSOPHICAL QUESTION.

THERE is an optical appearance so frequent, that it is rather surprising that writers on that science have never mentioned it. Whenever the sun shines upon agitated water not absolutely loaded with opake matter, and the spectator is so placed that the shadow of his head may be projected upon the surface of the fluid, he will see an innumerable quantity of divergent rays within the water, of which that shadow is the centre. They are incessantly shifting their place laterally; and if more persons than one are present, each sees a system of radiations or glory round his own head, but no such appearance round the shadows of the other persons, though these also are very visible to him. As this phenomenon has a striking effect when observed, and may be accounted for upon the common principles of optics and perspective, it is offered to correspondents as an object for explanation.

To Mr. NICHOLSON.

London, 22d August 1797.

SIR,

UPON perusing the account given in your last number, of the phenomenon of the Fata Morgana, and upon consulting the plate annexed by way of explanation, I was very much puzzled to comprehend how the spectator, "placed on an eminence of the city, with his back to the rising sun, and his face to the sea," looking of course towards the west, could at once enjoy the sight of this truly admirable phenomenon, a distant view of the *western* aspect of the city, and also of the mountains *behind* him. A few words in a future number, in explanation of this strange combination, will much oblige,

Sir,

Your constant reader,

DAVUS.

* * THE difficulty stated by this correspondent, together with several others, engaged my attention at the time the account (p. 225) was drawn up: but as they seemed scarcely capable of being cleared up by reasoning, while the theory remained so very uncertain, I thought it best to avoid entering the ample field of conjecture which offered itself. It seems altogether improbable, that the rays of light should be *reflected* immediately back to the city, and every part of the drawing directly contradicts this supposition. I am therefore inclined to conclude that the description, though literally translated, is faulty, so far as it contradicts the notion of the observer being so placed as to view the city over a portion of the bay. I imagine that the people, when they run hastily to the sea exclaiming Morgana! do not run to the ramparts of the town, but to the southern point of the bay, at the distance of half a mile or less from the town; whence, with the sun* behind them, they may have an oblique view of Reggio strongly illuminated, and a more direct prospect of the northern shore of the bay.

NEW PUBLICATIONS.

An Account of two Cases of the Diabetes Mellitus, with Remarks as they arose during the Progress of the Cure; to which are added, A general View of the Nature of the Disease, and its appropriate Treatment; including Observations on some Diseases depending on Stomach Affection, and a Detail of the Communications received on the Subject since the Dispersion of the Notes on the first Case. By John Rollo, M. D. Surgeon General, Royal Artillery.—With the Results of the Trials with various Acids and other Substances in the Treatment of the Lues Venerea, and some Observations on the Nature of Sugar, &c. By William Cruickshank, Chemist to the Ordnance, &c. in two Volumes Octavo, 14s. Dilly.

IN this treatise Dr. Rollo has considered the diabetes mellitus as a disease of the organs of digestion, and not of the kidneys; he conceives that the sugar which passes off by urine

* The sun's rays can never make an angle of 45° on the sea at Reggio, from the azimuth of the image in the drawing.

is formed in the stomach, and depends chiefly upon some vitiated, but increased action of this organ. He was led to take this view of the disease from reflecting on the state of the stomach and habits of life which preceded, the voracious appetite which always accompanies it, and the state of the blood, the serum of which, although not sensibly sweet, had not the usual salust taste.

The Doctor supposes, that in this complaint the vegetable matter taken into the stomach has not, from some defect in this organ, undergone a sufficient change to form proper chyle; that in consequence of this much saccharine matter is evolved, which, when carried into the circulation, proves a general stimulus, producing head-aches, and quickness of pulse, but that it acts more remarkably on the kidneys, occasioning a constant and copious secretion of sweet urine. From this hypothesis, he was naturally led to adopt a plan of cure, which has proved completely successful. The indications he lays down are: 1. To prevent the formation of saccharine matter in the stomach; and, 2. To remove the morbidly increased action of this organ, and restore it to a healthful condition. These indications are to be answered by a complete diet of animal food, and by the use of such medicines as shall diminish the action of the stomach, and at the same time counteract the formation of saccharine matter. The remedies employed for this purpose have been emetics, kali sulphuratum, lime-water, hepatized ammonia, and vegetable narcotics. But the principal dependence is to be placed on a total abstinence from all vegetable matter, which alone can supply the saccharine principle. By a regular perseverance in this plan, the first patient was completely cured in four weeks, although the disease had been of seven months continuance. The urine, which at the commencement of the treatment was sweet, and amounted to 24 pints daily, was at last reduced to $1\frac{1}{2}$ pint, being at the same time free from any saccharine impregnation.

The second patient, from his age and other circumstances, although relieved from the diabetic affection, did not regain his wonted state of health; but even in this case, the effects produced by the treatment, when properly attended to, were most decidedly in confirmation of this plan of cure.

The Doctor has received several communications in consequence of the dispersion of the printed notes on the first case. The most important are the result of two cases treated in this way by Dr. Cleghorn of Glasgow, and one by Drs. Currie and Gerard at Liverpool; all of which afford the strongest corroboration of the efficacy of this mode of treatment.

To this account of the diabetes are added some experiments on sugar, and the effects of different acids in the lues venerea. The cases of lues venerea treated in this way, and detailed at some length, are: 17 by Mr. Cruickshank, 2 by Dr. Irwin, 5 by Dr. Jameson, and 8 by Dr. Wittman—making in all 32. Of these 19 were cured by the nitrous acid, 4 by the oxygenated muriatic acid, 3 by lemon-juice, or the citric acid, and 6 by the oxygenated muriate of pot-ash. The affections in all these were of the primary kind, being chancres and buboes, but distinctly marked. The effects produced by the different remedies were nearly the same, and such as seemed to indicate a general increased action of the system. There was an increase of appetite, more thirst than usual, a white tongue, an augmentation in the quantity of urine, and the blood when drawn was generally fizy: nothing, however, like salivation was observed.

The

The cures in general seemed to have been performed in less time than would have been necessary under the mercurial plan; and without any confinement or particular regimen. The effect is supposed to have been produced by the disengagement of oxygen from the different substances employed, inducing a new disease in the system. Of the different remedies, the preference is given to the nitrous acid and the oxygenated muriate of pot-ash:— of the first, from one to three drachms were given daily, diluted with about a quart of water, and of the oxygenated muriate of pot-ash from six to sixteen grains four times a-day. No external applications were employed, but milk and water, or a very dilute solution of the cerussa acetata, merely to keep the parts clean. At the time this publication went to press, no relapses had taken place, although some of them had been cured upwards of three months.

Annals of Medicine for the Year 1796. Exhibiting a concise View of the latest and most important Discoveries in Medicine and Medical Philosophy. By Andrew Duncan, Sen. M. D. and Andrew Duncan, Jun. M. D. Fellows of the Royal College of Physicians, Edinburgh, Vol. I. octavo, 469 pages. Edinburgh, printed for Mudie and Son, and for Robinsons, London, 1796.

The Medical Commentaries of the elder Dr. Duncan, in twenty volumes, published at Edinburgh, are well known to the public. The *Annals of Medicine* are offered as a continuation of that work, from which the plan will not materially differ. The editors, from whose preface I give this account, expect that, when peace shall be again established, their accounts of foreign medical literature will be superior to what the English reader has been hitherto accustomed to receive.

Reflecting practitioners are invited to use this work occasionally as a channel of public communication of such practical observations and facts as they may think worthy of being so diffused. These may be transmitted either to Dr. Duncan of Edinburgh, or Dr. Pearson of Leicester Square, London.

The volume consists of four sections. The first contains analyses of books, twenty in number; the second, medical observations or cases; the third, medical news; and the fourth, a list of new books.

Professional men will not require to be informed of the utility of publications of this nature; and there is no question respecting the ability of the editors. If it were practicable, with consistent brevity, to give any analysis of the contents of such a work, it would on these accounts be the less necessary to make the attempt. I shall therefore only remark, that the number of new and interesting articles in this volume is considerable.

A Narrative of the successful Manner of cultivating the Clove-Tree, in the Island of Dominica, one of the Windward Charibbee Islands. By William Urban Buée, Esq. London, printed 1797. Quarto, 31 Pages, with an Engraving of the Clove-Tree, and some Implements for planting it. No bookseller's name, nor price known.

This pamphlet was printed by order of the Privy Council, in consequence of a Report from the Right Hon. Sir Joseph Banks, Bart. to whom it was referred, as appears in a letter from that gentleman to the Earl of Liverpool, which forms part of the Appendix. From that communication it appears, that Mr. Buée is the first person who has observed that

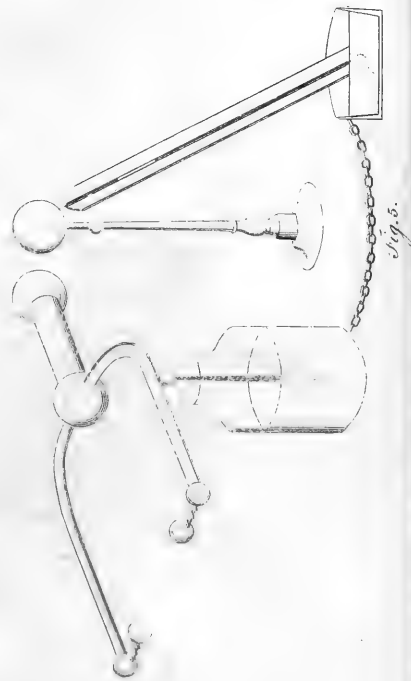
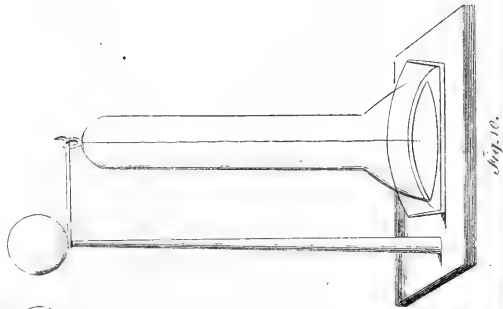
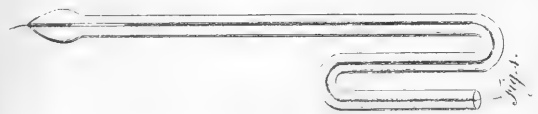
the pimento tree prospers best in those sterile soils where trees whose wood is of a hard texture abound, and that sugar cannot be cultivated to advantage in such places; and also, on the other hand, that where trees whose wood is soft are naturally found, pimento trees are rarely met with, and sugar plantations will succeed.

Mr Buée observes, that in the West Indies, particularly in Dominica, most lands facing the east are of a yellowish or reddish stiff clay; a soil which, with few exceptions, is hardly fit for any cultivation, but is productive of the hard wood trees. Partly by the observation of incidents which presented themselves, and partly from a rational process of investigation of the subject, the author has applied these facts to the clove-tree, which is of a hard clove grain, though not so tough as the pimento. In the Moluccas, where the clove-trees grow, the ground is covered with them, and will not admit the culture of any thing else. It was not, however, till after a number of experiments, during a series of years, that he arrived at the possession of several bearing clove-trees. The historical account of his proceedings with regard to the best method of planting the tree, and of rendering the cloves merchantable, is intelligent and clear. For this, however, I must refer the reader to the work itself. Mr. Buée has also succeeded in propagating the cinnamon-tree, of which he possesses a great number, and promises to make it the subject of his future remarks.

These observations, to use the words of Sir Joseph Banks, open to the cultivators of hot climates a new source of wealth, which will not probably be confined to the growth of cloves. Other spices may also prosper best in the barren soils of the West Indies, as lavender, thyme, and other aromatic plants are known to do in those of Europe.

Besides the letter of Sir Joseph Banks, the Appendix contains a list of the useful plants cultivated in the Royal Gardens at the Isle of France in 1790; and a letter from Mr. Rutton of Charing-Cross, expressing the uniform opinion of several eminent grocers, that certain samples of Mr. Buée's cloves, forwarded to the Council by Sir Joseph, will answer every culinary purpose as well as those of the Spice Islands in the East Indies.







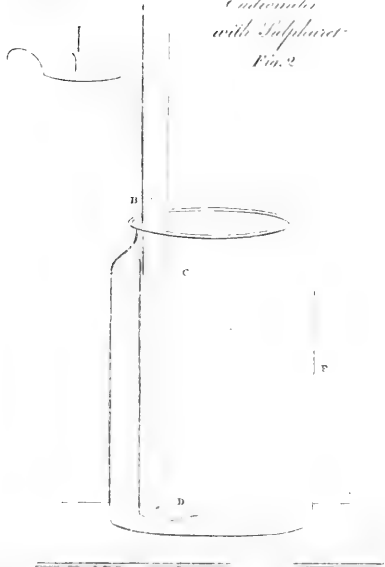
Method of closing Vapors

Fig. 1



Endoscopes with Sulphuric

Fig. 2



Endoscopes

Fig. 3

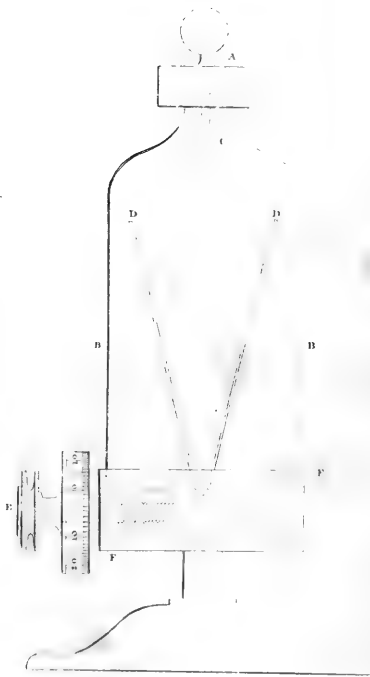
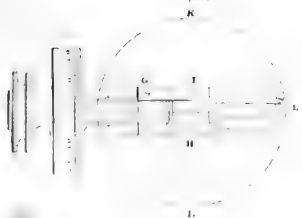


Fig. 4





A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

OCTOBER 1797.

ARTICLE I.

An Account of the Manner in which Heat is propagated in Fluids, and its general Consequences in the Economy of the Universe. By BENJAMIN Count of RUMFORD.*

THE doctrine of heat is of such singular importance, not only in the experiments of philosophers, but in the whole economy of animated and of inanimate beings, that at first sight it appears wonderful the greatest part of the discoveries relative to this natural power should have been made by our cotemporaries. Not many years ago, our knowledge amounted to little more than that bodies, by the communication of heat, acquire a common temperature; a fact which, simply expressed, denotes nothing more than that there is a state of equilibrium, at which two bodies may be in contact, without the communication of heat from the one to the other; and again, that the communication of heat is more rapidly effected through some bodies than through others. The discoveries of Doctors Black, Irvine, Crawford, and others, have taught us, that when bodies equal in weight or bulk, or alike in any other attribute (their intimate nature or composition excepted), are brought into contact, they do not, by acquiring the common temperature, occasion an equal change in the sensible heat of each. In some instances the heavier body must part with a greater number of degrees of its temperature before an equilibrium of communication becomes established between itself and a lighter body; and in other instances the contrary will be the case. If, therefore, water be used as a standard, and applied in like temperatures and circumstances to different bodies, the common temperature will differ accordingly as the bodies themselves require a greater or less portion of heat to occasion similar changes

* Abstract or abridgement from his writings, chiefly the VIIth Experimental Essay.

in their sensible heat. These important results constitute the foundation of the theory of the different capacities of bodies for heat, or their specific heats when at the same temperature.

Another discovery, of no less magnitude, seemed to render us acquainted with the principal causes by which the mutations of temperature are originally effected. It was found by experiment, not only that different bodies require greater or less quantities of heat to be communicated in order to raise their temperature through equal numbers of degrees, but also that the capacity of any individual substance is least when in the solid state, greater in the fluid state, and greatest of all when converted into elastic vapour or air. So that, as a natural consequence, a much greater portion of heat will be required to convert ice into water, and raise its temperature ten degrees, than would have been necessary to have equally elevated the temperature of cold ice without melting it. And so likewise the vaporization of water, without raising its temperature beyond the boiling-water point, requires a much larger quantity of heat than would have heated the same mass of water from the freezing to the boiling point.

A great number of interesting deductions, from these facts, have been applied to explain the general mutations of heat in the universe around us. For, in the first place, since every change of chemical combination is attended with a greater or less change of the capacities of the aggregate, it almost invariably happens that the temperature of the new compound is either above or below the common temperature of the atmosphere; and as the fusibilities of these compounds are also affected according to laws of which at present we know nothing, there is very frequently a change of temperature on this account also.

Such is the most luminous and beautiful theory of heat which our cotemporaries have begun to develop, and which is at present explained at considerable length in most elementary works. But as this subject appears to depend upon facts scarcely capable of being extended farther by hypothesis or analogical reasoning, it has happened that inductions experiments and applications, though of the greatest value, have been but slowly made. The effect of chemical operations on the temperature of bodies has been applied with great success to explain the act of combustion; and the general consequences of the change of capacity in the solid, fluid, and vaporous states have been considered with regard to their extensive influence upon the face of the globe. But those results which originate chiefly from the differences of conducting power in bodies with regard to heat, have scarcely constituted an object of direct enquiry among philosophers, excepting by Count Rumford in the Philosophical Transactions, and in his Experimental Essays, at present in the progress of publication. On the present occasion I shall not enter into the consideration of the extensive economical uses to which this philosopher has applied the results of his experiments and deductions, but shall chiefly confine the present memoir to the consequences which he has shewn to arise from the imperfect conducting power of fluids with regard to heat.

The free passage of heat, in all directions, through all kinds of bodies, has never yet been called in question, though the rapidity of its transmission is well known to differ exceedingly in various kinds of bodies. Under the influence of this opinion Count Rumford began his experiments on heat. His former experiments shewed that air is a non-conductor of heat; and the late experiments contained in his seventh Essay ascertain that water is in the same predicament. He thinks that all other fluids have the same property. He was led

to an experimental investigation of this curious subject by certain accidental events which he relates. These shewed, that when the circulation of water is impeded by mucilage or by fibrous matter, as in apple-pie and thick rice-soup, the heat is not only a long time in escaping, but may be very considerable in one part of the mass, while other parts are nearly cold. The baths of Baia afforded another instance of the same nature, which is very striking. When the Count was standing on the sea-shore, near the baths, where the hot steam was issuing out of every crevice of the rocks, and even rising up out of the ground, he had the curiosity to put his hand into the water. He was not surprised to find that the waves of the sea, which incessantly followed each other over the beach, should feel cold; but he was more than surprised when, on running the ends of his fingers into the sand beneath the water, he found the heat quite intolerable. The sand was perfectly wet, and yet the temperature was so very different at the small distance of two or three inches! He even found that the surface of the sand was to all appearance quite as cold as the water which flowed over it. He could not reconcile this to the supposed great conducting power of water; and then, for the first time, in consequence of his doubts respecting the existence or intensity of this power, he determined to make experiments to ascertain the fact. These however were delayed, and probably might have remained undone, if another unexpected appearance had not revived his curiosity.

In the course of a set of experiments on heat, Count R. had occasion to use thermometers of uncommon size, their globular bulbs being above four inches in diameter filled with various liquids. One of these, containing alcohol much heated, was placed in a window where the sun happened to be shining; when, casting his eyes on the tube, which was quite naked, and divided by means of a diamond, he saw the whole mass of its contents in a most rapid motion, in two opposite directions, up and down at the same time. These motions were rendered visible by some particles of fine dust that happened to be in the ball before it was filled. The tube was $\frac{1}{4}$ inch in diameter; and upon examination with a lens, the rising current of spirit was seen to occupy the axis or internal part of the tube, and the descending stream was contiguous to the sides. When the tube was inclined, the rising current occupied the uppermost side, and the descending stream the lower. The velocities were perceptibly increased by wetting the tube with ice-cold water; they became gradually less as the thermometer was cooled, and ceased when the fluid had acquired the common temperature of the room; and the motion was greatly prolonged when the cooling of the bulb was impeded by wrapping it in furs or any other warm covering.

The same experiments, with motion of the same kind, and quite as rapid, were repeated with a similar thermometer filled with linseed-oil.

From these facts Count Rumford was led to conclude that the fluids he had tried, and probably all others, are in fact non-conductors * of heat; that they transmit this matter

* Throughout this most valuable essay, the author speaks of fluids, and particularly water, as *perfect non-conductors of heat*; which seems to me to be an inaccurate expression of the facts. His experiments, hereafter to be related, may prove that the direct communication of heat through a line of particles of a fluid, on the supposition of their continuing immovable, is extremely slow; but not that it does not take place. If each individual particle were not capable of receiving and giving heat, the process by circulation could not happen; and if on the contrary they be capable, the propagation of heat from particle to particle is possible, and must doubtless happen; though from the facts it appears that the heat conveyed by the internal motion is very much greater than what passes in this way. Fluids then are very imperfect conductors of heat, but not perfect non-conductors. N.

of quality, by actual motions, from the heated to the cold bodies with which they may successively come into contact; and that every means of obstrucking or retarding those motions would render the propagation of heat more slow and difficult. He had before found that this was actually the case with air, and on the present occasion he proceeded to give the subject a thorough and careful investigation.

The apparatus made use of for the first course of experiments consisted of a large thermometer, which he calls the passage thermometer, consisting of a cylindrical vessel with hemispherical ends, forming the bulb, and a glass tube fitted into a neck by means of a good cork. Its dimensions were as follow:—Diameter of the bulb 1.84 inches—Length 4.99—Capacity or contents 13.2099 cubic inches—External superficies 28.834 superficial inches—Thickness of the sheet copper 0.03 inch—Weight when empty 1846 grains, and it contains 3344 grains of water at 55° of temperature. The glass tube is 24 inches long, and $\frac{1}{4}$ ths of an inch in diameter, and the cylindrical neck of copper into which it is fitted by means of cork is one inch long, and $\frac{1}{8}$ ths of an inch in diameter.

This thermometer, being filled with linseed-oil, and its scale graduated, was fixed in the axis of a hollow cylinder of thin sheet copper, $11\frac{1}{2}$ inches long, and 2.3435 inches in diameter internally. This cylinder, which is open at one end, is closed at the other with an hemispherical bottom with its convex side outwards. The bulb of the thermometer was confined in the axis of this case, by three small wooden pins, inserted in sockets within the large brass tube, and the upper end was properly secured by causing the glass tube to pass through a cork stopper adapted to the same metallic cylinder. The bottom of the bulb rested on a wooden pin fixed in a socket in the middle of the hemispherical bottom of the case. All the pins terminated in blunt wooden points, to reduce the contacts as much as possible.

The space between the thermometer when in its place, and the internal surface of the surrounding cylinder, was designed to contain the substance through which the heat was made to pass into or out of the thermometer; the temperature of this last mass being shewn by the graduations on the glass tube. The quantity of water required to fill the space, and cover the bulb of the thermometer about one quarter of an inch, was found to weigh 2468 grains.

When the bulb of the thermometer was surrounded in its place by water or any other liquid or mixture intended to be tried, a cylinder of cork, rather less in diameter than the internal cavity, was slipped down upon the tube, not quite so low as the water or mixture. Above this was placed a quantity of eider-down, sufficient to fill the remaining cavity, except what was occupied by the cork stopper, last of all to be inserted. The thermometer was divided according to Fahrenheit's scale, and the whole scale, from the freezing point to the boiling-water point, was above the stopper.

The operations with this apparatus were performed by placing the prepared instrument in melting ice till the thermometer fell to 32°. It was then immediately plunged into a large vessel of boiling water, and the conducting power of the substance under examination was estimated by the time the heat employed in passing through it into the thermometer; the time being carefully noted when the liquid in the thermometer arrived at the 40th degree of its scale; and also when it came to every 20th degree above it.

In the reverse operation, the instrument was kept in boiling water till its temperature appeared

appeared stationary, when it was taken out, and immediately plunged in melting ice, and the times of its descent carefully noted.

As soon as this apparatus was completed, the Count was desirous of ascertaining whether apples which continue hot so long in pies made of that fruit, do really possess a power of retaining heat greater than that of pure water, of which for the most part they consist; but in the first place he ascertained the quantity of fibrous matter in apples, by stewing two ounces troy of the pulp, and washing off the soluble matter with a large quantity of cold water. The fibrous remainder, when thoroughly dried, weighed only 25 grains, which, by remaining for several days in a plate on the top of a heated German stove, was further reduced to $18\frac{1}{2}$ ths, or less than 1-50th part of the whole mass.

The results obtained by surrounding the bulb of the passage thermometer with a quantity of stewed apples, so consistent as not to exhibit signs of fluidity, and then exposing the apparatus to the heating and cooling processes, are tabulated in the original Essay. The conducting power of the stewed apples proved to be little more than half that of pure water; that is to say, the heat was nearly twice as long in passing through the former as the latter.

As the attention of our author was steadily fixed on the position that heat is communicated by fluids, only by its being transported by virtue of their intestine motion produced by the change of specific gravity, he concluded that there may be two ways of obstructing this propagation of heat; namely, by diminishing their fluidity, which may be done by solution of any mucilaginous substance; or, more simply, by impeding the motion of their particles, which may be effected by mixing any solid substance with them which is an imperfect conductor of heat, and of an enlarged surface by being divided into small masses.

In the experiments with stewed apples, the passage of the heat in the water, which constituted by far the greatest part of the mass, was doubtless obstructed in both these ways. The mucilaginous part of the apples diminished very much the fluidity of the water, at the same time that the fibrous parts served to embarrass its motions.

In order to discover the comparative effects of these two causes, water was boiled with about 1-12th part of its weight of starch, and examined by the apparatus. The same weight of eider-down was also boiled with a like quantity of water. The intention of this last boiling was to free the eider-down from water. From the tabulated experiments, it appears that these several additions impaired the conducting power of water nearly to a degree of equality with the stewed apples.

In the Philosophical Transactions for 1792, where Count Rumford has ascertained that heat is actually propagated in air in the same manner as it is here stated to be propagated in water, he found that the thickness of a stratum of air, which served as a barrier to heat, remaining the same, the passage of heat through it was sometimes rendered more difficult by increasing the quantity of the light substance, which was mixed with it to obstruct its internal motion. To see if similar effects would be produced with water, he repeated the experiments with eider-down, reducing the quantity of it mixed with water to one-fourth of the quantity used in the former experiments. The resistance to the passage of heat was considerably diminished.

The results of these experiments are extremely interesting; they not only make us acquainted with a new and very curious fact, namely, that feathers, and other like substances, which

which in air are known to form very warm coverings for confining heat, may serve the same purpose in water, but that their effects in preventing the passage of heat are even greater in water than in air.

This discovery is very happily applied by the Count to elucidate some of the most interesting parts of the economy of nature, on which subject I shall give his inferences very nearly in his own words.

As liquid water is the vehicle of heat and nourishment, and consequently of life in every living thing; and as water, left to itself, freezes with a degree of cold much less than that which frequently prevails in cold climates, it is agreeable to the ideas we have of the Creator of the world, to expect that effectual measures would be taken to preserve a sufficient quantity of that liquid in its fluid state, to maintain life during the cold season: and this we find has actually been done; for both plants and animals are found to survive the longest and most severe winters: but the means which have been employed to produce this admirable effect have not been investigated; at least not as far as they relate to vegetables.

But as animal and vegetable bodies are essentially different in many respects, it is very natural to suppose that the means would be different which are employed to preserve them against the fatal effects which would be produced in each by the congelation of their fluids.

Among organized bodies which live on the surface of the earth, and which of course are exposed to the vicissitudes of the season, we find, that as the proportion of fluids to solids is greater, the greater is the heat which is required for the support of life and health; and the less are they able to endure any considerable change of their temperature.

The proportion of fluids to solids is much greater in animals than in vegetables; and in order to preserve in them the great quantity of heat which is necessary to the preservation of life, they are furnished with lungs, and are warmed by a process similar to that by which heat is generated in the combustion of inflammable bodies.

Among vegetables, those which are the most succulent are annual. Not being furnished with lungs to keep the great mass of liquids warm which fill their large and slender vessels, they live only while the genial influence of the sun warms them, and animates their feeble powers; and they droop and die as soon as they are deprived of its support.

There are many tender plants to be found in cold countries which die in the autumn, the roots of which remain alive during the winter, and send off fresh shoots in the ensuing spring. In these we shall constantly find the roots more compact and dense than the stalk, or with smaller vessels and a smaller proportion of fluids.

Among the trees of the forest, we shall constantly find that those which contain a great proportion of thin watery liquids not only shed their leaves every autumn, but are sometimes frozen and actually killed in severe frosts. Many thousands of the largest walnut-trees were killed by the frost in the Palatinate during the very cold winter in the year 1788; and it is well known that few, if any, of the deciduous plants of our temperate climate would be able to support the excessive cold of the frigid zone.

The trees which grow in those inhospitable climates, and which brave the cold of the severest winters, contain very little watery liquids. The sap which circulates in their vessels is thick and viscous, and can hardly be said to be fluid. Is there not the strongest reason to think that this was so contrived for the express purpose of preventing their being deprived of all their heat, and killed by the cold during the winter?

We have seen by the foregoing experiments, how much the propagation of heat in a liquid is retarded by diminishing its fluidity; and who knows but this may continue to be the case as long as any degree of fluidity remains?

As the bodies and branches of trees are not covered in winter by the snow which protects their roots from the cold atmosphere, it is evident that extraordinary measures were necessary to prevent their being frozen. The bark of all such trees as are designed by nature to support great degrees of cold, forms a very warm covering; but this precaution alone would certainly not have been sufficient for their protection. The sap, in all trees which are capable of supporting a long continuance of frost, grows thick and viscous on the approach of winter. What more important purpose could this change answer, than that here indicated? And it would be more than folly to pretend that it answers no useful purpose at all.

We have seen by the results of the foregoing experiments, how much the simple embarrasment of liquids in their internal motions tends to retard the propagation of heat in them, and consequently its passage out of them: and when we consider the extreme smallness of the vessels in which the sap moves in vegetables, and particularly in large trees;—when we recollect that the substance of which these small tubes are formed is one of the best non-conductors of heat known*;—and when we advert to the additional embarrasments to the passage of the heat which arise from the increased viscosity of the sap in winter, and to the almost impenetrable covering for confining heat which is formed by the bark, we shall no longer be at a loss to account for the preservation of trees during the winter, notwithstanding the long continuation of the hard frosts to which they are annually exposed.

On the same principles we may, I think, account, in a satisfactory manner, for the preservation of several kinds of fruit; such as apples and pears, for instance, which are known to support, without freezing, a degree of cold which would soon reduce an equal volume of pure water to a solid mass of ice.

At the same time that the compact skin of the fruit effectually prevents the evaporation of its fluid parts, which, as is well known, could not take place without occasioning a very great loss of heat, the internal motions of those fluids are so much obstructed by the thin partitions of the innumerable small cells in which they are confined, that the communication of their heat to the air ought, according to our hypothesis, to be extremely slow and difficult. These fruits do, however, freeze at last, when the cold is very intense; but it must be remembered that they are composed almost entirely of liquids, and of such liquids as do not grow viscous with cold; and moreover that they were evidently not designed to support for a long time very severe frosts.

Parsnips and carrots, and several other kinds of roots, support cold without freezing still

* I lately by accident had occasion to observe a very striking proof of the extreme difficulty with which heat passes in wood. Being present at the foundry at Munich when cannons were casting, I observed that the founder used a wooden instrument for stirring the melted metal. It was a piece of oak plank, green, or unseasoned, about ten inches square, and two inches thick, with a long wooden handle, which was fitted into a hole in the middle of it. As this instrument was frequently used, and sometimes remained a considerable time in the furnace, in which the heat was most intense, I was surprised to find that it was not consumed; but I was still more surprised, on examining the part of the plank which had been immersed in the melted metal, to find that the heat had penetrated it to so inconsiderable a depth, that at the distance of one-twentieth of an inch below its surface the wood did not seem to have been in the least affected by it. The colour of the wood remained unchanged, and it did not appear to have lost even its moisture. R.

longer than apples and pears; but these are less watery, and I believe the vessels in which their fluids are contained are smaller: and both these circumstances ought, according to our assumed principles, to render the passage of their heat out of them more difficult, and consequently to retard their congelation.

[*To be continued.*]

II.

Experiments and Observations on the fulminating Preparations of Gold and Silver.*

THE best process for the preparation of fulminating gold being shewn; the principles furnished by the acids during the solution of the gold, and those furnished by the ammoniac during the precipitation, and the order necessary in the application of them, were considered, and divers experiments were related, by which it appeared that this substance consists of oxygen, azote and hydrogen severally combined with caloric, and severally attached to the gold as a common base.

Twelve grains of this fulminating gold being placed in a conical heap on a thin plate of brass, were gradually heated. At a temperature between 300 and 400°, the whole exploded, with a very sharp and loud report, and the plate was pierced and torn. The round aperture was about an inch in diameter, and the lacerations extended much farther.

It was observed that the same effect might be produced by applying a spark to fulminating gold less heated, and that the accension of all such fulminating compounds, by a spark applied to any part, or by due augmentation of the temperature of the whole, is easily explicable on the grounds mentioned in the minutes of the last meeting, provided due attention be paid to the state of the active ingredients.

In fulminating gold, for instance, the attractive powers tending to produce the new combinations, which take place in the instant of combustion, seem to be almost equal to those by which the aggregation of this compound is maintained at low temperatures. For a small augmentation of temperature, or friction, or percussion, or any thing which disturbs the arrangement of the gaseous principles and caloric which adhere but weakly to the gold, is sufficient for the explosion of the whole, provided the fulminating gold be pure and dry.

In respect to the perforation of the brass plate, it was observed that all bodies which explode instantaneously would impress it in a similar manner. For the resistance of the air to projectiles or to expansions of this kind, being in the ratio of the squares of the velocities of the moving or expanding bodies, the resistance of the air to such instantaneous explosions as those lately mentioned, is almost equal to that of a solid, or of the metallic plate on which the fulminating body is heated.

SMALL portions of fulminating silver, each weighing less than a grain, were successively exploded, some by the touch of a slender brass wire, others by that of a feather.

* From the Minutes of a Society for Philosophical Experiments and Communications: B. Higgins, M. D. Operator. I do not hear that the Society is continued since the departure of the able Operator for the West-Indies. N.

A description was given of many other specimens which had been inadvertently exploded and lost; some by overheating them to about 90° , in the place where they were to be dried; and others by an accidental concussion of a great iron plate, on which they were placed in separate cups.

The experimenter said, that he had often exploded fulminating silver, in covered vessels of the smallest capacity that could be used with safety; and that this substance had frequently exploded unexpectedly in his hand, with a report louder than that of a musket.

A luminous and momentary gleam was always visible, but he could not discover any other adequate effect of the emitted caloric; and therefore he concluded, that in such instantaneous explosions the caloric was expelled with velocity sufficient to constitute light.

Mr. Berthollet, the inventor of fulminating silver, having contented himself with a general and concise description of this subject, many practical chemists have failed in their attempts to prepare it; and others, forming their opinions from the specimens which they had made, have been exposed to great danger; as will appear from the following relation, which is the only part of the Minutes on this subject that can be introduced in the present publication.

An ounce of fine silver was dissolved in the course of eight hours in an ounce of pure nitrous acid, of the London Pharmacopœia, diluted previously with three ounces of distilled water, in a glass matrass. The solution being poured off, the residuary black powder and the matrass were washed with seven or eight ounces of warm distilled water, and this was added to the solution. The black powder being gold was rejected; some gold being thus separable from any silver of commerce.

To the foregoing diluted solution, pure lime-water prepared with distilled water was added gradually; for the solution ought not to be poured into the lime-water. When about thirty pints of lime-water had been expended, and the precipitate had subsided, more lime-water was added, by successive pints, as long as it caused any precipitation. For it was deemed fitter that the precipitation should not be perfected, than that an excess of lime-water should be used; the earthy pellicle of the excessive lime-water being apt to mix with the precipitate. The clear liquor being poured away, the precipitate was poured off, and washed into a filter.

When the saline liquor had drained from it, two ounces of distilled water were poured on the magma; and when this water had passed, fresh portions were successively added and passed, until the whole quantity of water thus expended in washing away the nitrous calcareous salt amounted to a quart.

The filter being then unfolded, to let the magma of oxide of silver spread on the flattened paper, it was placed on a chalk-stone to accelerate the exsiccation, and was gradually dried in the open air; a cap of paper being placed loosely over it to exclude the dust.

When the weather served, the cap was removed, to expose the oxide to the rays of the sun; although this was not deemed necessary; and the exsiccation was promoted by cutting the oxide into thin slices. When perfectly dry it weighed 1 oz. 4 dwts. and about one-fifth of it was considered as oxygen.

“When aqua ammoniac pure of any Pharmacopœia is used with this oxide, either in the small quantity which blackens it completely, or in a greater quantity; the black matter which subsides, and which has been represented by systematic writers as the fulminating

compound, has no such property, any farther than may be owing to the matter deposited from the alkaline solution during the exsiccation.

"The alkaline liquor containing the fulminating silver ought to be poured off from the insoluble powder, and exposed in a shallow vessel to the air. In consequence of the exhalation, black shining crystals form on the surface only, and soon join to form a pellicle. As this pellicle adheres a little to the sides of the vessel, or maintains its figure, the liquor may be poured off by a gentle inclination of the vessel.

"This liquor will yield another pellicle in the same way; but the third or fourth pellicle will be paler than the former, and weaker in the explosion. The first pellicles, when slowly dried, explode by the touch of a feather, or by their being heated to about 96°.

"The quantity of water in the ordinary aqua ammoniac renders it less active in the solution of the oxide, and is an impediment to the speedy formation and separation of the fulminating silver; and an experimenter who has often used twenty grains of the oxide to produce successive pellicles of fulminating silver, which may be separately exploded with safety, and who has perceived that the pellicles never explode whilst wet, if they be not heated, would in all probability resolve on the following improvement, and expose himself to the unforeseen danger of it."

DISTILLED water was impregnated with as much pure ammoniac as it could easily retain under the ordinary temperature of the air. A quantity of this strong ammoniacal liquor, equal in bulk to a quarter of an ounce of water, was placed in a small bottle, and 24 grains of the oxide of silver, ground to fine powder, were added. The bottle, being almost filled, was corked, to prevent the formation of that film which usually appeared in consequence of the exhalation of the ammoniac in other experiments.

During the solution of the oxide, bubbles of the gaseous kind arose from it, and the solution acquired a blue colour.

As no film appeared, the bottle was agitated three or four times in the course of as many hours, in order to promote the solution of a small quantity of blackened oxide which remained at the bottom.

The experimenter considering this as an ample provision for twenty different charges, to be exploded in different circumstances, in the presence of the society, intended to pour off the solution into as many small vessels, and to weigh the residuary black powder, after allowing two hours more for the solution.

On the sixth hour he took his usual precaution of wearing spectacles; and observing that a small quantity of black powder still remained undissolved, and that no film was yet formed at the surface, he took the bottle by the neck to shake it; knowing that it might explode by the heat of his hand, if he were to grasp it, and that the explosion in this circumstance might wound him dangerously.

In the instant of shaking, it exploded with a report that stunned him. The bottle was blown into fragments so small as to appear like glass coarsely powdered. The hand which held it was impressed as by the blow of a great hammer, and lost the sense of feeling for some seconds; and about fifty-two small grains of glass were lodged, many of them deeply,

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in the skin of the palm and fingers. The liquor stained his whole dress, and every part of the skin that it touched.

Thus it appeared that fulminating silver may be made which will explode even when cold and wet, by the mere disturbance of the arrangement of its parts, in the aqueous fluid.

In subsequent experiments, privately and carefully conducted, it seemed that the property of exploding in the cold liquor, by mere commotion, depended on the unusual quantity or proximity of the explosive molecules in a given bulk of the liquor. And the flat bottoms, as well as the sides, of the thick vessels of glass or potters-ware, whether they stood on boards or on iron plates, were always beaten to small fragments.

This afforded a curious instance of the possible equilibrium between the powers tending to retain the caloric, and those which effect the expulsion of it; and experiments and considerations of this kind seemed to promise a true solution of the phenomena of Rupert's drops.

SMALL charges, each consisting of a grain of oxygenated muriate of pot-ash, finely powdered, and mixed with an equal quantity of flowers of sulphur, were exploded by mere trituration. And Mr. Godfrey's relation of the danger of keeping such a mixture in a bottle was duly noticed; for, after he had kept it thus for some time, he found that it had exploded spontaneously.

III.

Experiments and Observations made with the View of ascertaining the Nature of the Gas produced by passing Electric Discharges through Water; with a Description of the Apparatus for these Experiments. By GEORGE PEARSON, M. D. F. R. S.

[Continued from page 248.]

SECTION II.

EXPERIMENTS.

FROM my Journal of the numerous experiments made during the course of nearly two years, I shall select those which will serve to explain the nature of the process, and shew the power of the plate electrical machines; and I shall particularly relate those experiments which afforded the most useful results concerning the nature of the gas obtained.

1. With interrupted Discharges.

Experiment A. About 1600 of these discharges by means of a 34 inch single plate electrical machine, in nearly three hours, produced, from New River water taken from the cistern, and which had not been freed from air by the air pump or boiling, a column of gas, two-thirds of an inch in length, and 1-9th of an inch wide. On passing through this gas, between the two wires of the tube in which it was produced, a single electrical spark, its bulk was instantly diminished to two-thirds. In other experiments the bulk of gas was only diminished to about one half. And the result was the same with distilled water.

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B. The experiment A being repeated several times with distilled, and New River water, freed from air by the air pump or long boiling, the quantity of gaz just mentioned was obtained in about four hours.

On passing an electric spark through this gaz in the situation above mentioned, its bulk was instantly diminished in some cases 15-16ths, and in others 19-20ths.

C. 1600 interrupted discharges, by means of a 32 inch plate machine, produced from New River water, and distilled water, freed from their air by the air pump, a column of gaz, about 3-4ths of an inch in length and 1-9th of an inch in diameter, in the space of three hours. It was reduced in bulk 19-20ths by passing through it a single electrical spark.

D. 500 revolutions of the 32 inch plate machine, in three quarters of an hour, produced 600 interrupted discharges in river water freed from air by the air pump, by which a column of gaz, half an inch in length and 1-10th of an inch in diameter, was obtained. It was diminished as usual by an electric spark 19-20ths of its bulk.

E. Nearly four days incessant labour with the 32 inch machine produced only 56,5488 cubes of gaz, of 1-10th of an inch each, on account of the usual accidents during the process. The air had been exhausted by setting the water under the receiver of the air pump.

F. It was found that 6000 interrupted discharges produced about three inches in length of gaz, measured in a tube 3-20ths of an inch in width from water out of which its air had been drawn by the air pump.

G. It appeared from many experiments, that the same unboiled water, or water from which the air had not been exhausted by the air pump, which had repeatedly yielded gaz by passing through it electrical discharges, always left a residue of gaz which the electrical spark did not diminish; and this residue was in nearly the same quantity after six or seven experiments, each of which afforded a column of gaz half an inch in length and 1-9th of an inch in diameter, as was left on passing the electric spark through the gaz afforded by the third or fourth experiment.

Hence it seems that water is decomposed by the electric discharge, before the whole of the common or atmospherical air is detached from the water by merely the impulse of each discharge. Yet I think it probable that, after the discharges have been passed through the same water for a certain time, the whole of the air contained in water will be expelled, and no gaz be produced, but that compounded by means of the electric fire from water; in which case, supposing the gaz so produced to be at least merely hydrogen and oxygen gaz, it will totally disappear on passing through it an electrical spark. But I have never been able to determine this point, because the tubes were always broken after obtaining a few products, or long before it could reasonably be supposed the whole of the air of the water was expelled from it.

H. To the gaz obtained in the experiment E was added, over water, an equal bulk of almost pure nitrous gaz. Fumes of nitrous acid appeared, and the gaz examined was reduced almost one-third of its bulk. A small bubble more of nitrous gaz being let up, no further diminution took place. To this residue was added half its bulk of oxygen gaz, obtained from oxy-muriate of pot-ash. This mixture of gazes having stood several days over well burnt lime and boiled quicksilver, an electric spark was passed through the mixture over quicksilver, by which its bulk was instantly diminished one-fourth. But no moisture could be perceived upon the sides of the tube or on the quicksilver. The failure of the appearance of

of moisture was imputed to a bit of lime accidentally left in the tube which was burst by the explosion, and dispersed through the tube; or else the quantity of water produced was so small, comparatively with the residuary gaz, that the water was dissolved by it in the moment of its composition. For, supposing water to have been compounded, it could not amount to the 1-100th part of a grain, and the residuary gaz was at least 2000 times this bulk.

That a quantity of water can be compounded under the same circumstances as in this experiment, and be apparently dissolved in air, so as to escape observation, even with a lens, was proved by passing an electric spark through a mixture of hydrogen and oxygen gaz, well dried by standing over lime.

2. With complete or uninterrupted Discharges.

THE gaz obtained by the first described kind of apparatus for the uninterrupted discharges, p. 145, and Fig. 6, and 7, always left a residue of at least one-fourth of its bulk, on passing through it the electric spark; even when water was used which had been freed from air by boiling or the air pump. Nor will this result appear surprising, when it is considered how liable the water in this apparatus is to mix and absorb air during the experiment. However, this method would have been extremely valuable, if the next other method had not been discovered; for gaz may be obtained by it with fewer accidents, and much more rapidly, than with the interrupted discharges. The apparatus is also much more easily fitted up, and is more simple. But I think it unnecessary to particularly relate any experiments, as they afforded the same results as those already described, and as those next to be related.

The following experiments were made with the apparatus described p. 146, and shewn by Fig. 8, 9, and 10:

Experiment I. At 10 h. 40' P. M. began to produce discharges with a double plate twenty-four inch machine, in water taken from the cistern; and at 12 h. 6' P. M. of the same day there had been written down 10200 discharges, each of which occasioned air to ascend from the bottom of the wire and brass cup. The quantity of air obtained was now apparently about one-fourth of a cubical inch, and it occupied nearly half of the tube, the water in which was by this time very muddy.

After standing till the day following at noon, when the process was again commenced, it did not appear that any of the gaz had been absorbed by the water over which it stood.

At 2 h. 35' P. M. began to produce discharges, and at 8 h. P. M. had passed 6636; which, together with those of the preceding day, amounted to 16,836. The tube was now 5-8ths full of gaz, and there seemed to be almost half a cubical inch; for it was observed that the gaz was this day yielded at double the rate it had been the day before. This was accounted for from the diminished pressure upon the electric fire, by the tube containing gaz instead of water.

At this time, namely at 8 h. P. M. I was surprised on the passing of a discharge by a vivid illumination of the whole tube, and a violent commotion within it, with, at the same time, the rushing up of water, instantly to occupy rather more than 5-8ths of the space which had been occupied by gaz.

The residue of gaz was not diminished further by an electric spark; and to the test of nitrous gaz it appeared to be rather worse than atmospheric air, as it consisted of rather less than one part of oxygen, and three parts of nitrogen or azotic gaz.

It seemed as if the electrical discharge had kindled the oxygen and hydrogen gaz of the decompounded gaz, by flying from the bottom of the wire to the brass funnel, so that the fire returned into the tube where it passed through the gaz. Or the combustion might be occasioned by a chain of bubbles reaching from the brass dish to the surface of the water in the tube, which was set on fire in its ascent, and thus produced combustion of the whole of the gaz of decompounded water.

That this phenomenon was from the combustion here supposed, was in some degree proved, by finding that the mixture of hydrogen gaz and atmospherical air, under the same circumstances, was kindled in the same manner.

Experiment II. With a double plate electrical machine, 24 inches in diameter, and a similar apparatus to that in the last experiment, 14,600 discharges produced at least one-third of a cubical inch of gaz. While I was measuring with a pair of compasses the quantity of gaz produced, the points of them being in contact with the part of the tube occupied by gaz, I was again surprised, on the passing of a discharge, by an illumination of the whole tube, and the rushing up with considerable commotion of water, to occupy about two-thirds of the space filled by gaz.

The residuary air was found, as in the former experiment, to be rather worse than atmospherical air.

It was concluded that the points of the compasses had attracted electrical fire from the wire to the sides of the glass, and thereby kindled the hydrogen and oxygen gaz of decompounded water. But to determine this question, I introduced into the same tube a mixture of one measure of oxygen and two measures of hydrogen gaz, to occupy nearly the same space in the tube as the gaz had occupied; then passing an electrical discharge through it, no combustion was excited; but on passing a discharge, while the compasses were in contact with the tube, as just mentioned, an illumination and violent commotion were produced, with the rushing up of water, to leave only 1-8th of the gaz as a residue. On repeating this experiment with two measures of atmospherical air, and one of hydrogen gaz, combustion could not be excited; nor with one measure of atmospherical air, and two of hydrogen; but on adding to this last mixture one measure of oxygen gaz, the electrical discharge produced the phenomena of combustion just mentioned, with the rushing up of water, to occupy about two-thirds of the space which was occupied by the gazes.

Experiment III. Having passed 12,000 discharges through water, with the apparatus of the preceding experiment, and thereby obtained only one-fifth of a cubical inch of gaz; and having observed that the quantity of gaz was not greater than it was when only 8000 discharges had been passed, and yet bubbles had been seen to be produced on each discharge, as copiously, or more so, by the last three or 4000 discharges, as before; I began to suspect that part of the gaz had been destroyed during the process, or had been absorbed. While I was considering how to account for this disappearance of gaz, and was at the same time looking at the tube through which the discharges were passing, I observed one of them to be attended with a diminution, instantly, of about one-fifth of the gaz produced, and with a slight explosion. I was now sure, from this phenomenon, and from the unequal augmentation of the bulk of the gaz at given times during the process, that combustion had been excited several times before, not only in the present experiment, but perhaps in the former ones, without observing it. I conceived that a gradual combustion also very probably

bably took place in this process by the kindling of bubbles of gaz in their ascent through the water. I now perceived that the discharges ought to be produced more slowly, or the tubes to be wider, to allow the bubbles to pass quite through the water, in order to avoid the ascension of gaz during the process. My calculation, also, that 35 to 40,000 discharges were requisite to produce one cubical inch of gaz from water containing its usual quantity of common air, was rendered much more vague by this ascension, so often liable to be occasioned.

To the gaz which remained in the tube in this experiment was added an equal bulk of nitrous gaz; the mixture diminished to 1,5; and on adding to the residue half its bulk of oxygen gaz, and passing through it the electrical spark, no accension or diminution of bulk was produced. Hence all the hydrogen gaz and oxygen gaz, produced by the decomposition of the water, had been burnt during the process; the oxygen gaz thus detected being considered to be only that expelled from the water.

Experiment IV. By means of electrical discharges with the apparatus used in the preceding experiment, I obtained gaz from New-River water, letting it up into a reservoir, as soon as about 1-20th of a cubic inch was produced, till I had collected 1-8th of a cubic inch. To this was added an equal bulk of nitrous gaz, on which the mixture diminished to 1,2; and on the addition of a little more nitrous gaz, no further diminution took place. To this residue half its bulk of oxygen was added; and this mixture of gazes being well dried, by standing over lime and boiled quicksilver, an electric spark was passed through it, by which a diminution of one-sixth of its bulk took place. A little dew was then seen upon the sides of the tube where the quicksilver had risen; and with the aid of a lens the same appearance was perceived on the part of the tube containing the residue of gaz.

It may now be expected, that I should have made the experiment with this apparatus on distilled water, freed from its air, not only by long boiling or the air pump, but by sending through it several hundred electrical discharges. It would also have been, to some persons, more satisfactory, if the experiment had been made upon a larger scale, so as to have produced the combustion of a much larger quantity of gaz, and consequently have produced a greater quantity of water. As, however, I apprehend, the experiments contained in this paper, when well considered by competent judges, will be found to explain the nature of the gaz procured from water by electric discharges; and as another very important subject demands my attention, the honour of more splendid and convincing experiments must be reserved for other enquirers. If the same sacrifices be made by them, as have been made in performing the present experiments, I think it is scarcely possible but that still further light concerning the composition of water should be procured, as well as concerning oils, alcohol, acids, &c.; to the investigation of the composition of which, the mode of analysis and synthesis here indicated may be applied.

SECTION III.

On the Mode of Action of Electric Discharges.

THE mere concussion by the electric discharges, appears to extricate not only the air dissolved in water, which can be separated from it by boiling and the air pump, but also that which

which remains in water, notwithstanding these means of extricating it have been employed.

The quantity of this air varies in the same, and in different waters, according to circumstances. New-River water from the cistern yielded one-fifth of its bulk of air, when placed by Mr. Cuthbertson under the receiver of his most powerful air pump; but in the same situation, New-River water taken from a tub exposed to the atmosphere for some time yielded its own bulk of air. Hence the gaz procured by the first one, two, or even three hundred explosions in water containing its natural quantity of air, is diminished very little by an electric spark.

The gaz thus separable from water, like atmospheric air, consists of oxygen and nitrogen, or azotic gaz; which may be in exactly the same proportions as in atmospheric air; for the water may retain one kind of gaz more tenaciously than the other; and on this account the air separated may be better or worse than atmospheric air at different periods of the process for extricating it.

With regard to the gaz which instantly disappears on passing through it an electric spark, its nature is shewn by (a) this very property of thus diminishing; and by the following properties:

(b) A certain quantity of nitrous gaz instantly disappeared, apparently composing nitrous acid, on being added to the gaz (a) p. 303, Exp. IV.

Oxygen gaz being added to the residue after saturation with nitrous gaz, and an electric spark being applied to the mixture of gazes, well dried, a considerable diminution immediately took place, and water was produced.

(c) Combustion from hydrogen and oxygen gaz took place when the tube was about three-fourths full of gaz, p. 301, Exp. I; which was confirmed by passing an electric discharge, under the same circumstances, through a mixture of hydrogen and oxygen gaz, p. 302, Exp. II.

(d) Combustion from hydrogen and oxygen gaz took place when the points of the compasses were accidentally applied to the part of the tube containing gaz, p. 302; which was confirmed by passing a discharge, under the same circumstances, through a mixture of hydrogen and oxygen gaz, while the points of the compasses were applied to the tube.

(e) The observations made of the kindling of gaz, in small quantities, from time to time, during the process of obtaining it, particularly while it was ascending in chains of bubbles, or was adhering to the funnel of the tube, p. 302, Exp. III. confirm the evidence in favour of this gaz being hydrogen and oxygen gaz.

The evidence contained under the above heads (a) — (e) considered singly and conjunctively, I apprehend, must be admitted by the most rigorous reasoner, or severest logician, to be demonstrative that hydrogen and oxygen gaz were produced by passing electric discharges through water.

With regard to the origin and mode of production of these two gazes, our present observations and experiments do *not* afford complete demonstrative evidence; but although some hypotheses must be admitted, I conceive that the body of evidence we possess can afford a satisfactory interpretation of the phenomena.

It is demonstrable that the electric discharge and spark contain fire; and very probably they are merely a state of fire. Fire may be considered as consisting of caloric and light; but it is at least as consistent with the phenomena, and it is more philosophical, because it is

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more simple, to consider light not as a distinct species of matter, but as a state of caloric, which is manifested by its producing the sensation termed vision. It is demonstrable also, that the ponderable parts of oxygen and hydrogen gas constitute water. There is strong evidence that these gazes consist of a peculiar species of matter which is ponderable; and of imponderable matter, which is that which is separable from them in the state of fire, or flame. If fire could be applied in a sufficiently dense state and quantity, it is warrantable, from a full induction of facts, to conclude, that it is able to disunite the constituent substances of all the compound substances in nature.

[To be concluded in the next Number.]

IV.

Experimental Researches to ascertain the Nature of the Process by which the Eye adapts itself to produce distinct Vision.

THE structure of the eye, and particularly its provisions for adjustment to produce distinct vision, have engaged the attention of several philosophers during the last five years, who have delivered papers on these subjects to the Royal Society. I purpose to give the substance of their discoveries in the present communication.

In the year 1793, Mr. Thomas Young's Observations on Vision appeared in the Transactions. He gives a short summary of the theories of adjustment proposed by various earlier authors. Kepler supposed the ciliary processes to contract the diameter of the eye, and lengthen its axis by a muscular power. Descartes imagined the same effect to be produced by a muscularity of the crystalline humour, but did not attribute much to the change of figure which he supposed to take place in that lens. De la Hire, and also Haller, adopted the opinion that the eye undergoes no change to produce distinct vision, but the contraction and dilatation of the pupil. Pemberton supposed the existence of muscular fibres in the crystalline humour, by which the curvatures of its surfaces are changeable. Dr. Porterfield conceived that the ciliary processes draw forward the crystalline, and render the cornea more convex. Dr. Jurin maintained the hypothesis, that the uvea, at its attachment to the cornea, is muscular, and capable of increasing the convexity of that humour, by its contraction towards the axis of the eye. Muschenbroek conjectures that the relaxation of his ciliary zone, which appears to be nothing but the capsule of the vitreous humour, permits the coats of the eye to push forward the crystalline and cornea. And, lastly, an elongation of the axis of the eye has been supposed to be produced by the pressure of the external muscles, especially the two oblique muscles; and, on the other hand, the muscular action has been supposed to produce a contrary effect, namely, a contraction of the axis.

In the enumeration of these respective hypotheses, Mr. Young makes remarks tending to their several refutation. I have not transcribed them, principally because they chiefly tend to shew that effects of this nature deserve, if possible, to be submitted to actual experiment.

From the consideration of the whole subject, our author concluded that the rays of light emitted by objects at a small distance could only be brought to foci on the retina by a

nearer approach of the crystalline to a spherical form; and he could imagine no other power capable of producing this change than a muscularity of part or the whole of its capsule.

But on closely examining, with the naked eye, the crystalline from an ox turned out of its capsule, he discovered a structure which he thinks sufficient to remove all the difficulties with which this branch of optics has long been obscured. The crystalline lens of the ox is an orbicular, convex, transparent body, composed of a considerable number of similar coats, of which the exterior closely adhere to the interior. Each of these coats consists of six muscles, intermixed with a gelatinous substance, and attached to six membranous tendons. Three of the tendons are anterior, three posterior; their length is about two-thirds of the semi-diameter of the coat; their arrangement is that of three equal and equidistant rays, meeting in the axis of the crystalline; one of the anterior is directed towards the outer angle of the eye, and one of the posterior towards the inner angle; so that the posterior are placed opposite to the middle of the interstices of the anterior; and planes passing through each of the six, and through the axis, would mark on either surface six regular equidistant rays. The muscular fibres arise from both sides of each tendon; they diverge till they reach the greatest circumference of the coat; and having passed it, they again converge till they are attached respectively to the sides of the nearest tendons of the opposite surface. The anterior or posterior portion of the six viewed together exhibits the appearance of three penniform-radiated muscles. The anterior tendons of all the coats are situated in the same planes, and the posterior ones in the continuations of these planes beyond the axis. Such an arrangement of fibres can be accounted for on no other supposition than that of muscularity. This mass is enclosed in a strong membranous capsule, to which it is loosely connected by minute vessels and nerves; and the connection is more observable near its greatest circumference. Between the mass and its capsule is found a considerable quantity of an aqueous fluid, the liquid of the crystalline.

He conceives, therefore, that when the will is exerted to view an object at a small distance, the influence of the mind is conveyed through the lenticular ganglion formed from branches of the third and fifth pairs of nerves, by the filaments perforating the sclerotica to the orbiculus ciliaris, which may be considered as an annular plexus of nerves and vessels; and thence, by the ciliary processes, to the muscle of the crystalline; which, by the contraction of its fibres, becomes more convex, and collects the diverging rays to a focus on the retina. The disposition of fibres in each coat is admirably adapted to produce this change; for since the least surface that can contain a given bulk, is that of a sphere (Simpson's Fluxions, p. 486), the contraction of any surface must bring its contents nearer to a spherical form. The liquid of the crystalline seems to serve as a synovia in facilitating the motion, and to admit a sufficient change of the muscular part, with a smaller motion of the capsule.

To ascertain whether these fibres can produce an alteration in the form of the lens sufficiently great to account for the known effects, this author states, that the diameter of the crystalline of the ox is 700 thousandths of an inch, the axis of its anterior segment 225; of its posterior 350. In the atmosphere it collects parallel rays, at the distance of 235 thousandths. From these data he finds, by means of Smith's Optics, article 366, and a quadratic, that its ratio of refraction is as 10000 to 6574. Hauksbee makes it only as 10000 to 6832.7; but we cannot depend on his experiment, since he says, that the image of the candle which he viewed was enlarged and distorted; a circumstance that he does not explain,

explain, but which was evidently occasioned by the greater density of the central parts. Supposing, with Hauksbee and others, the refraction of the aqueous and vitreous humours equal to that of water, viz. as 10000 to 7465, the ratio of refraction of the crystalline in the eye will be as 10000 to 8806, and it would collect parallel rays at the distance of 1226 thousandths of an inch; but the distance of the retina from the crystalline is 550 thousandths; and that of the anterior surface of the cornea 250; hence (by Smith, art. 367.) the focal distance of the cornea and aqueous humour alone must be 2329. Now supposing the crystalline to assume a spherical form, its diameter will be 642 thousandths, and its focal distance in the eye 926. Then, disregarding the thickness of the cornea, he deduces (by Smith, art. 370.) that such an eye will collect those rays on the retina which diverge from a point at the distance of 12 inches and 8-10ths. This is a greater change than is necessary for an ox's eye: for, if it be supposed capable of distinct vision at a distance somewhat less than 12 inches, yet it probably is far short of being able to collect parallel rays. The human crystalline, he says, is susceptible of a much greater change of form.

The ciliary zone may admit of as much extension as this diminution of the diameter of the crystalline will require; and its elasticity will assist the cellular texture of the vitreous humour, and perhaps the gelatinous part of the crystalline, in restoring the indolent form.

He questions whether the retina takes any part in supplying the lens with nerves; but from the analogy of the olfactory and auditory nerves, he thinks it more reasonable to suppose that the optic nerve serves no other purpose than that of conveying sensation to the brain.

Although a strong light and close examination are required in order to see the fibres of the crystalline in its entire state, yet their direction was demonstrated, and their attachment shewn, without much difficulty. In a dead eye, the tendons are discernible through the capsule, and sometimes the anterior ones even through the cornea and aqueous humour. When the dry crystalline falls, it very frequently separates, as far as the centre, into three portions, each having a tendon in its middle. If it be carefully stripped of its capsule, and the smart blast of a fine blow-pipe be applied close to its surface, in different parts, it will be found to crack exactly in the direction of the fibres above described, and all these cracks will be stopped as soon as they reach either of the tendons. The application of a little ink to the crystalline is of great use in shewing the course of the fibres.

Mr. Young was not at first aware that the muscularity of the crystalline had ever been suspected, either by Descartes or any other person. But the laborious and accurate Lewenhoeck, to whose writings he refers, has described the course of the fibres of the crystalline in a variety of animals, and has even gone so far as to call it a muscle. He did not, however, attempt to account for the focal adjustment of the eye from its muscularity.

The remaining part of Mr. Young's paper is employed in the solution of some optical queries, not immediately relating to the present object.

In Plate XIII, Fig. 1. represents a vertical section of the ox's eye, of the natural size. A, the cornea covered by the tunica conjunctiva; BCB, the sclerotica, covered at BB by the tunica albuginea and tunica conjunctiva; DD, the choroid, consisting of two laminas; EE, the circle of adherence of the choroid and sclerotica; FG, FG, the orbiculus ciliaris;

HI, HK, the uvea; its anterior surface the iris; its posterior surface lined with pigmentum nigrum; IK, the pupil; HE, HL, the ciliary processes covered with pigmentum nigrum; MM, the retina; N, the aqueous humour; O, the crystalline lens; P, the vitreous humour; QR, QR, the zona ciliaris; RS, RS, the annulus mucosus.

Fig. 2. The structure of the crystalline lens, as viewed in front, likewise of the natural size;—Fig. 3. a side view of the crystalline.

In the following year, 1794, a communication was made to the Royal Society, by Everard Home, Esq. F. R. S. of some facts relative to a preparation for the Croonian Lecture, by the late Mr. John Hunter. He states that this celebrated anatomist had for many years entertained the notion, that the crystalline humour was enabled by its own internal actions to adjust itself so as to adapt the eye to different distances; and when the tænia hydatigena first came under his observation as a living animal, he was surprised to see the quantity of contraction that took place in a membrane devoid of muscular fibres; but made use of the fact in his investigation of the structure of the crystalline humour of the eye. Some time after this, he discovered the fibrous structure of the crystalline humour in the eye of the cuttle-fish, in which it is peculiarly distinct; and thence he was led to consider the exterior part of this humour as similar in all animals. It was his intention to have ascertained by experiment, whether muscular action does in fact take place; and having found that a certain degree of heat applied through the medium of water will excite muscular action, after almost every other stimulus had failed, it was proposed to apply this to the crystalline humour, and ascertain its effects. The crystalline humour taken from animals recently killed must be considered as being still alive. Such humours were to be immersed in water of different temperatures, and placed in such a manner as to form the image of a lucid, well-defined object, by a proper apparatus for that purpose, so that any change of the place of that image, from the stimulating effects of the warm water upon the humour, would be readily ascertained. These were the experiments which Mr. Hunter had instituted and begun, but in which he had not made sufficient progress before his death to enable him to draw any conclusions.

This communication contains an unfinished letter from Mr. Hunter to Sir Joseph Banks, containing some prefatory observations leading to the statement made by Mr. Home. It appears, in fact, as that gentleman remarks, that the discovery of a fibrous appearance in the crystalline appertains to Lewenhoeck; but that the discovery of an eye in which the structure is uncommonly distinct, is due to Mr. Hunter.

To this paper is annexed a plate exhibiting two sections of the crystalline humour of the cuttle-fish.

In the second part of the Transactions for the same year, I find a paper of some length on vision, by Dr. Hossack. This author shews in the first place, that the enlargement or contraction of the pupil of the eye is insufficient to produce the adjustment by which the rays of light shall converge to a point in the retina; and in fact a slight observation of what happens is sufficient to shew that the variations of the pupil are governed by different circumstances. He controverts Mr. Young's deductions with regard to the muscularity of the crystalline, and even disputes the facts. He quotes some authorities to shew that the eye is capable of accommodating itself to different distances without the assistance of the crys-

talline, as after couching or extraction. For these and other reasons his attention was more particularly directed to the external muscles.

Upon carefully removing the eyelids, the muscles of the eye present themselves to view in number six; four called recti or straight, and two oblique, so named from their direction. In Plate XIII. Fig. 4, AAAAA represent the tendons of the recti muscles, where they are inserted into the sclerotic coat, at the anterior part of the eye. B, the superior oblique, or trochlearis, as sometimes called, from its passing through the loop or pulley connected to the lower angle of the orbit or notch in the os frontis; it passes under the superior rectus muscle, and backwards to the posterior part of the eye, where it is inserted by a broad flat tendon into the sclerotic coat. C, the inferior oblique, arising tendinous from the edge of the orbit or process of the superior maxillary bone, passing strong and fleshy over the inferior rectus, and backwards under the abductor to the posterior part of the eye, where it is also inserted by a broad flat tendon into the sclerotic coat. DDD, the fat in which the eye is lodged. In Figure 5, the bones forming the external side of the orbit, with a portion of the fat, are removed, by which we have a distinct view of the abductor. ABC, three of the recti muscles, arising from the back part of the orbit, passing strong, broad and fleshy over the ball of the eye, and inserted by flat broad tendons into the sclerotic coat, at its interior part. D, the tendon of the superior oblique muscle. E, the inferior oblique. In Fig. 6, A represents the abductor of the eye. B, the fleshy belly of the superior oblique, arising strong, tendinous and fleshy from the back part of the orbit. C, the optic nerve. D and E, the recti muscles.

The use ascribed to these different muscles is that of directing the axis of the eye towards the different objects, or to express the passions of the mind. But Dr. Hossack rationally infers, from the general application of the combined forces of muscles through the whole of the animal system, that this set of muscles, which he conceives to be well adapted to produce the focal adjustment, may also be employed on that object. He assumes, as the necessary consequence of contraction in these muscles, that the axis of the eye will be elongated, and the elastic cornea rendered more convex; both which circumstances would tend to preserve distinctness of vision with regard to near objects. For as such objects afford a focal image more distant from the refracting surface, through which the light may pass, the elongation of the axis will be of advantage, by removing the retina further back; and the increased convexity and shortened focus of the cornea will conduce to the same end.

How far the action of the recti muscles might produce an elongated figure, is perhaps capable of dispute; though this effect will probably be admitted without hesitation, as a consequence of the contraction of the oblique muscles. But to put the matter out of doubt, whether this organ be capable of having its focal adjustment considerably varied by external pressure, our author applied the common speculum oculi to his own eye. With a very moderate pressure, while directing his attention to an object at the distance of about twenty yards, he saw it distinctly, as also the different intermediate objects; but endeavouring to look beyond it, every thing appeared confused: he then increased the pressure considerably: in consequence of which he was enabled to see objects distinctly, though placed much nearer than the natural focal distance. For example: he held before his eye, at the natural distance of two inches, a printed book. In the natural state of the eye, he could neither distinguish the letters

nor letters; but, upon making pressure with the speculum, he was enabled to distinguish both the lines and the letters of the book with ease.

Such being this author's conception of the action and effect of the external muscles, he has proceeded to apply the doctrines in explanation of the changes the eye is known to undergo at different periods of life or habits of occupation, from their strength in early life, their debility in old age, and their habitual action from use. These and other general facts are accounted for with considerable address.

Mr. Home, the brother-in-law of Hunter, and vindicator of his posthumous fame, was appointed to read the Croonian lecture on muscular motion, for the session of the Royal Society beginning in the year 1794. His lecture is peculiarly valuable for the experimental results it exhibits. In prosecuting the enquiry projected by Mr. Hunter, he had the great advantage of the assistance of his friend Mr. Ramsden, who, in conversing upon the different uses of the crystalline humour, made the following observations :

He said that, as the crystalline humour consists of a substance of different densities, the central parts being the most compact, and from thence diminishing in density gradually in every direction, approaching the vitreous humour on one side, and the aqueous humour on the other, its refractive power becomes nearly the same with that of the two contiguous substances. That some philosophers have stated the use of the crystalline humour to be, for accommodating the eye to see objects at different distances; but the firmness of the central part, and the very small difference between its refractive power near the circumference, and that of the vitreous or the aqueous humour, seemed to render it unfit for that purpose; its principal use rather appearing to be for correcting the aberration arising from the spherical figure of the cornea, where the principal part of the refraction takes place, producing the same effect that, in an achromatic object-glass, we obtain in a less perfect manner by proportioning the radii of curvature of the different lenses. In the eye the correction seems perfect, which in the object-glasses can only be an approximation; the contrary aberrations of the lenses not having the same ratio; so that, if this aberration be perfectly corrected, at any given distance from the centre, in every other it must be in some degree imperfect.

Pursuing the same comparison: In the achromatic object-glasses we may conceive how much an object must appear fainter from the great quantity of light lost by reflection at the surfaces of the different lenses, there being as many primary reflections as there are surfaces; and it would be fortunate if this reflected light was totally lost. Part of it is again reflected towards the eye by the interior surfaces of the lenses; which, by diluting the image formed in the focus of the object-glass, makes that image appear far less bright than it would otherwise have done, producing that milky appearance so often complained of in viewing lucid objects through this sort of telescope.

In the eye, the same properties that obviate this defect serve also to correct the errors from the spherical figure, by a regular diminution of density, from the centre of the crystalline outward. Every appearance shews the crystalline to consist of laminæ of different densities; and if we examine the junction of different media, having a very small difference of refraction, we shall find that we may have a sensible refraction without reflection. Now, if the difference between the contiguous media in the eye, or the laminæ in the crystalline, be very small, we shall have refraction without having reflection: and this appears to be the state

state of the eye; for although we have two surfaces of the aqueous, two of the crystalline, and two of the vitreous humour, yet we have only one reflected image; and that being from the anterior surface of the cornea, there can be no surface to reflect it back, and dilute an image on the retina.

This hypothesis may be put to the test whenever accident shall furnish us with a subject having the crystalline extracted from one eye, the other remaining perfect in its natural state; at the same time we may ascertain whether or no the crystalline is that part of the organ which serves for viewing objects at different distances distinctly. Seeing no reflection at the surface of the crystalline, might lead some persons to infer that its refractive power is very inconsiderable; but many circumstances shew the contrary: yet what it really is may be readily ascertained by having the focal length and distance of a lens from the operated eye, that enables it to see objects the most distinctly; also the focal length of a lens, and its distance from the perfect eye, that enables it to see objects at the same distance as the imperfect eye: these data will be sufficient whereby to calculate the refractive power of the crystalline with considerable precision.

Again, having the spherical aberration of the different humours of the eye, and having ascertained the refractive power of the crystalline, we have data from whence to determine the proportional increase of its density as it approaches the central part, on a supposition that this property corrects the aberration.

An opportunity presented itself for bringing the observations of Mr. Ramsden respecting the use of the crystalline lens, to the proof. A young man came into St. George's Hospital, with a cataract in the right eye. The crystalline lens was readily extracted, and the union of the wound in the cornea took place unattended by inflammation, so that the eye suffered the smallest degree of injury that can attend so severe an operation. The man himself was in health, 21 years of age, intelligent, and his left eye perfect: the other had been an uncommonly short time in a diseased state, and 27 days after the operation appeared to be free from every other defect but the loss of the crystalline lens.

A number of experiments were made on the imperfect eye, assisted by a lens, and compared with the perfect eye. The aim of these trials, which were judiciously varied, was to ascertain whether the eye which had been deprived of the crystalline lens was capable of adjusting itself to distinct vision at different distances. Among other results, the perfect eye, with a glass of $6\frac{1}{2}$ inches focus, had distinct vision at three inches; the near limit was $1\frac{1}{2}$ inch, the distant limit less than 7 inches. The imperfect eye, with a glass $2\frac{3}{8}$ ths inches focus, with an aperture $\frac{3}{8}$ ths of an inch, had distinct vision at $2\frac{1}{2}$ inches, the near limit $1\frac{1}{2}$ inch, and the distant limit 7 inches. The accuracy with which the eye was brought to the same point, on repeating the experiments, proved it to be uncommonly correct; and as he did not himself see the scale used for admeasurement, there could be no source of fallacy. From the result of this experiment it appears that the range of adjustment of the imperfect eye, when the two eyes were made to see at nearly the same focal distance, exceeded that of the perfect eye. Mr. Ramsden suggested a reason why the point of distinct vision of the imperfect eye might appear to the man himself nearer than it was in reality; namely, that from the imperfection of this organ, he might find it easier to read the letters when they subtended a greater angle than at his real point of distinct vision. The experiments, however, appear to shew that the internal power of the eye, by which it is adjusted to see at
different

different distances, does not reside in the crystalline lens; at least not altogether; and that if any agency in this respect can be proved to reside in the crystalline, the other powers, whatever they may be, are capable of exertion beyond their usual limits, so as to perform its office in this respect.

From these considerations; and in consequence of other reflections tending to shew that an elongation of the optical axis is not probably the means of adjustment, these philosophers directed their enquiries to ascertain how far the curvature of the cornea might be subject to change. They found by trial that this part of the organ possesses a degree of elasticity which is very considerable, both for its perfection and its range; and by anatomical dissection it was found, that the four straight muscles of the eye do in effect terminate in the cornea at their tendinous extremities; that the whole external lamina of the cornea could by gentle force be separated, by means of these muscles, from the eye; so that the tendons seem lost in the cornea, and this last has the appearance of a central tendon. It was also seen that the central part of the cornea is the thickest and the most elastic.

These were considerable advances towards establishing the hypothesis of adjustment by the external curve of the eye. It remained to be shewn, by experiments on the living subject, that this curve does really vary in the due direction, when the mind perceives the distinct visible sensation of objects at different distances. For this purpose Mr. Ramsden provided an apparatus, consisting of a thick board steadily fixed, in which was a square hole large enough to admit a person's face; the forehead and chin resting against the upper and lower bars, and the cheek against either of the sides; so that when the face was protruded, the head was steadily fixed by resting on three sides; and in this position the left eye projected beyond the outer surface of the board. A microscope, properly mounted, so as with ease to be set in every requisite position, was applied to view the cornea with a magnifying power of thirty times. In this situation, the person whose eye was the object of experiment was desired to look at the corner of a chimney, at the distance of 235 yards, through a small hole in a brass plate, fixed for that purpose, and afterwards to look at the edge of the hole itself, which was only six inches distant. After some management and caution, which the delicate nature of these experiments requires, the motion of the cornea, which was immediately perceptible, became very distinct and certain. The circular section of its surface remained in a line with the wire in the field of the microscope, when the eye was adjusted to the distant object, but projected considerably beyond it when adapted to the near one. When the distant object was only 90 feet from the observer, and the near object six inches, the difference in the prominence of the cornea was estimated at $\frac{1}{8000}$ of an inch. These experiments were repeated and varied at different times and on different subjects. The observer at the microscope found no difficulty in determining, from the appearance of the cornea, whether the eye was fixed on the remote or the near object.

From these different experiments Mr. Home considers the following facts to have been ascertained:

1. That the eye has a power of adjusting itself to different distances when deprived of the crystalline lens; and therefore, the fibrous and laminated structure of that lens is not intended to alter its form, but to prevent reflections in the passage of the rays through the surfaces of media of different densities, and to correct spherical aberration.

2. That the cornea is made up of laminae; that it is elastic, and when stretched is capable of

of being elongated 1-11th part of its diameter, contracting to its former length immediately upon being left to itself.

3. That the tendons of the four straight muscles of the eye are continued on to the edge of the cornea, and terminate, or are inserted, in its external laminae: their action will therefore extend to the edge of the cornea.

4. That in changing the focus of the eye from seeing with parallel rays to a near distance, there is a visible alteration produced in the figure of the cornea, rendering it more convex; and when the eye is again adapted to parallel rays, the alteration by which the cornea is brought back to its former state is equally visible.

The remaining part of Mr. Home's lecture contains some observations upon the muscular and elastic powers, which by their opposition produce so curious an effect in the adjustment of the eye. The separate action of the muscles produces a change in the direction of the axis of the eye. A small force of contraction in the whole system will steady the eye, and a greater force will compress the lateral and posterior parts of the eye, and render the cornea more convex. The experiments prove that the eye-ball cannot recede in its orbit by these actions. Other instances in the animal economy, by means of which the expenditure of muscular action is saved by the operation of elastic antagonists, are also stated by this author. The exertion required to adjust the eye to near distances, and the ease with which it was adapted to remote objects, prove that the first was a positive action, and the second a relief. The defect of elasticity inferred to arise from age is happily applied to explain the changes of vision which take place in advanced life.

Two other communications to the Royal Society by this philosopher, and one by Mr. Smith on the eyes of birds, remain to be considered; but, on account of the length of this communication, I shall for the present defer them.

V.

Concerning the Properties of the Sulphureous Acid, and its Combinations with earthy and alkaline Bases. By Citizens FOURCROY and VAUQUELIN.*

SEVERAL philosophers have paid attention to the properties of the sulphureous acid, and some of its combinations; but no one has given a complete account of this acid.

Berthollet is almost the only chemist who has opened this investigation. He is the first who published any accurate account of the subject†.

We shall not in this place describe either the apparatus or method of preparing this acid, because both are well known to such as are moderately acquainted with chemistry.

I. Physical Properties of the Sulphureous Acid.

THIS acid constitutes a permanent elastic fluid at the ordinary pressure and temperature of our atmosphere. Its odour is strong and suffocating. It cannot maintain combustion, nor the respiration of animals.

Bergman, in his Treatise on Elective Attraction, asserts that the sulphureous acid, pre-

* Journal de l'Ecole Polytechnique, cahier IV. p. 445.

† Annales de Chimie, II. 54.

pared in the pneumatic apparatus over mercury, cannot be reduced to the liquid state by any known means. Monge and Clouet affirm, on the contrary, that by the application of extreme cold, and a strong pressure exerted at the same time on this gas, they rendered it liquid.

Its specific gravity, according to Bergman, is 0,00246, and 0,00251, according to Lavoisier; which corresponds with 1.508 grain the inch cube, and 4 oz. 5 gros the foot cube (French weights and measures).

II. Chemical Properties of the Sulphureous Acid.

Action of Caloric.] Priestley and Berthollet affirm, that the sulphureous acid gas, exposed to an elevated temperature, deposits a portion of sulphur, and becomes converted into sulphuric acid. Bergman relates the same fact. This experiment, repeated in several different ways, did not afford us the same result.

III. Action of Oxygene Gas.

ONE part of dry oxygene gas, and two parts of sulphureous acid gas, prepared in the mercurial apparatus, and mixed together, suffered no remarkable change during the course of several months. If a small quantity of water be added to the mixture, a successive absorption, after the diminution which is produced by the combination of a portion of the sulphureous acid, is perceived; which proves the existence of a true combination between these two bodies. In fact, when the mixture is washed, after the expiration of several months, the residue of oxygene is found to be less than the original quantity.

The attraction of the water for the sulphuric acid is therefore favourable to the union of the oxygene with the sulphureous acid. We were not able, however, to convert the whole of the sulphureous acid into sulphuric acid, though in all our experiments oxygene gas remained in the apparatus.

The sulphureous acid is immediately converted into sulphuric acid, by passing a mixture of this gas and of oxygene gas through an ignited tube. A very dense white fume is formed, which becomes condensed into the liquid form, in a bottle placed for that purpose at the other extremity of the tube.

It several times happened, when by accident we had used the exact proportions, that the two gases were entirely destroyed, and not an atom of (elastic) residue was perceived.

IV. The Action of Water.

WHEN the sulphureous acid gas was passed into water cooled by means of ice, the combination was made with such rapidity that not a single bubble rose to the surface of the liquor until it was saturated. The ice is speedily melted; which proves that much caloric is disengaged. The water acquires at this temperature the 0,15th part of its weight. Its specific gravity, compared with that of pure water, is as 1020 to 1000.

Water thus saturated with the sulphureous acid, and exposed to the temperature of 15 degrees above zero (Reaumur), immediately becomes filled with an infinity of small bubbles, which successively increase, and rise to the surface. If a bottle full of this acid be plunged in hot water, it boils with astonishing rapidity, and loses by this operation part of its odour and acidity. It cannot, however, be easily deprived of these qualities, even by boiling.

boiling. This acid freezes at a few degrees below zero, or the freezing point of water; and what may appear astonishing is, that not the smallest portion of gas is disengaged, as happens when the other gases are dissolved in water.

These experiments prove, 1. That the colder the water the more it absorbs of sulphureous acid. 2. That the combination of these bodies is accompanied with heat. 3. That such a combination cannot preserve at a more elevated temperature all the acid it was charged with at a lower degree of heat. 4. That this combination passes to the solid state by cooling, without undergoing decomposition; a fact which shews a great affinity of water for the sulphureous acid, and a weak attraction of this last for caloric.

V. Action of Acids upon the Sulphureous Acid.

Action of the Sulphuric Acid.] We caused sulphureous acid gas to pass through the sulphuric acid cooled by a mixture of ice and salt. in proportion as the acid gas was extricated, it combined with the sulphuric acid without one single bubble arriving at the upper part. Soon afterwards the whole of the acid became congealed; notwithstanding which the combination continued to be made.

From time to time, the tube through which the sulphureous acid passed was obstructed, and required to be withdrawn out of the fluid. By this treatment the matter flowed, and a quantity of gas was disengaged in the form of bubbles. The solid combination had no very perceptible smell while it retained the form of ice; but when a portion was taken out, and laid upon a plate of glass, it exhibited an effervescence like that of marble when an acid is poured on it, and soon afterwards it became liquid and very odorant.

When the temperature of the mixture of salt and ice had risen to the freezing-water point, part of the solid mass became liquid, and bubbles were formed which rose up in the empty part of the vase; but as this was exactly closed, they crystallized on the sides in the form of the leaves of fern.

Action of the Nitric Acid.] The sulphureous acid decomposes the nitric acid, particularly when this last is in a concentrated state. The fluid immediately becomes red, and soon afterwards hot, with a disengagement of nitrous gas frequently mixed with a small quantity of sulphureous acid. The sulphureous acid is changed by this operation into sulphuric acid, for it abundantly precipitates the muriate of barytes. Hence it is proved, that the sulphureous acid has more affinity with oxygen than the nitrous oxide. This truth is confirmed by the conversion of sulphur into sulphuric acid by means of the nitric acid.

Action of the Oxygenated Muriatic Acid.] If the oxygenated muriatic acid gas and the sulphureous acid gas be brought into contact, white fumes are afforded; the two gases become immediately liquid, and lose their odour, if they have been employed in suitable proportions. The same effects take place when these two acids are mixed together in the liquid state, namely, the loss of odour and colour. From the action of these two bodies on each other, sulphuric acid and common muriatic acid are produced. The theory of this operation is nearly the same as that of the preceding experiment.

VI. Action of combustible Bodies on the Sulphureous Acid.

The Action of Hydrogen.] These two substances have no mutual action in the cold; but if they be passed through a tube of glass or porcelain well heated, in the proportion of three parts by measure of hydrogen gas and one part of sulphureous acid gas, the latter is decomposed.

decomposed. Sulphurated hydrogen gas is formed; and at the extremity of the tube opposite to that at which the gases were introduced, very abundant crystals of sulphur are deposited.

Hydrogene therefore, at an elevated temperature, has more affinity with oxygene than sulphur has. Consequently it is not probable, as some persons imagine, that sulphur may be burned by means of water to convert it into sulphuric acid.

The Action of Phosphorus.] Phosphorus does not alter the sulphureous acid. When these two substances are strongly heated in a glass tube, nothing passes which indicates the decomposition of the sulphureous acid. The phosphorus becomes fixed in cooling in the form of transparent drops which do not contain an atom of sulphur, and the sulphureous acid gas is still found to possess all its properties.

In this case, therefore, there is a stronger adhesion between the sulphur and oxygene than between the phosphorus and the same principle. And accordingly this combustible substance, heated with sulphuric acid, does not deprive it of more oxygene than exceeded the constitution of the sulphureous acid.

Action of Phosphorated Hydrogene Gas.] As soon as the sulphureous acid gas and phosphorated hydrogen gas come into contact, a white fume appears, and they lose their fluid state. Plates of a yellow matter are precipitated on the sides of the containing vessel, which take fire on a hot iron, first, in the manner of phosphorus, and afterwards with the characters of sulphur. It follows from this experiment, that the hydrogen is the only substance which burns, or combines with oxygene, in these circumstances, since the gases lose their elasticity, and the phosphorus and sulphur are found combined together.

Action of Sulphurated Hydrogene Gas.] The sulphureous acid gas is decomposed by the sulphurated hydrogen gas. The hydrogen takes the oxygene from the sulphur, and this last principle is separated on both sides. It therefore forms a very abundant deposition. When these gases are dissolved in water, they mutually undergo the same decomposition, and the sulphur then precipitates to the bottom of the liquor*. If suitable quantities of these solutions be taken, the odour of both disappears.

Action of Carbone.] We passed sulphureous acid gas through a tube of glass containing ignited charcoal. At the extremity of the apparatus crystallized sulphur was deposited, and carbonic acid was produced, together with a small quantity of sulphurated hydrogen gas, from a portion of the water which was decomposed. This decomposition does not take place in the cold.

VII. Combinations of the Sulphureous Acid with Alkalis.

THE sulphureous acid readily unites with alkalis and earths. The generic name of sulphites has been given to these combinations. They may be prepared in two ways; either by presenting the aqueous solution of the sulphureous acid to the bases, or by applying the acid in the state of gas to these bodies, either dissolved or mixed with water. The latter method is preferable, for several reasons too long to be detailed here.

The sulphureous acid being very different from the sulphuric acid, it may easily be conceived that its combinations ought to possess properties not at all resembling those of the sulphates. It will in fact be seen, that these salts possess peculiarities of taste, solubility and form, which belong only to themselves; and that they are subject to laws of attraction and decomposition altogether different from those of the sulphates.

* Fourcroy, *Analyse de l'Eau d'Enghien.*

The proportions of principles which constitute the sulphites not having been yet determined, we endeavoured to ascertain a method which might lead us as near as possible to the truth; but it could not be applied to all the kinds of sulphite, on account of the disagreement of their properties; so that we were under the necessity of employing several methods.

The Sulphite of Pot-ash.] Sulphureous acid gas was passed into a saturated solution of very pure carbonate of pot-ash, until the effervescence entirely ceased. During this combination, a small quantity of caloric is disengaged, and the solution crystallizes by cooling.

This salt is usually white and transparent; sometimes it is slightly yellow and semi-transparent, if its solution has been very concentrated, and the crystallization confused. Its taste is penetrating and sulphureous, its figure that of a long rhomboidal plate; its crystallization often presents small needles diverging from a common centre.

When exposed to a sudden heat it decrepitates, and loses its water of crystallization; afterwards by ignition it emits some vapours of sulphureous acid; and at length a portion of sulphur is separated, and the residuum is sulphate of pot-ash with a slight excess of alkali.

By exposure to air it slightly effloresces, becomes opaque and hard, its penetrating sulphureous taste disappears, and it acquires another, which is acrid and bitter. In this state it no longer effervesces with acids.

If this experiment be made in a closed apparatus with oxygen gas, it is found that the volume of the gas is diminished, and that it is even totally absorbed when the quantity is properly regulated. We see therefore that the sulphite of pot-ash may be converted into sulphate by depriving it of a portion of its sulphur by fire, or by introducing a quantity of oxygen at a low temperature.

The sulphite of pot-ash is soluble in a quantity of water nearly equal to its own mass; and this solubility is increased by heat.

This salt is decomposed by lime and by barytes, as may be shewn by pouring lime water into a solution of the sulphite of pot-ash. A white precipitate is afforded, which is the sulphite of lime, and the pot-ash remains disengaged in the water.

The sulphureous acid does not therefore follow the same laws of affinity as the sulphuric acid, since this last adheres more strongly to pot-ash than to lime.

The alkalis do not change the nature of the sulphite of pot-ash.

Among the acids, some decompose the sulphite of pot-ash by separating the sulphureous acid; others change its nature without driving off its acid, but by affording a portion of oxygen, and converting it into sulphuric acid. The first of these effects is produced by the sulphuric, muriatic, phosphoric and fluoric acids; the second is effected by the nitric and the oxygenated muriatic acids. The acids of borax and of carbo do not occasion any change in the cold.

When the sulphuric acid is poured on the sulphite of pot-ash, a rapid effervescence takes place with a crackling noise, at the same time that much caloric is disengaged.

The nitric acid, on the contrary, emits red vapours, mixed only with a small quantity of sulphureous acid, and the residuum is composed of a mixture of the sulphate and nitrate of pot-ash.

The oxygenated muriatic acid mixed with the sulphite of pot-ash immediately loses its smell, and the fluid is deprived of its sulphureous taste.

If the solution of the sulphite of pot-ash, and also that of the oxygenated muriatic acid, be both concentrated, crystals of the sulphate of pot-ash are formed immediately on the mixture.

Charcoal converts the sulphite of pot-ash into the sulphuret of pot-ash.

Salts with bases of pot-ash do not decompose this salt.

Those with bases of soda, except the borate and the carbonate, are decomposed by the sulphite of pot-ash.

All the other salts, of which the acids are stronger than the sulphurous acid, are equally decomposed by the sulphite of pot-ash.

The super-oxygenated nitrate and muriate of pot-ash, when heated with the dry sulphite of pot-ash, take fire, and are changed into sulphate.

Several metallic oxides act upon this salt. Some are entirely reduced to the metallic state; such as the oxides of gold, silver and mercury: others are brought nearer that state; such as those of lead, iron and manganese, at the maximum of oxygenation. There are others which change the nature of the sulphite of pot-ash in an opposite direction to those which take place in the foregoing cases; that is to say, which convert it into sulphate by depriving it of a certain quantity of sulphur, with which they form sulphurated oxides, such as the oxides of arsenic, and of iron slightly oxidized; but in order that this operation may succeed, it is necessary to boil these substances a long time in water, and afterwards to add to the solution an acid, which occasions a coloured precipitate, at the same time that the smell of sulphurated hydrogen gas is emitted.

All the metallic solutions except the carbonates are decomposed by the sulphite of pot-ash; and as most of the metallic sulphites are insoluble, different coloured precipitates are formed, according to the nature of the metal and its state of oxidation.

[*To be continued.*]

VI.

A Memoir on the Nature of the Alum of Commerce, on the Existence of Pot-ash in this Salt, and on various simple or triple Combinations of Alumine with the Sulphuric Acid. Read before the National Institute of France. By Citizen VAUQUELIN.*

IN my memoir on the leucite or white grenate, I have announced that many natural earths and stones contain pot-ash in a state of combination; and I founded my opinion at that time on the impossibility of obtaining solid crystallized alum in octahedrons by the immediate combination of sulphuric acid and pure alumine, whatever precautions were taken to clear it of the excess of acid, without the addition of alkali.

The necessity of this addition has long been known in the alum works, more particularly in the treatment of the mother waters; and it was thought that the use of pot-ash in this circumstance was merely to saturate the excess of acid, which was supposed to prevent the crystallization of the alum. Nevertheless, the remark made by Bergman, that soda and

* This important Memoir is inserted in the *Annales de Chimie*, XXII. 258, whence I have translated it.

lime, employed instead of pot-ash or ammoniac in the treatment of the mother waters, do not favour the crystallization of the alum, ought to have produced a change in the general opinion respecting the action of pot-ash or ammoniac *. This learned chemist had likewise ascertained, that several alums decomposed by ammoniac afforded by evaporation the true sulphate of pot-ash, the basis of which might be afforded, according to him, either from the argillaceous earth in which vegetables had been decomposed, or from the wood-ashes intentionally added, or, lastly, from a casual mixture during the calcination of the ores. And he concluded from these observations, that the sulphates of pot-ash and of alumine unite together in the state of a triple salt †.

Though Bergman appears to suspect that pot-ash is necessary to the formation of alum, yet he does not venture to affirm it; as may easily be seen in the course of his dissertation: so that this question remained still undecided. In fact, we see by another passage ‡, that Bergman falls into the common opinion, by confining the effect of the alkalis to the simple saturation of the excess of acid, existing, according to him and all other authors, in the aluminous waters; and even by considering the new salts formed by these alkaline substances as foreign bodies, less noxious in fact than the excess of acid, but which cannot however be sold for alum §.

If the only effect of alkalis in the management of aluminous lixivia were to saturate the excess of acid they are supposed to contain, it is evident that any other substance capable of absorbing this acid would answer the same purpose. But experience has proved the contrary, and it has been long known that this remarkable property belongs exclusively to pot-ash and ammoniac.

In order to explain this obscure circumstance by experiment, I dissolved very pure alumine in sulphuric acid of equal purity. I evaporated the solution several times successively, even to dryness, for the purpose of expelling the superabundant acid. I redissolved the dry and pulverulent residuum in water, and reduced the solution to different degrees of specific gravity, with a view to seize the point most favourable to crystallization: but whatever precautions I took, I could not obtain any thing but a magma formed of saline plates without consistence

* Notatu dignum est, quod hoc crystallisationis obstaculum alcali volatili æquè tollatur, non vero alcali minerali et calce. Bergman de Confect. Aluminis, pag. 325, tom. i. Opuscula.

† Hoc alcali quod inest, vel ex ipsâ argillâ repetendum quæ vegetabilium putrefactorum residuis fuit inquirata, vel ex cineribus studio additis, vel denique sub calcinatione et unctione fortuito immixtis. Interea hinc constat, quod alumen et alcali vegetabile vitriolatum facillè connubium ineant, quò sal oritur triplex. Berg. ibid.

‡ Allata momenta suspicionem movent, quod alcali vegetabile alumine perficiendo sit necessarium, ideoque omne alumen perfectum instar salis triplicis respiciendum: sed hæc conjectura vacillat; nam eadem perfectio alcali volatili et spontaneâ evaporatione obtinetur. Non tamen improbandam puto additionem alcali vegetabilis et depurati, nam heterogenea magis nocent quam juvant. Berg. ibidem.

§ Ut eo purius obtineatur alumen in alterâ crystallizatione nonnullis in locis additamenta usurpantur alcalina, calx et urina. Scilicet multorum annorum experientiâ compertum est, lixivium aliquando tantam et acquirere consistentiam (quod in officinis pinguescere dicitur) ut et crystalli ægrè secerantur, et quæ prodeunt variis heterogeneis irritate repellantur. His incommodis alcalinis præsertim obicem ponere tentatum fuit, quàm lixivium aciditate abundaret. Cineris clavetati et calx, sive usta sive cruda, acidum absorbent, et si iustâ addentur dosi, peregrina noxia re-vera præcipitando minuunt, quod, cognita lixiviorum indole, S. IX. luculentius patebit. Urina tamen nihil efficit, nisi quatenus alcali volatili præstat. Negari tamen non potest quin novi sales peregrini immisceantur, nimirum alcali vegetabile vitriolatum, vel alii pro diverso additamento, sine dubio, sublati magis inuocui, sed nihilominus pro alumine vendendi. Berg. ibid. p. 310.

or solidity. The solution here described, which had constantly refused to afford crystallized alum alone, afforded it immediately by the addition of a few drops of the solution of pot-ash; and as I had employed these two substances in the requisite proportion, the rest of the solution afforded to the very end pure alum without any mixture of sulphate of pot-ash.

Into another portion of the same solution of pure alumine I dropped the same quantity of carbonate of soda as I had added of that of pot-ash to the former. No crystallization was formed, even by the help of evaporation.

Lime and barytes produced no better effect.

These experiments began to confirm the opinion I had, that the crystallization of alum is not prevented by an excess of acid, as has hitherto been thought, and that pot-ash was not of use simply to saturate this acid, but that it performed an office of more importance. For I reasoned, that if the common opinion were true, soda, lime, barytes, and all the substances which by a more powerful force would take this acid from alum, ought to give the same result. Another argument likewise presented itself, which seemed decisive. If the alkalis, pot-ash and ammoniac do nothing more than unite to the superabundant acid of the alum, the sulphates of pot-ash and of ammoniac ought not to occasion any change in pure alum in its acidulated state; but if these alkalis enter as a constituent part into the alum, and are necessary to its existence, they ought to produce the same effects as pure pot-ash or ammoniac.

I therefore added to a third portion of the solution of sulphate of alumine before mentioned, some drops of the solution of sulphate of pot-ash; immediately upon which octahedral alum was formed. The sulphate of ammoniac presented the same effect.

This result gave still greater confirmation to my first notions, though it did not yet afford a demonstration perfectly without objection. For it might have happened that the two salts I made use of might determine the crystallization of the alum, simply by absorbing the superfluous acid, of which they are very greedy*.

To determine this possible fact, I mixed in the uncrystallizable solution of alumine some of the acid sulphate of pot-ash, and obtained a crystallization no less abundant than with the neutral sulphate of pot-ash.

This last experiment does not therefore leave any doubt with regard to the influence and mode of action of pot-ash and ammoniac in the fabrication of alum. This action is still more strongly confirmed, by the examination of the alums which have been formed by the processes above related. For in this manner it is proved that they contain notable quantities of the sulphates of pot-ash and ammoniac.

These experiments naturally led me to an examination of the different alums of commerce. Bergman had already announced, though in an indistinct manner, that not only the common alum, but likewise that of Rome, when decomposed by ammoniac, afforded traces of the sulphate of pot-ash; and Scheele had remarked on his side, that alum which does not contain pot-ash is not fit for making pyrophorus. Bergman, quoting this fact from Scheele, shews likewise that he considered the sulphate of pot-ash in alum as a foreign substance†.

* Ceterum alumen non tantum vulgare, sed etiam romanum alcali volatili præcipitatum liquorem exhibet, qui haud raro alcali vegetabile vitriolatum continet. *ibid.*

† Alumen hoc inquinamento spoliatum pyrophoro generando ineptum est: quod facile experiri licet, nam magma aluminosum distinctam respuens crystallizationem, nullum præbet pyrophorum, modo consueto tractatum, quamvis idem addito alcali vegetabilis pauxille, eximium porrigat, &c.

In fact, if this chemist had thought pot-ash essential to the constitution of alum, he certainly would not have thought it advisable to use a portion of the clay of Cologne to destroy the excess of acid in aluminous waters. For this addition is extremely vicious, and I am convinced that it was suggested by reasoning rather than by experiments*.

Out of all the kinds of alum which I had submitted to analysis, I did not find one which did not afford sulphate of pot-ash or of ammoniac, and frequently both at once. I used the following method of analysis: In the first place, to direct my operations, I take a small quantity of the alum I wish to assay, which I dissolve in the aqueous solution of pure pot-ash, and slightly heat the mixture. If it contain sulphate of ammoniac, a strong smell of volatile alkali is immediately perceived. I then put into a tubulated retort a given quantity of this sulphate of alumine; I adapt a receiver containing a small quantity of water, and then pour on the alum a solution of pot-ash in a proper quantity to decompose the sulphate of ammoniac and alumine at once. I boil this mixture for a quarter of an hour, at the end of which all the ammoniac is volatilized, provided no more than three or four decimastic quintals be operated upon. I combine this ammoniac to saturation with the sulphuric acid, and the quantity of salt which I obtain indicates that which was contained in the ammoniacal sulphate of alumine.

When the pot-ash does not indicate the presence of ammoniac, which is very seldom, I follow another method to separate the sulphate of pot-ash. I decompose the alum by means of ammoniac, and, after having washed the precipitated alumine, I evaporate the liquor to dryness, and heat the remaining salt in a crucible, till it emits no more white vapours of the sulphate of ammoniac. The remainder is the pure sulphate of pot-ash. By these analytical methods, I found that one pound of crystallized alum contains about one ounce 64 grains of sulphate of pot-ash; but as the alum contains about 0.44 water of crystallization, this raises the quantity of the sulphate to one ounce 7 gros 17 grains, for a pound of alum, or otherwise for crystallized alum, about 0.070, and for dry alum, 0.125. When the alum has been formed with volatile alkali, it is found to contain the sulphate of ammoniac, nearly in the same proportion as the sulphate of pot-ash. Whence it follows, that a quintal of alum prepared with pot-ash contains,

1. Sulphate of alumine	—	—	—	49
2. Sulphate of pot-ash	—	—	—	7
3. Water	—	—	—	44
				<hr/>
				100

When the alums contain both the salts here mentioned at once, which frequently enough happens, I use lime instead of pot-ash to disengage the ammoniac, and proceed with the residue as before.

It may therefore be ascertained, by these simple essays, whether pot-ash or ammoniac or both together have been used in a manufactory for the preparation of alum. This proof

* Hujus (lixivii magistralis) cantiaro duas addidi drachmas argillæ Colonienfis in subtilem comminatur pulverem, et pauci aquæ humiditæ; calore ebullitionem provocavi, qua per decem minutæ continuata, et postea, refrigeratione peractâ, residuum separavi argillam; totam siccavi; tandemque ponderatione inveni 25.5 grana soluta, quæ aluminis augmentum 141 granorum indicant. Berg. ibidem

may be of some utility; for Bergman pretends, I don't know on what foundation, that urine communicates to alum properties which are hurtful in dyeing: it is not probable, however, that this chemist asserted the fact without proof. In all the works where putrid urine is employed for the treatment of alum waters, the alum contains the sulphates of pot-ash and of ammoniac, because the combustibles which serve to roast the ores deposit a certain quantity of alkali, which unites with the sulphuric acid, and contributes, according to its proportion, to the formation of a greater or less quantity*.

It is known, from the experiments of Bergman and several other chemists, that by boiling a solution of ordinary alum with pure alumine in a very divided state, this last combines with the alum, and renders it insoluble in water; that is to say, it converts it into the state of neutral sulphate of alumine, or saturated with its earth. I have repeated this experiment with the design to ascertain whether the sulphates of pot-ash and ammoniac are precipitated with the alum; and I immediately observed, that the combination does not take place but by means of heat, though I used alumine recently precipitated from a solution, and still humid; and that at the end of a certain time the alum was entirely precipitated, and scarcely any signs of it left in the water. I re-dissolved the alum thus precipitated, in diluted sulphuric acid, and this solution afforded very fine crystals of octahedral alum by cooling; whence it follows that the pot-ash and ammoniac did fall down with the sulphate of alumine, and joined in the formation of this quadruple, earthy, tasteless salt. I at first imagined that this fact might serve to explain how it sometimes happens that aluminous waters, passed over materials less rich than those from which they were obtained, diminished in density by the loss of a portion of the acid necessary to the solution of the alum; but as the combination is not made without heat, and it requires a great division in the alumine, I presume that this effect is owing to another cause: nevertheless, it might be possible, in the process of time, especially in hot weather, that something of this nature might happen. It was interesting to determine what happens with regard to the sulphates of pot-ash and ammoniac in the precipitation of alum by its own base. I therefore boiled a solution of pure sulphate of alumine, that is to say, which contained no alkali, and was not crystallizable, with a certain quantity of the earth in a very divided state. It dissolved a small quantity, lost its slight acidity, but did not become insoluble. Having afterwards dropped a very small quantity of the solution of pot-ash into this liquor, a precipitate was formed in a short time, which was of the same nature as that of the foregoing experiment; that is to say, what is called alum saturated with its earth. It is therefore proved that the sulphates of pot-ash or ammoniac are necessary to render the alum capable of being precipitated by its earth, or to cause it to pass, as it were, to the earthy state. It is also proved, that the aluminous waters which do not contain pot-ash may remain as long as may be desired on their materials, without becoming saturated with too great a quantity of earth, or suffering alum to precipitate.

From the whole of what we have thus far explained, it will be easy to draw a number of consequences of importance to the arts, chemistry, and natural history.

1. It is not, at least in the greatest number of circumstances, the excess of acid which im-

* When alums contain at the same time the sulphate of ammoniac and of pot-ash, the quantity of the latter is less; and in this respect there must be great varieties in the proportions, according to the dose of urine or of pot-ash which is added. V.

pedes the crystallization of alum, but it is the want of pot-ash or ammoniac. For it is difficult indeed to imagine that the sulphuric acid could remain disengaged after so long remaining upon alumine in a state of extreme division, and always superabundant. It is true that the aluminous waters reddens the vegetable tinctures, but this property is not owing to a disengaged acid. This portion of acid is a constituent part of these waters, and it appears to have more affinity with the neutral sulphate of alumine than with a new quantity of this earth at the temperature of the atmosphere.

2. The sulphate of pot-ash may be used, as well as pure pot-ash, to cause the crystallization of alum. It even has the advantage over the latter salt, because, if the aluminous waters do not really contain a disengaged acid, the pot-ash, in its combination, will precipitate a portion of alumine, and diminish the product of the boiling; whereas the sulphate of pot-ash does not produce the same effect: but if the liviviums contain disengaged acid, which must very seldom be the case, it is not converted into alum by the sulphate of pot-ash, and is lost with regard to the product. I think, therefore, that with regard to such waters as really contain an excess of acid, or a very oxidized sulphate of iron, the use of pot-ash may be preferable to that of the sulphate of pot-ash. But with regard to the price of these substances, I think that in many places it would be profitable to use the sulphate of pot-ash, because it is a salt indirectly produced in a great number of manufactories, where it may of course be obtained at a very moderate price. The residues of the distillation of aqua fortis by the sulphuric acid would be excellent for this operation. I mean that they would be preferable to the neutral sulphate of pot-ash. For I have remarked that this last precipitates a portion of aluminous earth, which the other does not. This salt would more especially possess a decided preference before pot-ash, in those cases where the aluminous waters contain at the same time a great quantity of sulphate of iron intended to be usefully employed, because it would act immediately on the sulphate of alumine, without touching that of the iron; whereas the pot-ash does not begin to form alum until the whole of the ferruginous salt is decomposed. It would be in particular of much greater advantage than putrid urine, because this fluid always contains phosphoric salts, which decompose a portion of the sulphate of alumine, and considerably diminish the product.

This inconvenience might, however, be avoided by adding a certain quantity of lime to the urine, to precipitate the phosphoric acid.

3. Alumine cannot be used in the treatment of mother waters, as Bergman proposes. This earth is incapable of favouring the crystallization of alum, besides which, it decomposes a portion of alum by the assistance of ebullition; in which circumstance it seizes the acid necessary to its solution, and precipitates it in the form of that powder which is called alum saturated with its earth.

4. Many alum ores must naturally contain pot-ash, because perfect alum is often obtained from the first crystallization of new alum waters without the addition of this alkali. It is true that an objection may be made with regard to the wood used in calcining these ores, which may be supposed to have furnished the alkali; but it is not probable that the small quantity of wood employed, in comparison to the quantity of ore and the alum it affords, could supply enough of pot-ash for the crystallization.

5. All the earths and stones which have given, or shall hereafter afford, by analysis with the sulphuric acid, perfect alum without addition of pot-ash, must contain this alkali nat-

turally. For it is well proved, by the experiments related above, that alum cannot exist without pot-ash or ammoniac; and as there is little probability that this last should be found combined in earths or stones, unless perhaps in very rare cases, we may almost constantly be assured, when alum is obtained from any of these substances, that its formation was effected by pot-ash. The quantity of alum will immediately shew in what proportion this alkali existed in the substances analysed. I have announced to the Institute in the communication of my experiments on the leucite, that I had begun a series of experiments on several earths and stones which I presumed to contain pot-ash. At present I can give the results of some of my experiments.

The crude alum ore of La Tolfa afforded me 2,3 per cent.; but as it is difficult to extract more than two-thirds of this substance from the stones which contain it, we may, without fear of mistake, estimate the quantity at 3,4 per quintal*. The zeolite of the Isle of Ferro afforded 1,78, which makes according to our estimation 2,37 per cent. The argillaceous earth of Forges in Normandy, of which the pots of the glass works at Seves are made, likewise afforded it, but in a very small quantity†. The adamantine spar, on the analysis of which Citizen Guyton is at present employed, must contain a considerable quantity; for it affords much alum when treated immediately with the pure sulphuric acid. In reading over the analyses of stones which have hitherto been made, an almost certain proof will be found, no doubt, either by the loss in the sum of their products, or by the alum they afforded with the sulphuric acid, that they contained pot-ash, though no mention either was made or could be made respecting it.

6. The alum of commerce ought not to be considered as a simple salt, but as a combination in the state of a triple and sometimes quadruple salt of sulphate of alumine, sulphate of pot-ash, or of ammoniac. Among these last we may distinguish two species; the one without excess of acid, insoluble in water and insipid, being what is improperly called alum saturated with its own earth; and the other, which contains an excess of acid soluble in water, very sapid and astringent, is the common alum.

There is likewise a pure sulphate of alumine, very astringent, very difficult of crystallization, in the form of brilliant pearl-coloured plates without consistence, and which cannot be rendered insoluble by the addition of a new quantity of its base. This last salt may with the greatest propriety be called the sulphate of alumine.

7. It follows from the comparative analysis, and the knowledge acquired respecting the

* 100 parts of the ore of Tolfa contain

1. Alumine	-	-	-	43,92
2. Sulphuric acid	-	-	-	25,00
3. Pot-ash	-	-	-	3,08
4. Water	-	-	-	4,00
5. Silix	-	-	-	24,00

100,00

† 100 parts of the earth of Forges, calcined, contain:

Alumine	-	-	-	40
Silix	-	-	-	60
				100

different

different states of the combination of alumine with the sulphuric acid united at the same time with other bases, that we must distinguish seven states in this combination, and that it is necessary to express them according to the rules of the methodical nomenclature. Here follow the series, the nature, and the names of these seven sulphates of alumine.

1. Sulphate of alumine, or the artificial combination of sulphuric acid and alumine. This salt is astringent; it crystallizes in laminæ or flexible leaves, soluble in water. It has never been described nor named by chemists.

2. Acid sulphate of alumine. It is the foregoing salt, with excess of acid, from which it differs by reddening blue vegetable colours. It is easily made by dissolving that salt in the sulphuric acid, but it is not easy to convert this into the neutral sulphate of alumine but by boiling it a long time with its earth. This salt, like the first, has not been described.

3. Saturated sulphate of alumine and of pot-ash. It is the alum of the chemists saturated with its earth. I have described the manner of making it. It is pulverulent, insipid, infusible, not crystallizable, and is easily converted into true alum by the addition of sulphuric acid.

4. The acid sulphate of alumine and of pot-ash. It is easy to prepare it chemically. It greatly resembles common alum; but I have found none but that of La Tolfa which is of the same nature.

5. The acid sulphate of alumine and of ammoniac. It is easily made in our laboratories. I have not yet found it pure in commerce. It has all the properties of alum, and may be used for the same purposes.

6. The acid sulphate of alumine, pot-ash, and ammoniac. It is remarkable enough, that this should be the nature of the alum most frequently made in the arts, and that to express its combination so many words should be necessary. This, however, may be avoided by reserving the name of alum to this substance, which will be sufficient to distinguish it perfectly.

7. The acidulous sulphate of alumine and of pot-ash. I am less acquainted with this than the preceding species. The name which I propose to characterize it has been suggested to me, because by adding a small quantity of pot-ash to the solution more than is necessary to obtain octahedral crystals, it manifestly passes to the cubic form.

8. Lastly, the physician, the chemist, and the manufacturer, with whom the uses of alum are greatly multiplied, will hereafter possess a knowledge of the substance they employ, and may appreciate its effects on the animal economy, and other bodies to which it is so frequently applied.

VII.

*Description of an Instrument proper to measure the Volume of a Body without plunging it in any Liquid. By H. SAR, Capitaine du Génie *.*

THE ordinary method of determining the specific gravity of solids by immersion in a liquid is inconvenient for powders and fluids, and impracticable with regard to such bodies

* Abstract of a Memoir printed in the *Annales de Chimie*, XXIII. 1.

as may be acted upon by the standard liquid. The learned author of the memoir from which the following extract is taken, has proposed to remove this difficulty by plunging the subject of experiments in common air or a gaseous fluid, by means of an instrument he calls a stereometer.

If the capacity of a vessel, or, which is the same thing, the volume of air contained in that vessel, be measured, when the vessel contains air only, and also when the vessel contains a body whose volume is required to be known, the volume of air ascertained by the first measurement, deducting the volume ascertained by the second, will be the volume of the body itself. Again, if it be admitted as a law, that the volume of any mass of air be inversely as the pressure to which it is subjected, the temperature being supposed constant, it will be easy to deduce, from the mathematical relations of quantity, the whole bulk, provided the difference between the two bulks under two known pressures be obtained by experiment.

Let it be supposed, for example, that the first pressure is double the second, or, which follows as a consequence, that the second volume of the air be double the first, and that the difference be fifty cubic inches, it is evident that the first volume of the air will likewise be fifty cubic inches. The stereometer is intended to ascertain this difference at two known pressures.

The instrument is a kind of funnel, AB, Fig. 1, Plate XIV. composed of a capsule A in which the body is placed, and a tube B as uniform in the bore as can be procured. The upper edge of the capsule is ground with emery, in order that it may be hermetically closed with a glass cover M slightly greased. A double scale is pasted on the tube, having two sets of graduations; one to indicate the length, and the other the capacities, as determined by experiment.

When this instrument is used, it must be plunged in a vessel of mercury with the tube very upright, until the mercury rises within and without to a point C of the scale, see Fig. 2.

The capsule is then closed with the cover, which being greased will prevent all communication between the external air and that contained within the capsule and tube.

In this situation of the instrument, in which the mercury stands at the same height within and without the tube, the internal air is compressed by the weight of the atmosphere, which is known and expressed by the length of the mercury in the tube of the common barometer.

The instrument is then to be elevated, taking care to keep the tube constantly in the vertical position. It is represented in this situation Fig. 2, second position. The mercury descends in the tube, but not to the level of the external surface, and a column DE of mercury remains suspended in the tube, the height of which is known by the scale. The interior air is therefore less compressed than before, the increase of its volume being equal to the whole capacity of the tube from C to D, which is indicated by the second scale.

It is known therefore that the pressures are in proportion to the barometrical column and to the same column diminished by the subtraction of DE. And the bulks of the air in these two states are inversely in the same proportion, and again the difference between these bulks is the absolute quantity left void in the tube by the fall of the mercury; from which data by an easy analytical process the following rule is deduced: Multiply the number which expresses the less pressure by that which denotes the augmentation of capacity, and divide the product by the number which denotes the difference of the pressures. The quotient will be the bulk of the air when subject to the greater pressure.

To render this more easy by an example, suppose the height of the mercury in the barometer to be 78 centimetres, and the instrument being empty to be plunged in the mercury to the point C. It is then covered, and raised until the small column of mercury DE is suspended, for example, at the height of six centimetres. The internal air, which was at first compressed by a force represented by 78 centimetres, is now compressed only by a force represented by $78 - 6$, or 72 centimetres.

Suppose it to be observed at the same time, by means of the graduations of the second scale, that the capacity of the part CD of the tube which the mercury has quitted is 2 cubic centimetres. Then by the rule 72×2 give 24 cubical centimetres, which is the volume of the air included in the instrument when the mercury rose as high as C in the tube.

The body of which the volume is to be ascertained, must then be placed in the capsule, and the operation repeated. Suppose, in this case, the column of mercury suspended to be eight centimetres, when the capacity of the part CD of the tube is equal to 2 centimetres cube. Then the greatest pressure being denoted by 78 centimetres, as before, the least will be 70 centimetres, the difference of the pressures being 8, and the difference of the volumes two cubical centimetres. Hence $\frac{8}{2} \times 2$ gives the bulk of the included air under the greatest pressure 17,5 cubic centimetres. If therefore 17,5 centimetres be taken from 24 centimetres, or the capacity of the instrument when empty, the difference 6,5 cubic centimetres will express the volume of the body which was introduced. And if the absolute weight of the body be multiplied by its bulk in centimetres, and divided by the absolute weight of one cubic centimetre of distilled water, the quotient will express the specific gravity of the body in the common form of the tables where distilled water is taken as unity, or the term of comparison.

After this description and explanation of the use of his instrument, the author proceeds with the candour and acuteness of a philosopher to ascertain the limits of error in the results; an object seldom sufficiently attended to in the investigation of natural phenomena. From his results it appears, that with the dimensions he has assumed, and the method prescribed for operating, the errors may affect the second figure. He likewise gives the formulæ by means of which the instrument itself may be made to supply the want of a barometer in ascertaining the greatest pressure. He likewise adverts to the errors which may be produced by change of temperature. To prevent these as much as possible, the actual form of the instrument and arrangements of its auxiliary parts are settled as in Fig. 3, by which means the approach of the hand near the vessel and its tube is avoided. In this figure the vertical position of the tube is secured by the suspension of the vessel, and a perforation in the table through which the tube passes. The table itself supports the capsule in its first position, namely, that at which the cover is required to be put on.

It seems probable, from the author's general reflections immediately preceding the explanation of the figures, that he had not entirely finished his meditations on this subject. If he had, I think it likely that he would have determined his pressures as well as the measures of bulks by weight. For it may be easily understood, that if the whole instrument were set to its positions by suspending it to one arm of a balance at H Fig. 3, the quantity of counterpoise when in equilibrio might be applied to determine the pressures to a degree of accuracy much greater than can be obtained by linear measurement.

VIII.

Useful Notices respecting various Objects.—Styrian Steel—Elastic Strings for Musical Instruments and other Purposes—Wheels without Cogs.

1. *Styrian Steel.*

SOME time ago I was favoured with a visit from Mr. Ramondini, an Italian gentleman on his travels. He assured me that the Styrian steel is made very well in Hungary, and can be obtained from any iron originally smelted with charcoal, and not with coke. He says that the secret of the process consists in the management upon the hearth of the blast or refining furnace. If the iron be stirred about while in fusion, it will afford bar iron; but if the scoria be simply raked off as it rises, and the metal be left for a due time undisturbed in fusion, and then taken to the hammer, it proves to be steel. He affirms, that the iron made with coke will not cement into steel, but forms a slag. Perhaps this may arise from a redundancy of plumbago.

2. *Elastic Strings for Musical Instruments and other Purposes.*

THE elastic string known by the name of catgut, and very useful for making the bands of lathes, when its diameter is large, or the strings of musical instruments, when small, possesses peculiar properties which are well known. As it is composed of animal fibre, it is subject to putrefy by accidental moisture, and on this account the strength of such as appears to be perfect is very uncertain. I understand that musical strings of catgut are not made in this country. An intelligent friend of mine made an experiment many years ago with silk to form this last article. He wound the single thread or fibre of the silkworm, without twisting, till the mass of fibres formed a body equal to his wish. He smeared this mass with white of egg, which he rendered immediately consistent by passing it through heated oil. The string was exquisitely true, or uniform in its thickness. When tried upon an instrument, the tone was not satisfactory to an accustomed ear: the performer said it was tubby, a word which I suppose has reference to the sound emitted by a tub, and will, as I imagine, convey a correct though perhaps undefinable notion to a musician. Musical strings produce a sound more grave the greater their diameter or their length, and the less their tension. It is required that this sound should not only be intense and perfect, but also that it should endure for a certain short time without considerable diminution. The maximum of these qualities, when the length is limited, is aimed at by regulating the diameter and the tension. If the diameter be too great, the sound will not last; if too little, it will be deficient in intensity. There is an artifice by which the permanence of a grave sound from a given length of string is secured, without requiring the tension to be much diminished. It consists in adding to the weight of the string, without adding to its strength or elastic force. For this purpose a slender wire is wrapped round the string, which with this load gives a very different and more lasting tone than a simple string of the same length. I believe it is admitted that the tone of these strings is less brilliant or clear, and I suppose the strings made by my friend had the same quality, the brittle indurated white of egg adding to the weight but not to the elasticity. The caoutchouc or elastic gum was at that time unknown. I think it worth trying with silk for musical strings, and still more in the fabrication of strong bands for lathe work, to be used instead of catgut.

3. *Wheels without Cogs.*

VERY much of the disposal or transmission of mechanic force is effected by means of wheels acting upon each other. The connection of the moving parts is performed either by the mutual action of teeth, or by straps, or by the friction of one face of a wheel against another. I have seen a drawing of a spinning-wheel for children, at a charity-school, in which a large horizontal wheel with a slip of buff leather glued on its upper surface, near the outer edge, drove twelve spindles at which the same number of children sat. The spindles had each a small roller likewise faced with leather, and were capable by an easy and instantaneous motion of being thrown into contact with the large wheel at pleasure. Each child could therefore throw her own part of the apparatus into work, or cause it to stop, as often or as long as she pleased. The winding bobbins for yarn at the cotton-mills operate on the same simple and elegant principle, which possesses the advantage of drawing the thread with an equal velocity, whatever may be the quantity on the bobbin, and cannot break it. I do not know that this principle has been applied in large work, except in a saw-mill by Mr. Taylor of Southampton. In this the wheels act on each other by the contact of the end grain of wood instead of cogs. It makes very little noise, and at the time it was mentioned to me had been in wear about fourteen years. I suppose, of course, that there is a contrivance for bearing the wheels firm against each other by wedges at the sockets, or rather by levers.

IX.

An Economical Process to obtain Pure Caustic Alkali in the Large Way, with Fused Pot-ash, or the Lapis Causticus. By Citizen BOUILLON LE GRANGE*.

THE processes for obtaining the caustic alkali and fused pot-ash, called lapis causticus in the dispensatories, being either defective or tedious, the Citizen Welter and myself have endeavoured to abridge this operation, by using a process which requires less time and expence, and is more certain and useful, particularly for obtaining the caustic alkali in the large way, which is so necessary in the arts and in chemical experiments.

We believe, therefore, that practical men will receive with pleasure the description of a method by which no part of the pot-ash is lost, and which affords it in a state of purity, and very caustic, without much expence or apparatus.

Our apparatus consists of several vessels of white wood, or, which is better, calcareous stone, whose dimensions may be varied according to the quantities intended to be prepared. Those which we have established for the Polytechnic School are of stone, of the internal capacity of one foot cube, (see Fig. 4, Plate XIV.) having their bottoms grooved by channels of an inch in depth and the same width, and so far distant from each other that there are five or six parallel, which terminate at an end in a similar groove, which crosses the whole, and serves as a gutter to collect the saline solution. In the middle of this last an

* Annales de Chimie, XXII. 137.

hole is pierced, to insert a tube of glass, which comes out under an angle of 45 degrees with the horizon.

The channels are covered with tubes of glass ranged across, upon which is placed a cloth in such a manner as to cover them completely. Upon this a thin layer of wood-ashes is sprinkled, and afterwards the mixture which we shall proceed to mention is put into the vessels.

For want of vessels of stone, we may operate by using small tubs of white wood, as in Figure 5; and instead of the grooves, river sand must be spread on the bottom, after being first carefully washed; over this a finer stratum must be spread, and the whole then covered with a cloth, and wood-ashes strewed thereon. These vessels, like the others, must each have a tube to convey the fluid which filters through.

It is not necessary to remark, that the first method is to be preferred. For the caustic alkali always seizes a portion of the colouring matter of the wood, and may even, according to its degree of concentration, take up a small quantity of silex by solution. But these inconveniences are of little consequence, when merely the caustic alkali or lapis causticus is wanted.

For the arts, and for the delicate experiments of chemistry, the cisterns of calcareous stone are to be preferred: with these the liquor is obtained perfectly limpid.

Things being thus disposed, equal parts are to be taken of quicklime and of pot-ash, more especially when the lime is very caustic. In the contrary case, twenty parts of lime may be taken to fifteen of pot-ash. Water is then to be heated in an iron pot till it nearly boils; in this state the lime is to be added, which by its extinction brings it to the boiling heat. When the extinction is complete, the pot-ash is mixed, and the whole formed into a thick mass, which is left to cool a little.

The mixture is afterwards poured into the cisterns, and immediately covered with water. In order to avoid making perforations in the mixture by pouring the water, a small board is laid to receive the stream, which rises with the fluid.

Care must be taken to place pitchers or other vessels to receive the liquor which flows through the tube; and in order to prevent the lixivium from absorbing carbonic acid from the atmosphere, the vessels must be slightly closed to prevent the circulation of the external air.

It is likewise necessary that a supply of water should be kept up over the mixture. When it comes out of the tube in an insipid state, it is unnecessary to draw off any more.

The solutions obtained are all to the last very nearly of the same strength; for they become weak all at once: from this circumstance the very aqueous solutions are avoided.

Iron pots may be used to evaporate the waters. The last run must be taken first, because it is rather weakest; and by this means the stronger solutions are not so long exposed to the contact of the air. The evaporation must be performed by a strong ebullition.

When the concentration has arrived to a certain point, the sulphate of pot-ash crystallizes and falls down. It may easily be taken out by placing at the bottom of the pot a perforated iron ladle, in which the salt is collected. Strong ebullition is necessary to keep the atmospheric air at a distance from the surface, and towards the end the motion of boiling serves to carry the sulphate of pot-ash into the ladle.

If the lapis causticus be wanted, the concentrated liquor is to be poured into a smaller pot, and the evaporation carried so far that the salt may congeal when poured out on a plate of iron or marble*.

If a purer alkali be required for the delicate experiments of chemistry, instead of pot-ash, the tartareous acidule or cream of tartar may be calcined and made use of; or otherwise the fused pot-ash before mentioned may be purified by alcohol after the manner of Berthollet. Experiment has proved to us, that very pure alkali may by this means be obtained†.

In this case, the lixivium is to be evaporated to the consistence of a thick syrup in a silver basin‡, or preferably in close vessels. This matter is then to be dissolved in alcohol. The pot-ash alone combines with the fluid: the sulphate and muriate of pot-ash, the portions of earth and even of carbonic acid which it obstinately retains, or which it has resumed from the air during the evaporation, remain at the bottom of the solution. If the alcohol has been poured on the matter whilst still hot, and a less quantity of this solvent has been employed than was requisite for dissolving the pot-ash, it crystallizes by cooling in white blades, which are sometimes several inches long. If it be required to separate the pot-ash from the alcohol, and to obtain it in the dry state, the solution must be evaporated in a silver basin, and not in a glass vessel; for the pot-ash often dissolves a portion of flint, which alters its purity.

We see by this operation, of which the details deserve to be consulted, in the memoir of Berthollet, that the caustic pot-ash is deprived of flint, of carbonic acid, of all the foreign salts, and of the small portion of iron it may have taken up from the vessel in which the evaporation was performed.

* It is proper to remark, that the solid caustic alkali thus prepared is much more active than that usually made by chemists for sale. For this reason it is necessary to be very prudent in its application. G.

† See, however, the remark of Lowitz on this subject. "Philos. Journ. I. 165. N."

‡ What follows is extracted from the memoir of the celebrated Berthollet, for the advantage of those who may not possess the means of consulting the original. G.

X.

A TABLE for reducing the Units of the Metre, Litre, and Gramme into English Inches, Gallons, and Grains. (See page 199).

Fr. Measures	LITRES IN				Fr. Measures	LITRES IN			
	Metres in Inches.	Cubic Inches.	Me. Gal- lons of 28 Inches.	Wine Gal- lons of 231 Inches.		Metres in Inches.	Cubic Inches.	Me. Gal- lons of 28 Inches.	Wine Gal- lons of 231 Inches.
1	39.383	61.083	0.2166	0.2611	51	2008.5	3115.2	11.047	13.486
2	78.766	122.17	0.4332	0.5221	52	2017.9	3176.3	11.264	13.750
3	118.15	183.25	0.6498	0.7933	53	2087.3	3237.4	11.480	14.015
4	157.53	244.33	0.8664	1.0577	54	2126.7	3298.5	11.697	14.279
5	196.91	305.41	1.0830	1.3221	55	2166.1	3359.6	11.913	14.544
6	236.30	366.50	1.2996	1.5866	56	2205.4	3420.6	12.130	14.808
7	275.68	427.58	1.5162	1.8510	57	2244.8	3481.7	12.347	15.072
8	315.06	488.66	1.7328	2.1154	58	2284.2	3542.8	12.563	15.337
9	354.45	549.75	1.9495	2.3799	59	2323.6	3603.9	12.780	15.601
10	393.83	610.83	2.1661	2.6443	60	2363.0	3665.0	12.996	15.866
11	433.21	671.91	2.3827	2.9087	61	2402.4	3726.1	13.213	16.130
12	472.60	733.00	2.5993	3.1731	62	2441.7	3787.1	13.430	16.395
13	511.98	794.08	2.8159	3.4376	63	2481.1	3848.2	13.646	16.659
14	551.36	855.16	3.0325	3.7020	64	2520.5	3909.3	13.863	16.923
15	590.74	916.24	3.2491	3.9664	65	2559.9	3970.4	14.079	17.188
16	630.13	977.33	3.4657	4.2309	66	2599.3	4031.5	14.296	17.452
17	669.51	1038.41	3.6823	4.4953	67	2638.7	4092.6	14.513	17.717
18	708.89	1099.5	3.8989	4.7597	68	2678.0	4153.6	14.729	17.981
19	748.28	1160.6	4.1155	5.0242	69	2717.4	4214.7	14.946	18.246
20	787.66	1221.7	4.3321	5.2886	70	2756.8	4275.8	15.162	18.510
21	827.04	1282.7	4.5487	5.5530	71	2796.2	4336.9	15.379	18.774
22	866.43	1343.8	4.7653	5.8174	72	2835.6	4398.0	15.596	19.039
23	905.81	1404.9	4.9819	6.0819	73	2875.0	4459.1	15.812	19.303
24	945.19	1466.0	5.1985	6.3463	74	2914.3	4520.2	16.029	19.568
25	984.57	1527.1	5.4151	6.6107	75	2953.7	4581.2	16.245	19.832
26	1023.95	1588.2	5.6318	6.8752	76	2993.1	4642.3	16.462	20.097
27	1063.33	1649.2	5.8484	7.1396	77	3032.5	4703.4	16.679	20.361
28	1102.71	1710.3	6.0650	7.4040	78	3071.9	4764.5	16.895	20.625
29	1142.1	1771.4	6.2816	7.6684	79	3111.3	4825.6	17.112	20.890
30	1181.5	1832.5	6.4982	7.9329	80	3150.6	4886.6	17.328	21.154
31	1220.9	1893.6	6.7148	8.1973	81	3190.0	4947.7	17.545	21.419
32	1260.3	1954.7	6.9314	8.4617	82	3229.4	5008.8	17.762	21.683
33	1299.6	2015.7	7.1480	8.7262	83	3268.8	5069.9	17.978	21.948
34	1339.0	2076.8	7.3646	8.9906	84	3308.2	5131.0	18.195	22.212
35	1378.4	2137.9	7.5812	9.2550	85	3347.6	5192.1	18.412	22.476
36	1417.8	2199.0	7.7978	9.5194	86	3386.9	5253.1	18.628	22.741
37	1457.2	2260.1	8.0144	9.7839	87	3426.3	5314.2	18.845	23.005
38	1496.6	2321.2	8.2310	10.0483	88	3465.7	5375.3	19.061	23.270
39	1535.9	2382.2	8.4476	10.3128	89	3505.1	5436.4	19.278	23.534
40	1575.3	2443.3	8.6642	10.5772	90	3544.5	5497.5	19.495	23.799
41	1614.7	2504.4	8.8808	10.8416	91	3583.9	5558.6	19.711	24.063
42	1654.1	2565.5	9.0975	11.1060	92	3623.2	5619.6	19.928	24.327
43	1693.5	2626.6	9.3141	11.3704	93	3662.6	5680.7	20.144	24.592
44	1732.9	2687.7	9.5307	11.6348	94	3702.0	5741.8	20.361	24.856
45	1772.2	2748.7	9.7473	11.8992	95	3741.4	5802.9	20.578	25.121
46	1811.6	2809.8	9.9639	12.1636	96	3780.8	5864.0	20.794	25.385
47	1851.0	2870.9	10.180	12.4280	97	3820.2	5925.1	21.011	25.650
48	1890.4	2932.0	10.397	12.6924	98	3859.5	5986.1	21.227	25.914
49	1929.8	2993.1	10.614	12.9568	99	3898.9	6047.2	21.444	26.178
50	1969.1	3054.1	10.830	13.2212	100	3938.3	6108.3	21.661	26.443

MATHEMATICAL AND PHILOSOPHICAL CORRESPONDENCE.

QUESTION VII. Answered by J. F.—:—:—R.

THE order in which the first 51 cards are distributed being of no importance with respect to the chance for trumps, the solution of this question will be somewhat simplified by conceiving the hands of the dealer's two adversaries to consist of the first 26 cards dealt off. The chance that these may be all of them of the 39 which are not trumps will evidently be,

$$\frac{39 \cdot 38 \cdot 37 \cdot 36 \dots \&c. \dots 14}{51 \cdot 50 \cdot 49 \cdot 48 \dots \&c. \dots 26} = \frac{25 \cdot 24 \cdot 23 \cdot 22 \dots \&c. \dots 14}{51 \cdot 50 \cdot 49 \cdot 48 \dots \&c. \dots 40} = \frac{19}{580027} = \frac{1}{30527\frac{1}{2}},$$

so that the odds against it are 30526 $\frac{1}{2}$ to 1; and yet it is 5200300 times more probable that the dealer and his partner should have the thirteen trumps between them, than that the dealer himself should hold them all in his own hand.

De Moivre, in his "Doctrine of Chances," (2d edit. cor. to prob. xix.) gives a theorem respecting the chance of drawing any certain number of black and white counters, which is applicable in such an infinite variety of cases, that it is perhaps worth mentioning. It may be thus generalised:—If a and b be the respective numbers of things of two different kinds, heaped promiscuously together, the following formula will express the value of the chance, that out of c of them, taken at hazard from amongst the whole number $n (= a + b)$ there be found the exact number p , and no more, of that kind whose whole number is a :

$$c \cdot c - 1 \cdot c - 2 \dots \&c. (p \text{ terms}) \times d \cdot d - 1 \cdot d - 2 \dots \&c. (a - p \text{ terms}) \times \frac{a}{1} \cdot \frac{a-1}{2} \cdot \frac{a-2}{3} \dots \&c. (p \text{ terms}) \\ n \cdot n - 1 \cdot n - 2 \cdot n - 3 \dots \&c. (a \text{ terms}).$$

wherein d is $= n - c$. A proper substitution in this expression will also give us the same result as above.

QUESTION VIII. Answered by J. F.—:—:—R.

LET a and b be the hyperbolic logarithms of a and b . Then will a^x be $= h \cdot 1 \cdot a^x$ and $1 + a \cdot x + \frac{a^2}{2} x^2 + \frac{a^3}{2 \cdot 3} x^3 + \frac{a^4}{2 \cdot 3 \cdot 4} x^4 \dots \&c. = a^x$; and, in like manner, $1 + b \cdot x + \frac{b^2}{2} x^2 + \frac{b^3}{2 \cdot 3} x^3 + \frac{b^4}{2 \cdot 3 \cdot 4} x^4 \dots \&c. = b^x$; the sum of which $2 + (a + b)x + \frac{a^2 + b^2}{2} x^2 + \frac{a^3 + b^3}{2 \cdot 3} x^3 + \frac{a^4 + b^4}{2 \cdot 3 \cdot 4} x^4 \dots \&c. = c$, by the question. Hence, putting $d = c - 2$, and $\alpha, \beta, \gamma, \delta, \epsilon, \&c. =$ the above co-efficients of the several powers of x in the latter series, we get $x = \frac{1}{\alpha} d - \frac{\beta}{\alpha^3} d^3 + \frac{2\beta^2 - \alpha\gamma}{\alpha^5} d^5 + \frac{5 \cdot \alpha\beta\gamma - 5\beta^3 - \alpha^2\delta}{\alpha^7} d^7 \dots \&c.$ As, however, this series does not, when d is great in respect of α , begin to converge till after a considerable number of terms, it seems better to use the common tentative process, repeatedly assuming two values of x , and applying the following proportion:—Difference of results : difference of assumed values :: least error : correction for the nearest value.—This method, though an indirect one, has certainly great practical advantages in a variety of cases wherein the expressions are so entangled with surds or unknown exponents as not to be otherwise reducible without a great deal of trouble.

Another method of obtaining the value of x is by means of a table of artificial sines, as follows:—Let r be the logarithmic radius, a , b , and c the common logarithms of a , b , and c , and $d = r - \frac{1}{2}c$. Find an artificial sine, and its correspondent cosine, f and s , in the tables, so that $\frac{f-d}{\frac{1}{2}a}$ may be $= \frac{s-d}{\frac{1}{2}b}$, each of which quantities will then be $= x$. The demonstration becomes obvious by considering, that if one of three quantities be equal to the sum of the other two, their square-roots are the sides of a right-angled triangle.

Nearly a similar answer was given to this question by ANALYTICUS.

NEW MATHEMATICAL QUESTIONS.

QUESTION XI. By W. SIMPSON.

IF H , h , be the heights of any two signals above the horizon of an observer, A , the angle which they subtend, and a , the same angle reduced to the plane of the horizon, then will $\cos. A = \cos. a \times \cos. H \times \cos. b + \sin. H \times \sin. b$. Required the demonstration.

QUESTION XII. By W. C. of Greenwich.

ON the 1st of May 1797, the sun's declination being $15^{\circ} 9'$, it was observed that his altitude, azimuth, and the latitude of the place were all equal. Required the hour and place where the observation was made.

* * I SHOULD be glad to give a description and drawing of Mr. Varley's machine for producing perpetual motion, as requested by Mr. Notlem of Wisbech, if an attentive perusal of the specification enrolled in Chancery had shewn me any thing tending to improve the theory or practice of mechanics. The description in the periodical work he mentions is not sufficiently clear to shew the whole of what the writer meant to explain, and I found the original equally imperfect. Mr. Varley's notion, obscured by some extraneous and unimportant circumstances, appears to be, that if an exhausted cylinder be fixed to one part of the periphery of a wheel, and a piston fitted therein, the pressure of the atmosphere on this last, supposed also to be attached to the wheel by a spring and chain (parallel to a tangent), will tend to drive it into the vacuum, and, if prevented by the shortness of the chain, will draw the wheel round. It is obvious to any person moderately acquainted with statics, that the pressures on his wheel must counterbalance each other, and cannot produce motion.

It has always been easy to shew the fallacy of schemes for perpetual motion in the particular instances; but I have met with no clear enunciation of this project so general as to include every possible scheme, and evince its own absurdity. The difficulty of performing this seems to arise from a want of direct and concise demonstrations of the fundamental principle of the lever, and of the equal pressure of fluids in all directions.

* * THE correspondent who desires an explanation whence the sulphate of pot-ash mentioned in the last paragraph of page 218, was derived, is reminded, that this salt is the most abundant impurity in the common vegetable alkali. Kirwan, in the Irish Memoirs for 1789, states its amount in Dantzick pearl-ash at 505 grains in the pound troy, which is upwards of one seventh of the weight of the pure alkali. As Professor Siegling made his liquor of flints by the ordinary process, it is not to be supposed that his alkali had undergone any very laborious purification.

PUBLICATIONS ON GALVANISM.

THE celebrated Aldini, Professor of Natural Philosophy at Bologna, has forwarded to the first class of the National Institute a collection of works published in Italy, since the discovery made by Galvani, of the irritation produced in animal matters by the metals, and which treat on that subject. These works having been hitherto little circulated, Citizen Guyton, from whose announce in the 22d volume of the Annals of Chemistry, page 323, this extract is taken, naturally concluded that the list would be acceptable to the world. The paper of Mr. Humboldt, of which I have given a translation * ; another memoir by the same author, on the chemical process of vitality, together with certain truly philosophical remarks upon the same by Citizen Fourcroy, all which are inserted in the 22d volume of the same Annals, have shewn the great importance of a phenomenon which has engaged the attention of all Europe. The first class of the Institute of France has considered this as an object most worthy of investigation, and has for that purpose nominated a commission of seven members, who for five months past have been engaged in experiments proper to determine the circumstances, and develop its nature.

The following is the list. The annexed remarks are made by Citizen Guyton.

Aloysii Galvani in Bononiensi Archigymnasio Professoris, etc. de Viribus Electricitatis in Motu musculari, Commentarius. Cum Joan. Aldini Dissertatione et Notis. Mutinæ, 1792. Small folio, 80 pages, with three engraved plates.

This work contains a letter of Bassani Carminefi, which exhibits the opinion of Volta concerning the feat of animal electricity.

Joannis Aldini de Animalis Electricitate Dissertationes duæ. Bononiæ, 1794. Small folio, 41 pages, with figures.

Dell' Uso e dell' Attività dell' Arco Conduttore nelle Contrazioni dei Muscoli. In Bologna, 1794. Small quarto, 168 pages. With a Supplement of 23 pages.

Memoria intorno all' Elettività Animale, del Sig. Dott. Gio. Aldini, Prof. di Fisica nell' Università di Bologna, etc. 10 June, 1794. In quarto, 12 pages.

* Philof. Journ. I. 256.

Prima Brunonis Theorizæ Rudimenta. Bononiæ, 1797. Octavo, 16 pages.

This paper of Aloysius Zanottus is written in the form of a letter addressed to Charles Mundini.

It contains a development of Dr. Brown's opinion, that irritability is one and indivisible relative to the system of the nerves; that is to say, that this force is distributed through every point of the system, in the same manner as velocity, impressed on any body or system of bodies, appertains equally to all the elements of this body or system.

Irritants are distinguished into extrinsic and intrinsic.

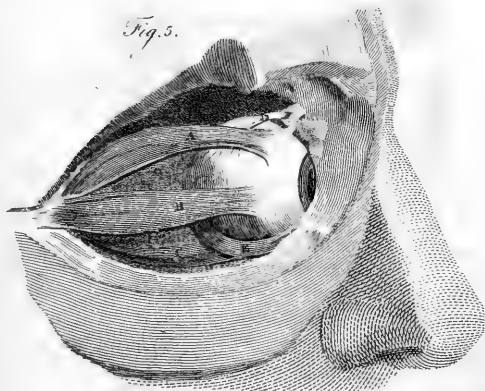
The extrinsic are heat, light, sound, food, &c. The blood and other animal fluids, though appertaining to the system of the animal, appear to act on its economy like extrinsic objects.

The intrinsic irritants are the functions of the system, such as the muscular contraction, the exercise of the senses, the action of thought, the movements of the brain, &c.

The common effect of these forces is motion. Each possesses a certain activity called stimulus. There cannot be such an agent as a sedative; for, when the irritants produce weakness, this weakness must be attributed to a defect of the stimulating power in degree, &c. &c.



Fig. 5.



Focal adjustment of the Eye.

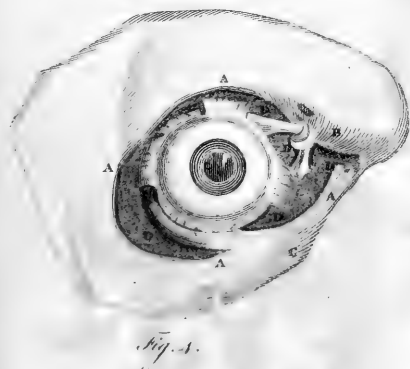


Fig. 4.

Fig. 2.



Fig. 3.



Fig. 1.

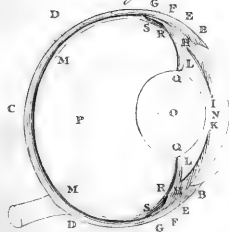
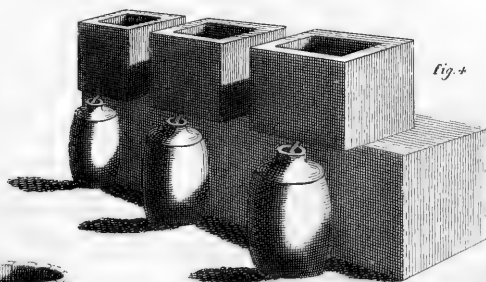
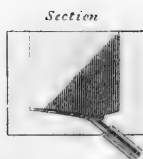
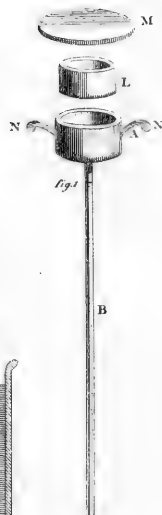
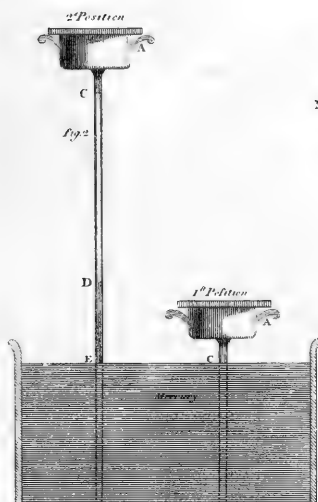
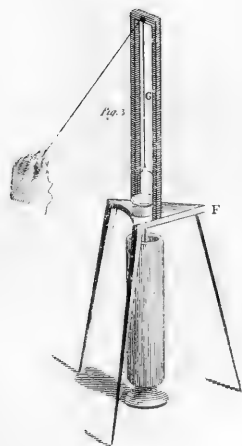


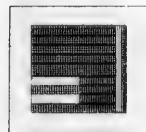
Fig. 6.







Plan



Scale of *Feet*



A
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OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

NOVEMBER 1797.

ARTICLE I.

Experiments and Observations on the Nature of Sugar. By WILLIAM CRUICKSHANK, Chemist to the Ordnance, and Surgeon of Artillery.

SUGAR has been supposed to be a substance intermediate between mucilages and vegetable acids, containing more oxygen than mucilage, and less than the acids. To ascertain this and some other circumstances, the following experiments were instituted :

Two ounces of refined sugar were introduced into a retort, and exposed to a heat gradually increased until its bottom became red-hot : there came over into the receiver seven drachms of a sharply acid liquor, which required 132 grains of a solution of pot-ash to saturate it : this liquor was mixed with a little empyreumatic oil : the charry residuum which remained in the retort weighed seven drachms ; the quantity of gas which escaped during the operation must therefore have amounted to two drachms : some of this was examined, and found to consist of a mixture of carbonic acid gas and hydro-carbonate.

Two ounces of gum-arabic were introduced into a retort at the same time, and exposed to a heat in every respect similar : the quantity of acid liquor which came over into the receiver amounted to seven drachms and 15 grains ; this contained a little more empyreumatic oil, but was not so sharp as that obtained from the sugar, and required only 117 grains of the same solution of pot-ash to saturate it : the charry residuum which remained in the retort weighed five drachms and 45 grains ; the quantity of elastic fluid or gas which escaped during this process must therefore have amounted to three drachms : it consisted, like the former, of a mixture of hydro-carbonate and carbonic acid gas ; but towards the end of the operation the proportion of hydro-carbonate was more remarkable. From these experiments it would appear that sugar yields by distillation more pyromucous acid than

gum, in the proportion of 132 to 117. The residuary charcoal of the sugar also exceeded that of the gum by 1-7th; but this may in some measure be accounted for from the greater quantity of the hydro-carbonate yielded by the latter. As oxygen is now allowed to be the universal acidifying principle, and as the acid yielded in both instances (viz. the pyromucous) was exactly of the same kind, it may be reasonably inferred, that the sugar which afforded the greatest quantity of acid contained likewise the greatest proportion of oxygen; for it is probable that both the carbonic acid and the hydro-carbonate were formed from the decomposition of the water by the carbons of these substances, as neither was produced in any quantity until near the end of the operation: the oxygen, therefore, contained in the former should not be considered as entering essentially into the composition of either the gum or sugar.

It is well known that vegetable mucilages and fecula are somehow converted into sugar by malting: we conceived, therefore, that it would throw considerable light on this subject, to observe with more attention than had hitherto been done, the particular changes and decompositions which take place during this process: it was with this view that the following experiments were made:

December 1, 1796. A quantity of barley, after being soaked in water for 24 hours, was put into a wine-glass, and introduced into a jar containing common air, and inverted over water: the temperature in this and the following experiments was preserved between 60 and 70 as nearly as possible. At the end of five days it began to grow, and on the 28th the greatest part had thrown out shoots at least half an inch in length. On January 7, vegetation was still going on, and the air in the jar had somewhat diminished: the barley being now withdrawn was found to be very sweet, and nearly converted into the state of malt. The air in the jar was found to consist of azotic and carbonic acid gas in the proportion of 20 to 6, the whole of the oxygen being either absorbed, or converted into carbonic acid.

January 19, 1797. A quantity of barley, previously steeped in water for 48 hours, was introduced, as in the last experiment, into a jar containing oxygen gas, and inverted over water to which sulphuric acid had been added. At the end of three days it began to grow, and this process went on to the 29th. The water had now risen considerably in the jar, the gas having suffered a diminution of about one-third. The barley being withdrawn smelled completely of malt, and tasted sweet. The gas in the jar, on examination, was found to consist of 64 parts carbonic acid, 32 azote, and 4 oxygen; from which it would appear that the air employed in this experiment had contained originally about 20 per cent. of azotic gas.

To be more certain of the nature of the change which the pure air undergoes in this process, the experiment was repeated as follows:

January 23. A quantity of barley, soaked in water for two days, was introduced into a jar containing 46 measures of very pure oxygen gas, and inverted over mercury. At the end of three days the barley began to grow, and this process continued for ten days, although very slowly: the column of gas remained exactly of the same height, so that it had undergone no apparent diminution or increase: the barley being withdrawn, the air in the jar was examined, and found to consist of carbonic acid gas, mixed with only 1-55th of its bulk of oxygen gas. The barley was partly converted into malt, the quantity of oxygen being insufficient to produce this change upon the whole.

Another

Another experiment with common air was made at the same time, and exactly under similar circumstances. In this case the barley did not begin to grow until the end of the fourth day; and at the end of ten days had made much less progress than that in the oxygen gas. It was now withdrawn, and the air in the jar, which had increased a little, examined; when it was found to consist of carbonic acid and azotic gas, in the proportion of one to two very nearly, mixed with a very small quantity of oxygen gas. A little of the barley tasted sweet.

Being now satisfied that during the evolution of the saccharine principle from vegetable mucilage, a quantity of oxygen was either absorbed or converted into carbonic acid, we wished to know if this process could take place in any degree without the presence of this gas.

In order to determine this point the following experiments were made:

January 20. A quantity of barley, soaked as in the former experiments, was introduced into a jar filled with, and inverted over, mercury. At the expiration of 12 days a very considerable quantity of gas was produced, at least five or six times the bulk of the barley; but nothing like vegetation was perceivable. The gas, on examination, was found to consist of carbonic acid, being entirely absorbed by lime-water. The barley had not the least sweet taste, nor did it appear to have undergone any sensible change.

On January 20th, another portion of the same soaked barley was introduced into a wine-glass, and placed in a jar containing nitrous gas inverted over water. At the expiration of ten days the gas had undergone a slight diminution, but there was not the smallest appearance of vegetation. The barley being withdrawn and examined was found to have undergone no apparent change. The gas contained about one-ninth of its bulk of carbonic acid, the remainder being pure nitrous gas, as was manifest from the diminution it underwent when mixed with pure air. The nitrous gas which disappeared in this instance must have been absorbed either by the barley or the water. The carbonic acid which was found mixed with it is accounted for by the last experiment.

Two other portions of soaked barley were introduced into jars, the one containing hydrogenous and the other azotic gas, and inverted over mercury. At the expiration of 12 or 14 days there was not the least appearance of vegetation in either, but the gas in both had increased in bulk about one-fifth. The barley being withdrawn and examined, that in the hydrogenous gas tasted musty, but not in the least sweet; the portion in the azote appeared to have undergone no change. The gas in both jars contained from one-third to one-fourth of its bulk of carbonic acid, the remainder being the original gases not sensibly changed.

From these experiments, therefore, it is manifest that oxygen is absolutely necessary for the conversion of vegetable mucilage into sugar; as in no one instance was saccharine matter formed where this was not present, and the quantity of the former was always in proportion to that of the latter; for we found in all the experiments, that when the oxygen was consumed this process immediately ceased.

It may still remain doubtful, whether the oxygen is absorbed by the barley, or merely converted into carbonic acid: we are inclined to think that it is chiefly absorbed, although part may also be consumed in the formation of this acid; for we have seen that carbonic acid is formed without the presence of oxygen gas, and that in very considerable quantity;

which we conceive must proceed from the decomposition of the water, whose oxygene unites with the carbonaceous principle of the barley, whilst its hydrogen is fixed, and may be necessary to the production of the saccharine principle. We suppose, therefore, that vegetable mucilage is converted into sugar by being deprived of part of its carbone, whilst at the same time it is combined with a greater proportion of oxygene, and probably also with hydrogen from the decomposition of the water. Thus, then, both from analysis and synthesis, it would appear that sugar contains more oxygene than gum or mucilage. From this hypothesis it should follow, that if sugar be deprived of part of its oxygene, it must lose its sweetness, and form something like a gum. To see how far this might be accomplished was the object of the following experiments:

A quantity of syrup was introduced into a jar, filled with, and inverted over, mercury: to this was admitted about an equal quantity of the phosphuret of lime: a considerable production of phosphoric gas almost immediately took place, and the mercury descended in the jar. At the expiration of eight days the syrup was withdrawn and examined: it had no sensibly sweet taste, but rather a bitter astringent one: when filtered, alcohol produced a copious white precipitate in flakes, very much resembling mucilage separated from water by the same substance.

This experiment was somewhat varied, as follows: A little refined sugar was dissolved in alcohol, and to this solution a little phosphuret of lime was added: no phosphoric gas was disengaged, nor was there any apparent action produced. More phosphuret being added, the mixture was allowed to remain in an open phial for several days. The alcohol having now evaporated, some distilled water was added; but this produced no disengagement of gas, as the phosphuret had been decomposed, and converted principally into phosphate of lime. The mixture being filtered, and the clear liquor evaporated, there remained a substance extremely tenacious, and which had much the appearance of gum-arabic: its taste was bitter, with a very slight degree of sweetness: when squeezed between the teeth it had exactly the feel of gum, but more tenacious. It did not appear to be soluble in alcohol, or at least in any considerable quantity: when thrown upon a red-hot iron it burned like gum, and left a bulky and insipid charcoal.

It would appear that the saccharine principle had been destroyed in these experiments, and converted into something resembling a gum. That this was effected by the abstraction of oxygene is rendered highly probable from the nature of the substance employed, and the change which it was found to have undergone; for there are few substances which have so strong a tendency to combine with oxygene as the phosphuret of lime.

Some other trials of a similar nature were made by mixing solutions of sugar with the different sulphurets, and by agitating them with nitrous gas in close vessels. The sulphurets, more especially that of pot-ash, manifestly destroyed the saccharine taste; but on account of the solubility of the different products, the particular change produced could not be so easily and accurately ascertained. The action of the nitrous gas was more doubtful.

In order to be satisfied how far the effects produced on the sugar in the former experiments might be owing to the abstraction of oxygene, I added to solutions of this substance in water both lime and pure pot-ash, and boiled the mixtures for some time: the lime appeared manifestly to combine with the sugar, to which it communicated a very bitter astringent taste; but it was still sweet: a little alcohol, added to the filtered solutions, produced

duced a precipitate in white flakes, somewhat similar to that in the experiments with the sulphuret of lime, and which appeared to be a combination of sugar with lime. Some of the filtered solution being evaporated by a gentle heat, there remained a semi-transparent substance, much more tenacious than the thickest syrup, but not equal to that produced by the phosphuret of lime; and it had a rough bitter taste, mixed with a certain degree of sweetness. The pot-ash, likewise, appeared to combine with the sugar, the sweet taste being more completely destroyed than by the lime: but on the addition of sulphuric acid, sulphat of pot-ash was formed; and this being precipitated by alcohol, the sweetness appeared to be completely restored. It may likewise be proper to observe, that when alcohol was added to a portion of the solution of sugar and pure pot-ash, after it had been boiled to the consistence of a syrup, no union took place; but the alcohol, notwithstanding the mixture was completely and repeatedly agitated, still swam pure on the top: a circumstance which would seem to prove that a new compound is formed by these substances, which is not soluble in this fluid, although they are both completely so in a separate state.

Having found that sugar might be converted into a species of gum by depriving it of part of its oxygen, we conceived that gum might, by the addition of oxygen, be changed into a substance resembling sugar: but although several trials were made with a view of combining oxygen in different proportions with gum-arabic, no remarkably sweet taste was ever perceived; on the contrary, in every experiment, it seemed to run very readily into the acid state, particularly when it was exposed to the action of the oxygenated muriatic acid gas.

Indeed, when we reflect on the change which vegetable mucilage must undergo in the process of malting, the simple addition of oxygen does not appear to be sufficient; for it is probable, from the decomposition of the water, that some of its hydrogen is fixed, whilst its oxygen disengages and unites with a certain portion of charcoal, forming the carbonic acid. Although, therefore, sugar and mucilage consist of the same principles, viz. carbone, hydrogen, and oxygen; yet unless these are combined in certain determinate proportions, the former, which when pure is no doubt always a substance of exactly the same nature, cannot be produced: the hydrogen and carbone must be accurately proportioned, as well as the oxygen*.

II.

An Account of the Manner in which Heat is propagated in Fluids, and its general Consequences in the Economy of the Universe. By BENJAMIN COUNT of RUMFORD.

[Continued from page 296.]

FROM the internal motions of the particles of fluids during the propagation of heat, and from this propagation being impeded by every thing which obstructs those motions, results amounting to a theory of considerable perfection were obtained. But the subject in the hand of our author naturally produced other interesting consequences. It was by

* This paper is, with the author's consent, extracted from Dr. Rollo's account of two cases of Diabetes Mellitus, of which an account is given in this Journal, p. 235.

the motions of very fine particles of dust, accidentally mixed with the spirits of wine in his large thermometer, and strongly illuminated by the sun, that he first discovered the internal motions which take place in that fluid during the time of its cooling. Reflecting on this fact, he immediately concluded that the internal motions of water might be rendered equally visible if he could find any solid body of the same specific gravity as water, and not liable to be dissolved by it; but such a substance was not to be found. On reflection, it occurred to him, that it is very fortunate that such substances do not abound; for otherwise we should find great difficulty in procuring water in a pure state.

As the solid could not be found, he determined to adapt the specific gravity of the fluid to that of a solid very little heavier than the water in its pure state. The solid he made choice of was transparent yellow amber, of the specific gravity 1.078, and he increased the density of the water by the addition of a certain quantity of pure alkaline salt.

The amber was broken into pieces about the size of mustard-seeds, and put into a glass vessel with water, to the bottom of which it sunk. Upon the gradual addition of an alkaline solution some of the pieces rose to the top, and others subsided to the bottom. The former were removed, and the fluid was carefully adjusted with regard to density, until the latter, at 60° of Fahrenheit, remained permanently suspended in the different parts of the fluid.

A glass vessel, consisting of a globe about two inches in diameter, with a cylindrical neck three quarters of an inch in diameter and twelve inches long, was filled with this prepared liquid. The first experiment made with this instrument was to plunge it into a tall glass jar, nearly filled with water almost boiling hot. Two currents, in opposite directions, began to move at the same instant with great celerity in the liquid in the cylindrical tube, the ascending current occupying the sides of the tube, while that which moved downwards occupied its axis.

As the saline liquor grew warm, the velocity of these currents gradually diminished; and at length when the liquor had acquired the temperature of the surrounding water in the jar the motion ceased entirely. On taking the glass body out of the water, the internal motions of the liquor recommenced: but the currents had changed their directions; that which occupied the axis of the tube being now the ascending current. When the cylindrical tube was inclined a little, the ascending current occupied the upper side, and the descending current the lower. Both ceased when the instrument had acquired the common temperature of the room.

In all cases when the instrument received heat, the current in the axis of the tube, or on its lower side if inclined, moved downwards; but when it parted with heat this motion was in the opposite direction. A change of a few degrees was sufficient to set the contents of the instrument in motion. By the application of an heated body, the fluid nearest the place of contact was made to ascend; but the contrary effect took place if the body were colder than the instrument. And in either case the motion thus produced was attended with a contrary motion in some other part of the instrument.

Among the applications made by this enlightened philosopher to an instrument so calculated to detect the secret operations of nature, one consisted in applying a lighted candle to the middle of the tube while it was inclined in an angle of about 45 degrees from the perpendicular. In this situation the motion of the fluid in the upper part of the tube became excessively rapid, while that in the lower end, where it was united to the globe, as well as that

that in the globe itself, remained almost perfectly at rest. He even found that he could make the fluid in the upper part of the tube actually boil, without that in the lower part of it appearing to the hand to be sensibly warmed. This fact not only served to shew that heat is propagated in fluids chiefly, if not altogether, by virtue of these motions which arise from change of density, but also that this transportation cannot be effected in a downward direction.

It is an opinion generally received among philosophers, that water cannot be heated in contact with ice. Our author saw that this position must be in a great measure true with regard to water upon which ice floats; because the hot water must ascend and be cooled by causing part of the ice to melt, while the cold water from this last would descend to the bottom, and tend to preserve the uniformity of temperature. But he saw likewise that the case would be very different if the ice were at the bottom. The inferences which this course of argument would point out are singular and striking, and they are not less so for being announced to us under the incontrovertible sanction of experiment. Natural as the induction is, from the valuable experiments of Count Rumford, it still seems in some measure astonishing to assert, that water may be actually made to boil, and kept in that state without melting a piece of ice, plunged in it, with more rapidity than could be effected by cold water at 40 degrees.

The direct experiments with ice and hot water are related at large in the Essay. A circular cake of ice, three inches and an half thick, weighing $10\frac{1}{2}$ ounces, and nearly as large as the internal diameter of a cylindrical glass jar, which was 4.7 inches (its height being 1.4 inches), was gently put into the jar containing six pounds 14 ounce troy of boiling-hot water. It was entirely melted in two minutes and fifty-eight seconds.

The same experiment was repeated; but instead of the ice being suffered to swim at the surface, it was fastened down in the bottom of the jar, and the hot water poured upon it. The ice was retained in the jar by means of two slender and elastic pieces of deal about $\frac{1}{8}$ th of an inch thick, and $\frac{1}{4}$ th of an inch wide, which, being a very little longer than the internal diameter of the jar, were of course slightly bended when they were introduced into it horizontally; and on being put down to the ice at right angles to each other, they confined it from rising to the surface when the water was added. A circular piece of strong writing-paper was laid upon the ice to protect it from the action of the boiling-hot water while poured, which was afterwards removed, as gently as possible, by means of a string fastened on one side; and to prevent the glass jar from being cracked by the sudden application of the boiling-hot water, a small quantity of cold water was first poured in, to cover the ice to the height of about a quarter of an inch. The hot water was poured against the middle of the circular piece of paper.

The jar with the ice and the hot water in it being placed on a table near a window, the paper which covered the surface of the ice was gently drawn away, and the Count prepared himself to observe at his ease the result of this most interesting experiment.

A very few moments were sufficient to shew that his expectation with regard to it would not be disappointed. In the former experiment a similar cake of ice had been entirely melted in less than three minutes; but in this, after more than twice that time had elapsed, the ice did not shew any apparent signs of beginning to melt. Its surface remained smooth and shining, and the water immediately in contact with it appeared to be perfectly at rest;

though the internal motions of the hot water above it, which was giving off its heat to the sides of the jar and to the air, were very rapid, as was distinctly perceived by means of some earthy particles or other impurities which this water happened to contain.

The ice was examined with a very good lens, but it was a long time before any signs of its melting could be perceived. The edges of the cake remained sharp; and the minute particles of dust, which by degrees were precipitated by the hot water as it grew colder, remained motionless as soon as they touched the surface of the ice.

As the hot water had been brought from the kitchen in a tea-kettle, it was not quite boiling-hot when it was poured into the jar. After it had been in the jar one minute, a thermometer was plunged into it, and its temperature found to be at 180°.

After 12 minutes had elapsed, its temperature at the depth of one inch under the surface was 170°. At the depth of seven inches, or one inch above the surface of the ice, it was at 169 $\frac{1}{2}$ °; while at only 3-4ths of an inch lower, or 1-4th above the surface of the ice, its temperature was 40°.

When 20 minutes had elapsed, the heat in the water at different depths was found to be as follows :

	Degrees.
Immediately above the surface of the ice	40
At the distance of half an inch above it	46
At 1 Inch	130
At 3 Inches	159
At 7 Inches	160

When 35 minutes had elapsed, the heat was as follows :

	Degrees.
At the surface of the ice	40
Half an Inch above it	76
1 Inch above it	110
2 Inches	144
3 Inches	148
5 Inches	148 $\frac{1}{2}$
7 Inches	149

At the end of one hour the heat was as follows :

	Degrees.
At the surface of the ice	40
1 Inch above it	80
2 Inches	118
3 Inches	128
4 Inches	130
7 Inches	131

After one hour and 15 minutes had elapsed, the heat was found to be as follows :

	Degrees.
At the surface of the ice	40
1 Inch above it	82
2 Inches	106
3 Inches	123

The heat of the water had hitherto been taken near the side of the jar:—in the two following trials it was measured in the middle or axis of the jar.

When

When one hour and 30 minutes (reckoning always from the time when the boiling water was poured into the jar) had elapsed, the heat of the water in the middle of the jar was found to be as follows :

	Degrees.
At the surface of the ice	40
1 Inch above it	84
2 Inches	115
3 Inches	116
7 Inches	117

When two hours had elapsed, the heat in the middle of the jar was found to be as follows :

	Degrees.
At the surface of the ice	40
1 Inch above it	76
2 Inches	94
3 Inches	106
4 Inches	108
6 Inches	108 $\frac{1}{2}$
7 Inches	108 $\frac{1}{2}$

An end being now put to the experiment, the hot water was poured off from the ice ; and on weighing that which remained, it was found that five ounces six grains troy (= 2406 grains) of ice had been melted.

Taking the mean temperature of the water at the end of the experiment at 106°, our author remarks that the mass of hot water (which weighed 73 $\frac{1}{4}$ ounces) was cooled 78 degrees, or from the temperature of 184° to that of 106° during the experiment. Now as it is known that one ounce of ice absorbs just as much heat in being changed to water as one ounce of water loses in being cooled 140 degrees, it is evident that one ounce of water which is cooled 78 degrees, gives off as much heat as would be sufficient to melt $\frac{78}{140}$ ths of an ounce of ice ; consequently the 73 $\frac{1}{4}$ ounces of hot water, which in this experiment were cooled 78 degrees, actually gave off as much heat as would have been sufficient to have melted $\frac{73\frac{1}{4} \times 78}{140} = 40\frac{9}{10}$ ounces of ice.

But the quantity of ice actually melted was only about five ounces ; and hence it appears that less than one-eighth part of the heat lost by the water was communicated to the ice, the rest being carried off by the air.

As the same quantity of hot water was used in this experiment and in that which immediately preceded it, and as this water was contained by the same vessel, it appears that ice melts more than eighty times slower at the bottom of a mass of boiling-hot water than when it is suffered to swim on its surface. For as in the one experiment 10 $\frac{1}{2}$ ounces of ice were melted in two minutes and 58 seconds, five ounces at least must have been melted in one minute and 29 seconds ; but in the other experiments two hours or 120 minutes were employed in melting five ounces.

The ice however was melted, though very slowly, at the bottom of the hot water; and that circumstance alone would have been sufficient to have overturned the hypothesis respecting the propagation of heat in fluids solely by means of the intestine motion. This consideration demanded an attentive enquiry into the circumstances of the experiment. One of the most striking among these was, that at the end of the first half-hour, on examining the surface of the ice it was seen that it had been melted, excepting only where it had been covered, or as it were shadowed, by the flat slips of deal which secured the ice in its place. It was singular that the ice was defended, not only by the undermost piece of wood, but by the other, which, lying across the under piece, did not touch the ice any where except at its ends. It was natural to imagine, from this event, that the ice had been melted by radiant heat or calorific rays from the water, and that a portion of these had been intercepted by the slips of deal. This supposition pointed out another course of experimental enquiry.

Into a cylindrical glass jar, $6\frac{1}{2}$ inches in diameter, and eight inches high, was put a circular cake of ice, as long as could be made to enter the jar, and about $3\frac{1}{2}$ inches thick; and on the flat and even surface of the ice was placed a circular plate of the thinnest tin that could be procured, near $6\frac{1}{2}$ inches in diameter, or sufficiently large just to cover the ice. This plate of tin (which, to preserve its form or keep it quite flat, was strengthened by a strong wire, which went round it at its circumference) had a circular hole in its centre just two inches in diameter; and it was firmly fixed down on the upper surface of the cake of ice, by means of several thin wooden wedges which passed between its circumference and the sides of the jar.

A second circular plate of tin, with a circular hole in its centre, two inches in diameter, and in all other respects exactly like that already described, was now placed over the first, and parallel to it, at the distance of just one inch, and, like the first, was firmly fixed in its place by wooden wedges.

These perforated circular plates being fixed in their places, the jar was placed in a room where Fahrenheit's thermometer stood at 34° , and ice-cold water was poured into it, till the water just covered the upper plate; and then the jar was filled to within half an inch of its brim with boiling water, and, being covered over with a board, was suffered to remain quite two hours.

At the end of this time, the water, which was still warm, was poured off, and, the circular plate being removed, the ice was examined.

A circular excavation, just as large as the hole in the tin plate which covered the ice (namely, two inches in diameter), and corresponding with it, perfectly well defined, and about $2\text{-}10\text{ths}$ of an inch deep in the centre, had been made in the ice.

This was what our author expected to find; but there was something more which he did not expect, and which for some time he was quite at a loss to account for. Every part of the surface of the ice which had been covered by the tin plate appeared to be perfect, level, and smooth, and shewed no signs of its having been melted or diminished, excepting only in one place, where a channel about an inch wide, and a little more than $2\text{-}10\text{ths}$ of an inch deep, which shewed evident marks of having been formed by a stream of warm water, led from the excavation just mentioned in the centre of the upper part of the cake of ice to its circumference. As the edge or vertical side of the cake of ice was evidently worn away where this stream passed, there could be no doubt with respect to its direction.

It certainly ran out of the circular excavation in the middle of the ice; and though it might at first appear difficult to explain the fact, and to shew how this hot water could arrive at that place; yet it was quite evident that the immediate cause of the motion of this stream of water could be no other than its specific gravity being greater than that of the rest of the water at the same depth; and that this greater specific gravity was at the same time accompanied by a higher degree of heat, is evident from the deep channel which this stream had melted in the ice, while other parts of the surface of the ice at the same level were not melted by the water which rested on it.

This experiment, though attended with remarkable circumstances, did not clearly shew that the fusion of the uncovered part of the ice had been effected by radiant heat. It occurred to the author, that radiant heat, like light, might probably be reflected downwards in part from the internal surface of the water; and consequently, that it might be expected that a light black body, namely, a circular piece of deal board, covered over with black silk, would absorb a portion of the whole body of rays which were directed to the ice. This trial was made, but with no sensible effect.

As it is uncertain whether heat in the radiant state can be reflected in this manner, the experiment may perhaps be considered as likely to afford a less equivocal conclusion than might have been obtained from the application of a polished metallic surface to the superior termination of the cylinder of water. But the appearance of a channel worn in the plate of ice in the preceding experiment gave rise to meditations which clearly pointed out the manner in which the ice was melted, and rendered the attempts to detect the supposed operation of latent heat unnecessary. The Count's explanation, in his own words, is as follows:

Though it is one of the most general laws of nature with which we are acquainted, that all bodies, solids as well as fluids, are condensed by cold, yet, in regard to water, there appears to be a very remarkable exception to this law. Water, like all other known bodies, is indeed condensed by cold at every degree of temperature which is considerably higher than that of freezing; but its condensation, on parting with heat, does not go on till it is changed to ice; but when in cooling its temperature has reached to 40 degrees of Fahrenheit's scale, or eight degrees above freezing, it ceases to be farther condensed; and on being cooled still farther it actually expands, and continues to expand as it goes on to lose more of its heat, till at last it freezes; and at the moment when it becomes solid, and even after it has become solid, it expands still more on growing colder. This fact, which is * noticed by M. de Luc, in his excellent Treatise on the Modifications of the Atmosphere, has since been farther investigated and put beyond all doubt by Sir Charles Blagden. (See Philosophical Transactions, vol. lxxviii.)

Now, as water in contact with melting ice is always at the temperature of 32°, it is evident that water at that temperature must be specifically lighter than water which is eight degrees warmer, or at the temperature of 40°: consequently, if two parcels of water at these two temperatures be contained in the same vessel, that which is the coldest and lightest must necessarily give place to that which is warmer and heavier, and currents of the warmer water will descend in that which is colder.

* The simple fact was also observed by the celebrated Robert Boyle.

In the two last experiments, as the circular tin plate which covered the surface of the ice served to confine the thin sheet of water which was between the plate and the ice—as this water could not rise upwards, being hindered by the plate—and as it had no tendency to descend—it is probable that it remained in its place; and as it was ice-cold, it was not capable of melting the ice on which it reposed. But as the tin plate had a circular hole in its centre, the surface of the ice in that part was of course naked; and the ice-cold water in contact with it being displaced by the warmer and heavier water from above, an excavation in the form of a shallow basin was formed in the ice by this descending warm current.

The warm water contained in this basin overflowed its banks as soon as the basin began to be formed; and issuing out on that side which happened to be the lowest, opened itself a passage under the tin plate to the edge of the ice, over which it was precipitated, and fell down to the bottom of the jar. The water of this rivulet being warm, it soon formed for itself a deep channel in the ice; and at the end of the experiment it was found to be everywhere deeper than the bottom of the basin where it took its rise.

This manner of accounting for the appearances in question seemed to the Count to be quite satisfactory; and the more he meditated on the subject, the more he was confirmed in his suspicions that all liquids must necessarily be perfect non-conductors of heat.

On these principles he was now enabled to account for the melting of the ice at the bottom of the hot water, as also for the slowness with which that process went on; and encouraged by this success he proceeded with confidence to plan and to execute still more decisive experiments; from the results of which he considers the important facts in question to have been put beyond all possibility of doubt.

If water be in fact a perfect non-conductor of heat, that is to say, if there be no communication whatever of heat between neighbouring particles or molecules of that fluid (which is what he supposes); then, as heat cannot be propagated in it, but only in consequence of the motions occasioned in the fluid by the changes in the specific gravity of those particles which are occasioned by the changes of their temperature, it follows that heat cannot be propagated downwards in water, as long as that fluid continues to be condensed with cold; and that it is only in that direction (downwards) that it can be propagated after the water has arrived at that temperature where it begins to be expanded by cold; which has been found to be at about the 40th degree of Fahrenheit's scale.

Reasoning on these principles, he was led to this remarkable conclusion: namely, that *water which is only eight degrees above the freezing point, or at the temperature of 40 degrees, must be able to melt as much ice in any given time, when standing on its surface, as an equal volume of water at any higher temperature, even though it were boiling-hot.*

The experiments by which this unexpected result was confirmed and established must be deferred to the concluding part of this abstract.

III.

Experiments and Observations made with the View of ascertaining the Nature of the Gaz produced by passing Electric Discharges through Water; with a Description of the Apparatus for these Experiments. By GEORGE PEARSON, M. D. F. R. S.

[Concluded from page 248.]

THE fire of the electric discharge, in a very condensed state, passes with inconceivable velocity through the whole length of the upper wire, in the case of the interrupted explosion, p. 243—247, and of the single wire in the case of the complete explosion, p. 247, 248; so that it neither exerts its energy on the wire, nor on the water, till it arrives at the extremity of the wire. There it is momentarily interrupted and accumulated; and, in the moment before its diffusion through the water, it is so dense and in such quantity as to manifest itself by a spark at, or nearly in contact with, the extremity of the wire. In the moment of its diffusion, a small part of this condensed fire interposes betwixt the constituent elements of the ultimate and invisible particles of water, that is, betwixt the hydrogen and oxygen, of which water is compounded, so as to place them beyond the sphere of their chemical attraction for one another; and each ultimate particle of hydrogen and of oxygen uniting with a determinate quantity of fire, new compound ultimate particles, consisting of hydrogen and caloric, and of oxygen and caloric, that is, hydrogen gaz and oxygen gaz, are compounded. This mode of action of electric fire on water is confirmed by the effect of electric fire and common fire, of a due degree of density, on oxide of quicksilver, which is by them resolved into oxygen gaz, and quicksilver in the vapour state. All calculation must needs be extremely vague; yet, perhaps, some elucidation will be obtained by considering that as it probably requires 70 or 80 thousand discharges to produce a cubic inch of gaz from the supposed decomposed water, the gaz produced by each discharge cannot amount to one 200,000th of a grain weight. The quantity of condensed fire at the extremity of the wire must be immensely great, comparatively with the quantity of it which enters into the composition of the gazes from decomposed water; otherwise it is not easy to conceive how even the minutest particle of water could be decomposed, the electric fire being in contact with a large body of water, and passing through it with a velocity which is incalculable.

The reason of the metal wire not uniting to the oxygen of the decomposed water, as in the experiment of passing water through red-hot iron tubes, might be assigned from the intensity of the fire only; but it is also on account of the rapid motion of the discharge, as well as, partly, from the great quantity of light. In a very low temperature, light decomposes oxide of silver, and of several other metals; also oxy-muriatic acid, nitric acid, &c. Hence light both decomposes bodies, and prevents oxygen from coming within the sphere of chemical attraction of the metal.

It is supposed that but a very minute proportion indeed, of the electric discharge, is consumed at the end of the wire in the composition of gazes, during a momentary interruption, as above said; for it diffuses itself through the water, between the wires, yielding a volume of vivid light, till it arrives at the extremity of the under wire or point of the convexity

convexity of the metal cup, where it is again condensed by the superior conducting power of the metal to that of water, and where, if it be in due quantity, and of sufficient density, it manifests itself by a spark, and infallibly again decomposes water: hence bubbles are seen to rise from the end of the lower wire, and from the metal cup, as well as from the point of the upper wire.

From these interpretations it will not be difficult to explain the reason of a spark appearing in some cases at the points of both the upper and under wire; why in other cases it appears at the point of the upper wire only; why in others it appears at the point of the lower wire only; namely, according to the density of the fire of the discharge there accumulated.

Concerning the agency of electrical fire in causing the hydrogen and oxygen gas of the supposed decomposed water to undergo combustion and produce water, it is well known that the smallest visible spark, or particle of flame, or fire, can kindle as rapidly a very large quantity of hydrogen and oxygen gas, as the greatest quantity of flame, sparks, or fire, can kindle the smallest quantity of these two gases; while, on the other hand, the largest mass of matter, heated most intensely, but short of ignition, cannot produce combustion of oxygen and hydrogen gas.

Although, as hath been above explained, caloric and light, in a sufficiently dense state, may decompose every compound substance in nature; it is also well ascertained, that caloric, in certain states, universally promotes chemical combinations. The mode of agency, in this latter case, I apprehend to be in a different way from that commonly accepted; for I think it is unnecessary to suppose that it operates by increasing the power of chemical attraction, and I conceive its agency to be merely diminishing or destroying the powers which counteract chemical union; especially diminishing cohesive attraction, and exciting motion among the particles of the different substances: hence these substances are applied to one another, within their spheres of chemical attraction; or the chemical attraction acts between a greater number of points of the different bodies, as when caloric renders solids and inelastic fluids into the elastic fluid state.

Accordingly, when an electric spark, or the smallest particle of flame, or of an ignited substance, is applied to the gas produced in the above process, or to the mixture of hydrogen and oxygen gas, the ultimate particles of these gases nearest to the flame are driven from it in all directions, as from a centre, by the interposition of fire, or of caloric and light; so that they are brought within the sphere of their chemical attraction for the ultimate particles of the gases at a certain distance from the centre of application of fire; which therefore unite, and the caloric and light, disengaged by that union, act in a similar manner in producing union among the next set in order of proximity, of the ultimate particles of the gases; the disengaged caloric and light of which act in producing union of the next set of ultimate particles in order; and so on successively, but with incalculable velocity, the greatest bulk of ultimate particles of the two gases unite with one another; the known products of which union are fire and water; or light, caloric, and water. According to this hypothesis, if caloric or fire, or merely caloric of sufficient intensity and quantity, be applied to a given bulk of hydrogen and oxygen gas, no combustion should be produced; as the caloric will be interposed in such quantity that all the ultimate particles must be at the same instant driven from one another in all directions, so that they are beyond their spheres

spheres of chemical attraction for one another: and in various instances common experience verifies this hypothesis.

If light be considered as a different species of substance from caloric, then the theory of its agency, lately published by Dr. Parr, M. D. may be applied very happily to explain the explosion from the combustion of oxygen with hydrogen gas. From a very large induction of facts, Dr. Parr infers that, although light and caloric subsist together very commonly in the same compound substance, they simultaneously repel each other on the decomposition of the substances with which they were united; and from this repulsion, and also the repulsion of oxygen and light of one another, he chiefly accounts for the combustion of oxygen gas.

On my principles above stated, we can explain why quicksilver of the temperature of 1000° , or more, of Fahrenheit's scale, cannot unite to the oxygen of oxygen gas; but why at this temperature caloric separates oxygen in the gas state from oxide of quicksilver: and why at the temperature of between 600 and 1000° quicksilver does unite to the oxygen of oxygen gas; but at which temperature oxygen gas is not separable from the oxide of this metal: and again, why at below 600° oxygen of oxygen gas can neither unite to quicksilver, nor be separated from its oxide.

These principles may be applied variously to the interpretation of the phenomena of combustion, and other cases of chemical combination, according to the state of aggregation of the substances which have a chemical attraction for one another. For example; the oxygen of oxygen gas cannot unite to the constituent substances of a wax or tallow candle, in a low temperature; because the cohesive attraction of the wax or tallow, as well as the chemical attraction between their constituent substances—hydrogen and carbon—counteract the chemical attraction between the oxygen of oxygen gas, and the ultimate particles of hydrogen and carbon of the candle. But at a pretty elevated temperature, when the wax and tallow are in the vapour state, the cohesive attraction no longer subsisting among the ultimate particles of these substances, and the motion excited by the ignited portion of wax or tallow bringing the ultimate particles of these substances within the sphere of chemical attraction of the particles of oxygen gas, these latter unite first to the carbon, by virtue of the stronger attraction between the oxygen and carbon, than that between the oxygen and hydrogen; but, in the instant of this disengagement of the carbon from the hydrogen, a portion of this hydrogen unites also to oxygen, and thus not only carbonic acid gas but water is produced: hence the *blue flame*, as appears on other occasions, in which there is combustion of hydro-carbonate gas. The remainder of the hydrogen of the decomposed wax or tallow, ascending in the gas state, it unites to the oxygen of oxygen gas; and, as in other cases of combustion of oxygen gas with hydrogen, a *white or straw coloured flame* is produced. The wick answers the purpose, by means of capillary attraction, of applying the wax and tallow in such quantities as can be decomposed and combine with the oxygen of atmospherical air; hence the combustion is gradual and equal. The wick itself contains hydrogen and carbon; hence, in combining with oxygen of oxygen gas, it also produces a *blue flame*. Hydro-carbonate gas being specifically heavier than hydrogen gas, is another reason for the blue flame appearing distinct from the white, and at the inferior part of the *frustum* of a cone of flame from a burning candle. It is scarcely necessary to say that hydrogen and carbon are constituent substances of wax and tallow;

and

and that when they combine with oxygen, as above explained, the products are water and hydro-carbonate gaz.

If I did not use the term *demonstration* in a more strict and precise sense than is usual, except in mathematics, I would venture to affirm that this theory is almost demonstrated by the agency of fire and carbon on black oxide of manganese. If colourless vitrifiable matter, and that oxide be melted together, by means of the *white* part of the flame of a candle, purple-coloured glass will be produced; for then the hydrogen of this part of the flame carries off little or none of the oxygen from the oxide: but if these substances be melted together by the *blue* part of the flame, colourless glass will be produced; for then the carbon of this part of the flame carries off oxygen from the black oxide, and produces white oxide. By combining oxygen anew with colourless glass, containing manganese, it will become purple; and this is effected by melting such colourless glass with a little nitrate of pot-ash, or by melting it in open vessels by the yellow flame of a candle. By separating oxygen from glass rendered purple by manganese, colourless glass is produced; and this is effected by melting such coloured glass with a little carbon, or by the blue flame of a candle. I have not thought it necessary to distinguish between the indigo and violet rays; or to notice the extremely thin film of violet flame, which an attentive observer may perceive surrounding the inferior part of the white, extending as high but scarcely higher than the wick; because the explanation is perfectly obvious from the preceding distinction of the two kinds of gazes afforded by the candle. If I have explained more satisfactorily than former members of this society the above phenomena of combustion, I owe this advantage to some experiments of combustion of inflammable gazes which I have made for some years past, in my chemical lectures; by which the colours are shewn to be very different, and correspond to the above theory. I apprehend the theory of an ingenious member of this society cannot explain adequately the phenomena, and does not appear supported by any facts: for there is no evidence that the white flame is not equally the effect of immediate decomposition as well as the blue; or that the blue becomes white flame by ignition; and I presume that the experiments which I have mentioned shew that these differences depend upon different decomposing substances contained in the candle.

With regard to the evidence afforded by the foregoing experiments concerning the composition of water and of hydrogen and oxygen gaz. These substances are now accounted for in two ways only; namely, 1. By saying that these two gazes consist of water and imponderable matter; and that during combustion the water is precipitated. 2. By saying that the two gazes consist of a peculiar basis, one of which is named oxygen and the other is hydrogen, each of which is rendered into the gaz state by uniting to caloric, and perhaps also to light; and that during combustion these bases unite with one another, thus compounding water and discharging caloric and light. If complete demonstration could be given, there would not be two opinions; for its proofs, if understood, command universal assent: but the case being otherwise, that opinion must be adopted on the side of which the evidence preponderates according to the laws of reasoning in physical science. Now with regard to the former of these opinions, I can perceive but two facts in support of it. The first of these is supposed to afford a sort of *synthetical* proof: it is the instance of water being required to obtain, by fire, the whole of the carbonic acid from carbonate of baryt. Here the fact, if admitted, is true only of carbonic acid; but even in this case it has not been

been shewn that the water enters into the composition of carbonic acid; and until that is proved, it is warrantable to suppose that the water serves to disengage the carbonic acid, by attracting and uniting with the baryt; especially as this acid gaz may be wholly separated from other substances without the intermediation of water, and as it can be compounded by uniting the driest carbon to the driest oxygen gaz. Further, although carbonic acid gaz be very apt to contain water, it may be obtained in so dry a state as not to indicate moisture to the most delicate tests.

The next fact affords a sort of analytical proof; which is the existence of water in air or gaz in general, as shewn by muriate of lime, acetite of pot-ash, sulphuric acid, quicklime, pot-ash, &c. But the absorption of water by these substances only shews that it may be suspended or dissolved in gazes; but not that it enters into their composition. Gaz may be obtained or rendered quite free from water, as just said; and as the compounds by the combination of the gazes with various different substances are quite different from the compounds by the combination of water with the same various different substances, there does not appear to be in this case any admissible evidence of water being an essential constituent substance in the composition of gazes.

The experiments of separating oxygen and nitrogen gaz from water by boiling, and by exposing it in vacuo and applying caloric, only shew how much more tenaciously, and in how much greater quantity, these gazes are contained in water than is commonly supposed; for however long a given quantity of water yielded gaz, so that there seemed no limit to the quantity, the water which afforded it was still very considerably greater than the weight or quantity of gaz.

With respect to the other opinion, that hydrogen and oxygen gaz are compounded of a peculiar basis and caloric, and that water is compounded of these two bases—1. It has been shewn by an experiment, the accuracy of which is not questionable, that when these two gazes produce water by combustion, the amount of the weight of the water is exactly the weight of the gazes consumed, and that no ponderable matter but water is produced when nothing but the bases of these two gazes unite; but that when other substances, such as nitrous acid and carbonic acid, were produced, as well as water, then nitrogen and carbon were present. 2. It has been shewn, that when oxygen in a concrete state, as in metallic oxides, unites to the hydrogen of hydrogen gaz, the water thus compounded is equal in weight to that of the hydrogen gaz consumed, and the deficient weight of the oxide. 3. It appears that when hydrogen in a concrete state, as in alcohol, is united to the oxygen of oxygen gaz, the water compounded is equal in weight to the weight of the alcohol and oxygen gaz consumed, provided the addition be made of the carbonic acid also produced by the carbon of the alcohol. The carbon of alcohol can be shewn by analysis; and its existence in carbonic acid can be shewn both by analysis and synthesis. 4. When water is applied to certain substances, nothing but hydrogen gaz is obtained, and new compounds, the constituent parts of which are oxygen and the substance applied; for these new compounds have the same properties as the compounds of the same substances so applied to water, and of the oxygen of oxygen gaz. And in these cases the additional weight derived from the water, together with the weight of the hydrogen gaz separated from the water, is equal to the water destroyed. 5. It has been rendered at least very probable, that when oxygen and hydrogen are rendered into the gaz state, they absorb or unite with a

large quantity of caloric, or of both caloric and light. I might add, that besides the positive evidence just stated, there does not appear to be any evidence to contravene the conclusion that water is compounded of hydrogen and oxygen; and that these gases are compounded of a peculiar basis, called hydrogen and oxygen, united to caloric: for it would be easy to shew, but it would be digressing too far, that the remaining partizans of the phlogistic sect have advanced erroneous evidence in some cases, and in others have neglected the consideration of opposing but well authenticated evidence.

In the experiments contained in this paper no support can be found for the former opinion concerning water, and hydrogen and oxygen gas; but they confirm the latter opinion. For, 1. the combustion of these gases, rendered perfectly dry, afforded water, p. 303, Exp. IV. 2. The evidence from the process above described is peculiar; for no *one* other process affords oxygen and hydrogen gas from water. In all other processes for decomposing water, either the decomposing concrete substance receives something from the water applied, and at the same time hydrogen gas is produced; as in the instance of passing water through a red-hot iron tube: or the decomposing concrete substance receives something from the water; and this compound unites to that in water, which produces hydrogen gas with caloric; as in the instance of applying water to red-hot carbon: or two decomposing substances being applied, each of them receives a different thing from the water, namely, hydrogen and oxygen. Hence, by the process above described, the objection is removed, that the hydrogen gas might be separated from the decomposing substance itself, by water taking its place. 3. The production of hydrogen and oxygen gas, and the production of water by the combustion of them, in the above process, afford an additional evidence of the decomposition and composition of water, and of hydrogen and oxygen gas; as the mode of their production by this process is perfectly consistent with the rationale of their production in all other cases.

Thus it appears that the grandest discovery ever made in chemical philosophy, by an illustrious member of this Society, in 1781, has been confirmed by a number of subsequent experiments. The body of evidence is indeed so numerous, and of such a nature, that, in the minds of those who understand its import, and who rely on the accuracy of the weights and measures employed, it produces as much conviction concerning the composition of water as can be obtained by the evidence of almost any other case of composition. I must, however, beg leave to protest against those able philosophers, who have maintained, that the composition of water, and several gases, has received full and complete demonstration; by such unwarrantable pretensions their adversaries have obtained over them some advantages. For in the chain of causes and effects there are some links which cannot be explained by the direct evidence of sense; and there, in so far as we admit hypotheses, although consistent with the phenomena, we may be said to quit day-light, notwithstanding we at the next step emerge into light derived from the perceptions of sense. For instance, if I combine a certain known weight of hydrogen and oxygen by combustion of their gases, and produce water equal in weight to that of these gases; and if I again resolve this water, by means of the electric fire, or red-hot iron, into the original quantity of hydrogen gas and oxygen, I cannot give the full and complete demonstration of the composition of water and these gases: for, as I proceed in the interpretation, I at length come to demonstrate the mode of agency of the particles of the hydrogen and oxygen gas on one another
when

when they produce water, caloric, and light. But here I must call in the aid of the imagination: accordingly I imagine that the gazes consist of hydrogen and oxygen, which are ponderable—united to caloric, and perhaps light, which are imponderable; that these ponderable particles unite with one another, and their caloric and light are set at liberty. Now here I have not any evidence of sense; for I cannot perceive, by the senses, the existence of the composition of the gazes just stated, nor of their decomposition, and union of their ponderable parts. This being the case, other kinds of imponderable matter may unite, or escape, besides caloric and light; consequently I cannot give the full and complete demonstration in these instances. But the same objections may be made to the pretensions to demonstrate fully and completely the composition of sulphate of soda, sulphate of pot-ash, or any double salt, perhaps, whatever; for caloric is separated, and possibly other imponderable matter, when these substances are compounded; and caloric, and perhaps other matter, may unite when the sulphuric acid and alkali are disunited.

Hence chemistry, in its present state, ought not to pretend to vie with mathematical philosophy in its demonstrations. But it does not appear improbable, that the same certainty as in mathematics may hereafter be attained in chemistry. We are encouraged to entertain this hope, from finding that the art of observation, and the invention of artifices for rendering the properties of matter evident to the senses, have been proportionate to the advances of knowledge of facts; as was predicted by Chancellor Bacon.

IV.

Observations on the Electrophore, tending to explain the Means by which the Torpedo and other Fish communicate the Electric Shock.

THOSE who are conversant with electrical experiments know that the electrophore consists of a flat metallic plate with an insulating handle, and another separate plate of non-conducting matter, either varnished glass, or some resinous substance, coated with metal beneath, and placed with its uncoated side uppermost. When this uncoated face is electrified by friction or otherwise, and the plate of metal placed upon it, this last is found to give a small spark to the finger, which is of the same nature as the electricity of the non-conducting surface; but when the plate is lifted by its insulating handle, it will emit a spark to the finger which is much stronger, and of the opposite kind to that of the non-conducting plate. This phenomenon, which was at first considered as very difficult to be explained, was afterwards found to be produced throughout by the same energies, whatever they may be, which govern the electric charges. For, as I have elsewhere observed*, the charge consists of two electricities, usually called plus and minus, which compensate each other; and also of a portion of electricity on the insulated side, which in equal charges is greater, the greater the distance between the two electrified surfaces, and in unequal moderate charges is nearly in proportion to the charge itself. When the non-conducting surface of the electrophore is rubbed, it acquires both these electricities, namely, the charge by virtue of the compensating power of the uninsulated coating beneath, and also the portion of simple electricity requisite to maintain the charge. If the metallic plate be then

* Philosophical Transactions, 1789.

placed upon it, and touched by the finger, a portion of the electricity may really pass to the metal at the few places of actual contact; and another part, if the intensity be strong, may strike through the thin plate of air interspersed between the metal and the resinous face. What remains on the resinous surface will lose by far the greatest part of its intensity, in consequence of its becoming a charge to that small plate of air, while the metallic plate, possessing the opposite part of the charge, will have its intensity equally low. During these charges, the spark which passes from the metal must therefore be of the same character as that of the resinous plate; in the same manner as the outer coating of the jar emits positive sparks during its transition to the negative state. The commencing intensity of this spark cannot exceed that which belonged to the charge between the resinous surface and the lower coating; but as the intensity requisite to maintain a charge on the thin plate of air is extremely minute, by far the greater part of the electricity will be employed in constituting that charge, and the spark given off by the plate will be much shorter and more gradual than if the same quantity had passed at once from the plate in a state of simple electrization; that is to say, the spark will be apparently very small. But when the plate is raised up by its insulating handle, part of the charge on the resinous surface will begin to acquire its former state with regard to the lower coating; and that which continues to be compensated by the upper moveable plate, will require a greater portion of uncompensated electricity to maintain it. The intensity of the plate will rapidly increase as it rises. It will throw out sparks and ramifications to the lower plate, and to the surrounding bodies; and at a certain distance the whole remaining portion of its former charge may be considered as simple electricity, which will strike the finger at a greater distance, and with more suddenness and brilliancy. This spark, though in fact consisting of less electricity than the former, will nevertheless be more perceptible both to the eye and the ear, and consequently will be thought larger: the first is the gradual explosion of a charge; the second consists of the sudden escape of a portion of simple electricity.

If we suppose the non-conducting matter of the electrophore to be extremely thin, and the spark when in contact to be made to pass from the upper to the lower metallic plate, the effect will be nearly the same as the transition of the shock from such a coated electric with a charge of the quantity of electricity which passes; and when the moveable plate is raised, the effect will be similar to that of a spark from the prime conductor. Or rather, perhaps, by a comparison familiar to electricians, the first may be considered as the shock from a large battery charged by only one turn of the handle of the machine, and the latter as the shock from a small jar capable of being charged high by the very same quantity of electric matter. It is certain that the shock, as well as the spark from the battery, would in these circumstances be inconsiderable, though the effects of the jar might be very striking.

When the electrophore is thus compared with the jar, its charge will be found to be wonderfully small, as it consists of no more electricity than, when uncompensated, would pass off in the simple spark which was obtained.

I found that two square inches of Muscovy tale, about one-hundredth part of an inch thick, which is an exceedingly good non-conductor, required, when coated with tinfoil, one turn of a small cylinder to discharge through one-tenth of an inch; and one turn of the same cylinder charged a simple conductor of about six square feet surface, so as to give a spark about nine inches long. Now if we assume the quantities of electricity in

conductors to be as the length of the spark, which, though a doubtful position in great intensities, may serve the present occasion, and that a talc electrophore of two square inches surface on each side have the intensity to give a spark of one-tenth of an inch, its whole electricity will be expressed by $4 \times 0,1 = 0,4$. But the whole electricity of the conductor which gave the nine inch spark will be expressed by $6 \times 144 \times 9 = 7776$. And this number doubled, or 15552, for a double electrophore, also expresses such a charge of the talc as gives a spark of explosion or discharge of 0,1 inch. The length of the explosive spark, on applying such a simple electrophore to its plate, will therefore be shorter than 0,1 in the proportion of 0,4 to 15552, or 1 to 38880; the spark would therefore be the ,000002 of an inch long. And as Mr. Cavendish has found that electric shocks nearly equal are produced when the quantities of electricity are inversely as the lengths of the spark of discharge, and also that the quantities of electricity in charges are as the surfaces, it will follow, that the shock of two square inches of the talc with a spark of 0,1 inch, will be equal to the shock of 77760 square inches with a spark of 0,000002 inches. This surface is equivalent to an electrophore of 279 inches square, or measuring 23 feet by the side; or 19440 of the double electrophores of two inches surface.

To elucidate this, I constructed a small electrophore adapted to perform the experiments which Beccaria called vindicating electricity. It consisted of two metallic plates with insulating handles. Each was a square whose side measured two inches, and the face of each was coated with the thin talc. When the two uncovered surfaces of the talc were applied together, and the whole charged as one plate, the discharge gave a dense shock of considerable severity. The sparks, on separation, were about one-eighth of an inch long, and very weak. No perceptible spark was exhibited on bringing them together. The operation appeared capable of being repeated almost incessantly, as in the electrophore.

It may perhaps be simpler to consider the electrophore as a compound jar or plate variable in its thickness. When the plates of the instrument just described are together, the two electricities on the contiguous surface compensate each other, and the external coatings may be supposed nearly in the natural state. But when they are separated, the portion of compensated electricity will be less, and the intensities would rise if it were not for the external coatings, which in a great measure prevent it; each plate becoming charged like a simple jar, and the equilibrium being possible to be restored by communication between the external coatings. This second charge, intensity, and explosion will be greater the further the plates are removed. It appears, therefore, that in a deduction of the manner in which electric fish may communicate the shock, we may safely avoid the more complicated consideration of the electrophore, and compare the action immediately to that of a simple jar.

Mr. Hunter, in the 63d volume of the Philosophical Transactions, page 434, describes the electric organ of the torpedo to consist of a number of columns varying in their length from an inch and an half to a quarter of an inch, and their diameters about 2-10ths of an inch. The number of columns in each organ of the torpedo he presented to the Royal Society was about 470; but in a very large torpedo the number of columns in one organ was 1182. These columns were composed of films parallel to the base of each, and the distance between each partition of the columns was 1-150th of an inch. If we suppose these films to be charged with electricity, and to be 1-300th of an inch thick; and a middling size torpedo to contain in both organs, on the whole 1000 columns of an inch long, and 0,03 square

square inches area at the base; then $1000 \times 150 \times 0.03 = 4500$ square inches. Now I found that Muscovy talc, of 0.01 inch thick, has twelve times the capacity of the glass of a jar of 421 square inches, which, from former experience, I know to be as thin as such jars can be without danger of breaking by explosion; and the torpedinal membrane, being less than one-third the thickness of the talc, will have three times the capacity; that is to say, its capacity will be thirty-six times that of stout glass; or both organs will be equivalent to $4500 \times 36 = 162000$ square inches, or 1125 square feet.

My large jar, with Lane's electrometer, measuring a spark about 1-200th of an inch long, gave very sensible and rather unpleasant shocks across the hand, and also the tremulous sensation caused as I suppose by the imperfect conducting power of the skin with so low an intensity. When the spark was 1-100th of an inch long, the shock was strong enough to convulse the hand, and at less than 1-50th it was painfully strong. This last was probably stronger than the shock of the torpedo. If we therefore avail ourselves of Mr. Cavendish's deduction, that, the quantity of electricity being increased in proportion as the length of spark is diminished, the shock will be rather greater than before, we may compute the length of the spark in the torpedinal shock of the magnitude last mentioned. For the organs of the torpedo, compared as to their capacity with that of the jar, will be equivalent to $\frac{162000}{421} = 375$ times as great, and they will give such a shock when charged as to afford a spark

$\frac{1}{375}$ of $\frac{1}{50}$ of an inch = $\frac{1}{18750}$ inch. No wonder, therefore, that the shock will not pass through an interrupted circuit, and that the spark is not exhibited.

Respecting the manner of operation, there are no facts which shew how this charge is actually produced, maintained, and communicated. Whether electricity be actually collected, composed, or decomposed in the organs of the fish, or whether it simply exists in those organs, as perhaps it may in all bodies, in the state of what is called compensation, are questions concerning which we in fact know nothing. It has appeared to me, from the observation of the high electric state which talc naturally possesses, and from the innumerable shocks the electrophore is capable of giving by mere change of arrangement, that a machine might be constructed also capable of giving numberless shocks at pleasure, and of retaining its power for months, years, or to an extent of time of which the limits can be determined only by experiment. I will not here describe the mechanical combinations which have occurred to me in meditating on this subject, but shall simply shew that the dimensions of the organs of the torpedo are such as by certain very possible motions, and the allowable supposition of conducting and non-conducting powers, may produce the effects we observe. How far it may be probable must undoubtedly be left to future experimental research.

In new talc, which had never been excited nor electrified, and exhibited no signs of electricity when applied to Bennet's electrometer, I found that the laminæ were naturally in strong, opposite, electric states, counterbalancing each other. When they were torn asunder in the dark, they gave flashes at least 1-10th of an inch long to each other. This is 1875 times the intensity of the torpedinal electricity, as before deduced. If, therefore, one or more columns of talc, or other thin electric plates 1-300th of an inch thick, and making up the surface of the electric organs of the torpedo, were so constructed as that the plates might touch each other by pairs only, naturally in opposite states, and coated on the out-

side;

tide; if, moreover, there were one common conductor communicating with the upper plate of every pair, and another in the same manner with the lower;—then a separation of all the pairs to the distance of only $\frac{1}{10000}$ inch would produce the torpedinal intensity; the equilibrium would be restored by the two conductors if made to communicate, and whatever living creature was in the circuit would receive a shock: and on restoring the original situation of the apparatus, a shock might also be given. The force of these shocks would differ according to the quantity of the apparatus made use of at the time, or the distance to which the plates were drawn asunder. If different columns were exploded in rapid succession, the quick repetition of small shocks would produce the tremulous sensation.

If we were to conjecture that the torpedo actually operates like a machine of this kind, we should find our supposition to include the following subordinate parts:—1. The membranes may be non-conductors, and the fluid between them a conductor. 2. They may act as electrophores. 3. The white reticular matter between the columns may consist of conductors separately leading to the two opposite surfaces. 6. These separate conductors, in all their subdivisions, may be well kept asunder by a covering of non-electric matter. If this be of the same kind as the membranes, and $\frac{1}{10000}$ th of an inch thick, it would be sufficient for the purpose, because the intensity of the electric state is deduced from its power of breaking through a much more permeable electric, namely, air, at nearly twice that interval. 7. The effects may also be produced by the motion of conducting plates in a non-conducting fluid.

V.

A Letter from Mr. VON HUMBOLDT to M. H. VAN MONS on the Chemical Process of Vitality; together with the Extract of a Letter from Citizen FOURCROY to Citizen VAN MONS on the same Subject.*

I HAVE lately addressed several letters to Messrs. Dolomieu and Fourcroy at Paris, and perceive by those I have received from the former that mine have miscarried. Permit me, Sir, to address myself to you. By your means I may perhaps succeed in forwarding to Paris some explanations respecting facts which, as I understand, employ part of the time of the National Institute. Be pleased to accept my assurances of the great respect which your zeal and your chemical discoveries have inspired me with. The natural philosophers of Europe ought to form a single family. They are in pursuit of the same interesting objects; and this is a sufficient motive to produce that useful degree of intimacy which is calculated to promote their researches.

You are probably acquainted with my Essays on the Vegetable Philosophy, such as my *Aphorismi ex doctrinâ physiologie chimicæ plantarum*, annexed to my *Flora subterranea Fribergensis*, and several memoirs which I have presented to the National Institute.—The memoir on the action of oxygenated muriatic acid upon the vegetable and animal fibre, which is printed in the *Magazin Encyclopédique* of Millin, Noël, and Wares, seems to have

* *Annales de Chimie*, XXII. 64.

had more success. I am happy to hear that Messrs. Vauquelin and my friend Dolomieu have begun to repeat my experiments. As the memoir which was read to the National Institute related principally to the germination of vegetables, I have thought it my duty to announce to you certain facts more striking respecting the animal fibre. The strongest stimulus of the nervous fibre is that of the alkalis. It appears that these salts affect the irritable and sensible system by means of their azote. Let the thigh of a frog be thrown into the oxygenated muriatic acid, or the nitric acid, and it will remain motionless. Let it be put into a solution of pot-ash, or of soda, and it will undergo contractions no less strong than when irritated by the metals. These motions always commence at the lower extremities. The toes move first, afterwards the *musculus gastrocnemius*, and then the thigh. If the nerve be very sensible (for nothing more is required than simply to immerse the extremity of the crural nerve in the oleum tartari per deliquium), the contractions will end in an universal tension or rigidity. The leg rises up perpendicularly, the membrane of the feet extends itself, and the tetanus appears. In this situation all the irritability of the fibre appears to be extinguished; and if an electric stroke be passed through the limb, the exhaustion becomes real. It is a striking phenomenon to see the last remaining signs of tetanus disappear in an instant. But there is another method by which the tension disappears, and by which I am able to restore the irritability to the organs. It seems that the acidifiable bases of the alkali, principally the azote, have consumed all the oxygene contained in the fibre. The chemical process of vitality ceases. If I pour an acid, for example the nitric acid, upon the nerve, an effervescence will take place; part of the alkali becomes latent, and the rest will have a proper proportion with respect to its oxygene. From this moment the contraction with zinc and silver is again produced. Increase the quantity of acid, and the movements are again weakened. In this manner it is, that by forming an equilibrium between the azote of the alkali and the oxygene of the acid applied to the animal fibre, the irritability of the organs may be taken away or restored three or four times in succession. You may easily perceive, Sir, that these experiments require steady attention. The degree of insensibility to which the nerve is reduced by repeating them may be very different. It is possible to determine exactly the quality of the chemical agents, their weight and temperature; notwithstanding which, many experiments do not succeed. The reason is, that there are conditions which depend on the individuality of the organization, and concerning which we must still confess our total ignorance. The influences of the oxygenated muriatic acid upon the animal fibre are less marked than those of the alkalis; but they are nevertheless of much importance. I steeped the feet of a frog (I mention this animal by preference, though I have made the same experiments on other species) in a solution of opium in alcohol. The metals, or galvanism, excited no motion. I threw one leg into pure water, and the other into the oxygenated muriatic acid; the first remained motionless, the second gave very strong contractions, and shewed that its irritability was restored. The common acids depress the irritability of the nervous fibre. A crural nerve, rendered insensible by the ordinary muriatic acid, remains so though it has been steeped in the solution of pot-ash: but the mineral acids exhaust the forces of the muscles, by condensing the elements of the muscular fibre. These acids act in the same manner as cold, which depresses the nerves, and is beneficial to the muscles. The muscles and the nerves have specific stimuli, agreeable to the diversity of the elements. The terrible action which the alkalis exercise on the

nerves

nerves appears to explain the effect of the secretion of the feminal liquor on the blood. It is this alkali which, distributed throughout the system, answers the purpose of a stimulus beneficial to the animal fibre. By this action I account for the ferocity of the ichthyophagi.

My eldest brother, who is very skilful in the study of anatomy, applied zinc and silver to the mouth and the brain of a dead fish; it afforded no motion. I poured oxygenated muriatic acid on the nerves, and at that instant the contractions became very strong. Mr. Herz and several learned men of Berlin were present at these and many other experiments. The heart of the same fish, which had entirely ceased to palpitate, began to perform this movement with regularity when I threw it into the oxygenated muriatic acid. The same experiment succeeded very often with the hearts of frogs: when a heart is immersed in a solution of pot-ash, it loses its irritability for ever; so that azote is not the specific stimulus of the heart.

Mr. Pfaff, while employed in my experiments respecting germination in the oxygenated muriatic acid, has discovered that frogs suffocated in the oxygenated muriatic acid gas exhibit a very high degree of irritability after their death. I beg you will fix the attention of Mr. Vauquelin on the action of sulphate of pot-ash upon the nerves. I have been astonished at every thing I beheld. Two legs of frogs in a very lively state were steeped in the solution of the sulphate of pot-ash. I tried them three or four minutes afterwards with the metals. The contractions had increased in force, and were even convulsive. It appeared that the three acidifiable bases contained in the solution hydrogen, azote, and sulphur, acted strongly on the oxygen conveyed by the arterial blood. This action revives the process of vitality. After fourteen or sixteen minutes the whole thigh became of a blackish brown. All the oxygen of the blood was absorbed, and the carburet of hydrogen appeared in a disengaged state. The zinc and the silver are not then capable of exciting the smallest motion.

Yet it would be a great mistake to conclude that all irritability is exhausted in this case. I have seen the contractions re-appear several times on restoring oxygen to the fibre by means of a solution of the oxide of arsenic. The flame is thus renewed which seemed ready to expire. The oxide of arsenic produces a tetanus and perfect insensibility if the nerve remains long immersed. It seems then that the too great quantity of oxygen absorbs as it were the acidifiable bases which support the chemical process of vitality. I have thrown the whole thigh into the solution of pot-ash, and I observed that galvanism afterwards had the power of exciting motion.

You see, Sir, what an immense number of experiments remain to be made on these objects of vital chemistry. It is enough that a method has been pointed out of measuring the degree of irritability of the organic parts by means of galvanism. I shall have the honour to send you my work on the nervous and muscular fibre, and on the chemical process of vitality. I collect facts, and mistrust my own hypothetical ideas. You will perceive with me how mistaken the notion is that oxygen performs the principal part in this process. My experiments prove that the irritability or tone of the fibre depends only on the mutual equilibrium between all the elements of the fibre, azote, hydrogen, carbone, oxygen, sulphur, phosphorus, &c. The chemical combinations of phosphorus and of azote, for example, appear to be in no respect less important than those of oxygen with the acidifiable bases. How much light may we not expect from the advances of yourself, Fourcroy, and Vauquelin on these objects!

VON HUMBOLDT.

ADDITION to the foregoing LETTER.

HAVING preserved some frogs for the winter, I have this morning repeated some experiments, of which I venture to send you an account. In the preceding letter I have remarked that, as we are only superficially acquainted with the principles of vital chemistry, we ought not to be surpris'd if we do not always obtain the same results. A negative experiment proves nothing against another of an affirmative nature.

I am very sure that a nerve rendered insensible by alcohol will not recover its irritability by sulphate of pot-ash. But it may very well happen that a thigh, of which the tetanus has been caused by the oxide of arsenic, should remain in a state of tension notwithstanding the action of the solution of pot-ash.

I have seen the following facts within this quarter of an hour. I took the four extremities of a very lively frog. The right arm and the right leg leaped on zinc and silver. I steeped them for four minutes in alcohol. The hydrogen acted strongly on the fibre. The toes of the foot trembled during the first minute. Soon afterwards a total rigidity came on; the muscle became white, the blood having apparently lost its oxygen. I replaced the arm and the leg on the zinc and silver, but there was not the slightest contraction. I then quickly threw them into the oxygenated muriatic acid, which I had shaken strongly before it was poured out; the limbs remained in it for three minutes. A slight tremulous motion shewed, even in the cup, that the vital forces were restored. I replaced the arm and the leg on the metals; the contractions were again produced, not only with zinc and silver, but with zinc and iron.

Here I think is a very simple and decisive experiment. I then changed the method in order to observe the effect. I took the left thigh, and immersed it for nine minutes in alcohol. It lost all irritability, and the oxygenated muriatic acid was no longer capable of restoring the vital force. The left arm had remained untouched for fifteen or eighteen minutes. I prepared its nerve, but it shewed only very weak and slow contractions with zinc and silver. I threw it into alcohol. After the first minute its irritability was increased, the galvanism acted more strongly; but after three minutes all the irritability was exhausted, and I applied in vain the remedy of oxygenated muriatic acid. I steeped the arm in the solution of the oxide of arsenic, and it then afforded contractions, though very weak.

Here are four experiments, two of which succeeded, and in the two others the vital forces were not restored. I think, nevertheless, that in good logic we ought to admit the affirmative experiments. Examine the conditions, and you will see they are very different. The left leg remained too long, nearly nine minutes, in the alcohol. The right arm was already very weak when the experiment began. Who can boast of reviving the dead?—If of two chemists the one should obtain oxygen gas by heating the red oxide of mercury, while the other did not obtain it, we should always believe that the apparatus of the latter was not hermetically closed. I never saw an organ rendered insensible by alcohol which recovered its irritability by being left to itself. It necessarily follows, therefore, that in the experiments I have ventured to relate, and of which my work contains a very great number, the oxygen of the muriatic acid must have been a principal agent. The art of medicine will be infinitely benefited if we should succeed in observing the phenomena which the several elements produce in contact with the irritable fibre. It is proper to begin

with simple combinations, and afterwards proceed to combinations of two, three, and four principles.

I have sent to the National Institute a memoir on the nature of light, and its chemical combinations. Mr. Wedgwood pretends that the phosphorescence of calcined bodies is not altered in hydrogen and azotic gas. I think he did not purify these gases by means of phosphorus, as I did. I have seen luminous wood extinguished in the azotic and hydrogen gases. A small quantity of oxygen, being admitted into the vessel, causes the whole of the phosphorescence * to revive. I have also converted morilles (*Phallus Efculentus*) into a substance which resembles tallow, by means of the sulphureous acid. I have made soap of it.

Bayreuth, December 29, 1796.

VON HUMBOLDT.

Extract of a Letter from Citizen FOURCROY to Citizen VAN MONS on the Subject of that of Mr. HUMBOLDT.

I THINK Mr. Humboldt proceeds with rather too much expedition in his solutions. It is to be feared that he may find it necessary to retract. I am apprehensive that he admits too many hypotheses †: that he does not repeat each experiment sufficiently before he forms a conclusion. This in particular is much more important with regard to the philosophy of animal bodies than any other branch of science, because it is surrounded by numberless difficulties and multiplied sources of error and illusion. I fear that if certain chemists continue to advance with such rapidity, the physicians will soon have reason to exclaim against this encroachment. If applications be too suddenly made, and arbitrary suppositions accumulated, it may perhaps come to pass that this science may a second time be rejected from the healing art, as it was formerly by Stahl and Boerhaave, in consequence of the excessive abuse of hypotheses committed before their time in this respect by Tachenius, Willis, and others. Too much earnestness may be equally pernicious to chemistry as well as medicine, and impede the progress which the first is capable of producing, and ought to produce, in the second.

Messrs. Girtanner and Valli appear to me to make an ill use of their abilities and knowledge in this respect. They suffer themselves to be carried away by the ingenious notions they derive from modern chemistry.

All this, however, does not prevent my being of opinion that the experiments of Mr. Humboldt are extremely interesting, and that he ought to continue them with assiduity; but I should wish that he would vary them more, repeat each in particular more frequently, and be moderate in his conclusions. I cannot, for example, repeat to you how many new ideas and chemical explanations, very probable in themselves, have occurred to me during

* Spallanzani observed the same phenomena; and, what is more remarkable, he observed the phosphoric or shining animals ceased to emit light in the azote, hydrogen and carbonic gases, and that they emitted a light infinitely more vivid in oxygen gas than in the atmospheric air. *Chimico Esame degli Esperimenti de Goettling.* Modena, 1796.

† For example, in the preceding memoir he speaks of the azote of alkalis as if it were demonstrated that azote is one of the principles of these bodies. I first announced or suspected this eight years ago, but it is not yet proved. F.

several years past, in consequence of my researches into the animal analysis. They would be sufficient to change the aspect of physiology and medicine; but I have been careful not to publish them until well matured and proved by experience, lest I should otherwise embarrass two sciences at once. I wish to risk nothing of this kind, but proceed gently, and hope to arrive at solid conclusions in the course of time. I am earnestly desirous of seeing the work of Mr. Hildebrandt, as well as that of M. Humboldt. Notwithstanding the speed they appear to shew in their chemical explanations of vegetable and animal life, I know not why I am persuaded that they are less advanced than we in the analysis and true intimate knowledge of the materials of these two kingdoms. I very much commend their zeal, and admire their bold advances; but they cannot blame our well-grounded caution and prudent slowness. It is admirable to proceed with expedition, and make great advances in the paths of nature; but it is still better to observe well, to see clearly, and to communicate with accuracy what we observe in our progress. I am still on my journey, and confess that I am very far from having arrived at the place I am desirous of reaching.

The young men have attended with ardour my course of Animal Chemistry at the School of Medicine. Nothing can equal their wish to learn. The twenty lectures I give on this part of chemistry, which is so new, produce, as I see, a great movement in this branch of the study of nature. But I moderate their enthusiasm as much as I can. I fear lest by too much precipitation this beautiful machine should be broken in my hands. It would be much to be regretted if expectations so rich and so happy were to be dissipated in smoke; and this will not fail to happen if the edifice be built on hypotheses, or too much haste be made in construction before the materials are ready. I collect them by degrees, but they are still too scanty to risk the formation of a system. Yet, to speak plainly, I think there are few chemists who possess more facts than myself on the animal analysis: but they are not yet sufficiently connected in their relations to each other to form an entire work. If the attempt were made, it certainly would not be *Ære perennius*, &c. &c.

VI.

Concerning the Properties of the Sulphureous Acid, and its Combinations with Earthy and Alkaline Bases. By Citizens FOURCROY and VAUQUELIN.

Concluded from page 318.

Sulphite of Soda.] **T**HIS salt is white, and perfectly transparent. Its figure a four-sided prism, two of the sides being very broad, and two narrow, terminating in dihedral pyramids. Its taste is cool, and afterwards sulphureous.

Its habitudes in the fire are absolutely the same as those of the sulphite of pot-ash, excepting only that it commences its alterations by the aqueous fusion.

By exposure to the air it effloresces, and is afterwards converted into sulphat, but less speedily than the sulphite of pot-ash.

It requires four parts of water for its solution; is more abundantly soluble in hot water, and readily crystallizes by cooling.

Barytes,

Barytes, lime, and pot-ash decompose the sulphite of soda.

It is not soluble in alcohol.

Salts with bases of pot-ash, except the carbonate, do not decompose this salt; the other genera decompose it like the sulphate of pot-ash.

The metallic oxides, and their solutions in acids, have the same effects with the sulphite of soda as with that of pot-ash.

It contains per quintal—1. Soda 18,8;—2. Sulphuric acid 31,2;—3. Water 50.

Sulphite of Ammoniac.] This salt has the form of a prism of six sides, terminated by six-sided pyramids. It sometimes assumes the figure of a square tablet, the borders of which are sloped so as to form a solid of six irregular faces. Its taste is cool and penetrating, like that of the ammoniacal salts; but it leaves a sulphureous impression in the mouth.

By exposure to the air it attracts moisture, and soon afterwards passes to the state of sulphat.

It is very soluble, and requires at most its own weight of water for its solution.

Heat increases its solubility, and it crystallizes by cooling.

On the fire it is volatilized without decomposition.

Barytes, lime, pot-ash, and soda decompose it in the cold; magnesia produces the same effect by the assistance of heat.

The acids act on the sulphite of ammoniac in the same manner as on those of pot-ash and soda; but the results are different.

Charcoal does not convert it into sulphuret, because it rises too speedily by heat.

It does not decompose salts with bases of pot-ash or of soda, but it decomposes those of lime, magnesia, barytes, and alumine, with which it forms insoluble precipitates.

Its habitudes with the metallic oxides and salts are nearly the same as those of the sulphites of pot-ash and of soda, excepting that it forms with several of them triple salts, as we shall more amply explain in another memoir on the metallic sulphites.

Its component parts in the quintal are—1. Ammoniac 29,07;—2. Sulphuric acid 60,06;—Water 10,87.

Sulphite of Lime.] The form of the sulphite of lime is that of a six-sided prism, terminating in a very long pyramid. Its taste at first is scarcely perceptible; but when it has been kept for some time in the mouth, it communicates to the tongue a taste which is manifestly sulphureous.

This salt, when well neutralised, is very sparingly soluble in water; but it becomes soluble by an excess of acid. In this way it may be obtained in crystals; that is to say, by exposing its solution in the sulphureous acid to the air. The acid is dissipated, and leaves the salt in a state of purity. Barytes alone, among the earths, is capable of decomposing it. This may be ascertained by mixing a solution of that earth with a solution of the neutral sulphite of lime, when a light precipitate is formed.

Heat converts it into sulphate, by depriving it of a portion of sulphur.

The mineral acids decompose it, like the other sulphites.

The alkalis produce no change in this salt; for the alkaline sulphites are decomposed by lime.

Among the neutral salts, the alkaline carbonates, as well as the alkaline phosphates and fluates, alone decompose it.

It does not acquire the state of sulphat by contact of the air, but very slowly.

Metallic

Metallic solutions decompose it, at least for the most part.

The quintal contains—1. Lime 47;—2. Sulphuric acid 48;—3. Water 5.

Sulphite of Magnesia.] This salt is white and transparent. Its form a depressed tetrahedron; its taste is mild and earthy at first, and afterwards sulphureous.

By exposure to heat, it softens, swells up, and becomes ductile, like gum; it loses about $\frac{1}{5}$ of its weight by the deficcation.

If the heat be kept up after it has lost its water of crystallization, the sulphureous acid flies off, and the residue in the retort is magnesia, nearly pure. In this way we found that a quintal of this salt is composed of 16 parts magnesia, 39 sulphureous acid, and 45 water.

This salt is sparingly soluble in water, but very soluble in an excess of acid; and this solution, exposed to the air, crystallizes very readily, by losing its excess of acid.

It becomes opaque in the air, and changes by degrees into sulphate; but for this purpose much time is required.

The fixed alkalis, lime and barytes, decompose it completely; ammoniac decomposes it in part only, and a triple salt is formed.

The alkaline and earthy salts, except those of alumine, decompose it likewise.

The mineral acids and metallic solutions produce the same effects on this salt as on the other sulphites.

Sulphite of Barytes.] This salt does not crystallize, has no perceptible taste, and is perfectly insoluble in water. For this reason the sulphureous acid carries down a precipitate from the aqueous solution of barytes. It is not rendered soluble by an excess of acid, like the other earthy sulphites.

Heat changes it into sulphate; but it does not pass to this state by exposure to the air, but with extreme difficulty.

No earth nor alkali decomposes it.

The acids decompose it; whence the sulphureous acid does not precipitate the barytic salts, as the sulphuric acid does.

Among the neutral salts there are none but the alkaline carbonates which decompose it.

The other properties of this salt are such as are common to the sulphites.

It is composed of—1. Barytes 59;—2. Sulphureous acid 39;—3. Water 2.

Sulphite of Alumine.] It is insoluble in water, but becomes abundantly so by excess of acid. Its dissolution does not crystallize by the contact of air, and becomes converted into a softish ductile mass.

Fire disengages the sulphuric acid without alteration.

All the alkalis and earths decompose it, as do also the mineral acids.

Its component parts are—44 alumine, 32 sulphureous acid, and 24 water.

From the facts described in this memoir it is evident that the sulphites possess very different properties from those of the sulphates; and that they follow peculiar laws of solution, crystallization, affinity, and decomposition.

In fact they possess—1. A sulphureous taste, similar to that of their acid. 2. They are decomposable by fire, either by the escape of their acid without alteration, or by losing a portion of sulphur and becoming converted into sulphates. 3. They are converted into sulphates by the contact of air, or of any other substance capable of affording oxygen; and their weight is increased by this conversion. 4. They are decomposed by most acids; which expel

the

the sulphureous acid with effervescence, and the production of a strong penetrating odour. 5. They burn rapidly, and with flame, when heated with super-oxygenated muriate of potash, or with saltpetre, and become sulphates. 6. The alkaline sulphites are more soluble than the sulphates, and the earthy sulphites are much less so. 7. Lastly, the sulphite of lime is not decomposed by the alkalis, like the sulphate.

VII.

An Account of the Great Copper Works in the Isle of Anglesey. By Mr. ARTHUR AIKIN.*

August 13, 1796.

THIS has been a most interesting and entertaining day, being spent in visiting the vast copper works connected with the Parys Mountain. We breakfasted at Amlwch, a considerable town on the coast, about two miles from the mine, and almost entirely peopled by the miners and their families.

We had no difficulty in distinguishing this celebrated mountain, for it is perfectly barren from the summit to the plain below; not a single shrub, and hardly a blade of grass, being able to live in this sulphureous atmosphere.

"No grassy mantle hides the sable hills,
No flowery chaplet crowns the trickling rills;
Nor tufted moss nor leathery lichen creeps
In russet tapestry o'er the crumbling steep."

DARWIN.

The nearer we approached the scene of business, the more penetrating was the fume of the sulphur; but we had very soon too many objects of attention to regard this inconvenience. The mountain is about a mile in length, and is the property of Lord Uxbridge and the Rev. Mr. Hughes, and the fortunate discovery of the copper took place a little more than thirty years ago; thus converting a piece of ground, originally of very little value, into one of the most profitable estates in the kingdom.

The substance of the mountain being ore, the work is carried on in a very different manner from the custom of other mines: here are, comparatively, few shafts or levels, the greater part being quarried out, so as to leave a vast excavation open to the day. There are two of these quarries or mines, which are worked by two different companies: the first goes by the name of the Mona Mine, and is the sole property of Lord Uxbridge; the other, called the Parys Mine, is shared between the Earl and Mr. Hughes. The view down this steep and extensive hollow is singularly striking. The sides are chiefly of a deep yellow or dusky slate colour, streaked, however, here and there, by fine veins of blue or green shooting across the cavern, mingled with seams of greyish yellow. The bottom of the pit is by no means regular, but exhibits large and deep burrows in various parts, where a richer vein has been followed in preference to the rest. Every corner of this vast excavation resounds with the noise of pickaxes and hammers: the edges are lined with workmen drawing up the ore

* Tour through North Wales.

from below; and at short intervals is heard, from different quarters, the loud explosion of the gunpowder by which the rock is blasted, reverberated in pealing echoes from every side.

The exterior covering of the mountain is an aluminous slate; the matrix black-grey chert; the ore *copper*, chiefly

I. *The yellow sulphurated*: of which the richest contains, according to miners' computation, that is, in the proportions of the ounce troy,

Sulphur	—	—	5 dwts. (25 per cent.)
Copper	—	—	Ditto.
Refuse	—	—	10 dwts. (50 per cent.)

The worst ore yields nearly the same quantity of sulphur, but of metal no more than six grains ($1\frac{1}{2}$ per cent.); this inferior kind, however, is chiefly worked for the sulphur. The other species and varieties of ore that the mine produces are,

II. Black ore, containing copper, mixed with galena, calamine, and a little silver.

III. Malachite, or green and blue carbonate of copper.

IV. Native copper, but in very small quantity.

V. Sulphate of copper, crystallized and in solution.

VI. Sulphate of lead in considerable quantity, containing a pretty large proportion of silver.

VII. Native sulphur.

Process.—The ore is got from the mine by blasting; after which it is broken into smaller pieces by the hammer (this being chiefly done by women and children), and piled into a kiln, to which is attached by flues a long sulphur chamber. It is now covered close; a little fire is applied in different places, and the whole mass becomes gradually kindled; the sulphur sublimes to the top of the kiln, whence the flues convey it to the chamber appointed for its reception. This smouldering heat is kept up for six months, during which the sulphur chamber is cleared four times, at the expiration of which period the ore is sufficiently roasted. The poorest of this, that is, such as contains from $1\frac{1}{2}$ to 2 per cent. of metal, is then conveyed to the smelting-houses at Amlwch-port; the rest is sent to the company's furnaces at Swansea and Stanley near Liverpool. The greater part of the kilns are very long, about six feet high; and the sulphur chambers are of the same length and height, connected by three flues, and on the same level with the kilns; four new ones, however, have been built at Amlwch-port, by which much sulphur is preserved that would have been dissipated in the old kilns. The new ones are made like lime-kilns, with a contrivance to take out the roasted ore at the bottom, and thus keep up a perpetual fire. From the neck of the kiln branches off a single flue, which conveys the sulphur into a receiving chamber, built on the rock, so as to be on a level with the neck of the kiln, that is, above the ore.

The two smelting-houses, of which one belongs to each company, contain 31 reverberatory furnaces, the chimnies of which are 41 feet high; they are charged every five hours with 12 cwt. of ore, which yields $\frac{1}{2}$ cwt. of rough copper, containing 50 per cent. of pure metal; the price of rough copper is about 2l. 10s. per cwt. The coals are procured from Swansea and Liverpool, a great part of which is Wigan slack. From experiment it appears, that though a ton of coals will reduce more ore than the same quantity of slack, yet, owing to the

the difference of price, the latter is, upon the whole, preferable; the prices of the two at Liverpool being—coals 8s. 6d. per ton—slack 5s. per ditto.

The sulphate of copper, however, is the richest ore that the mine yields, containing about 50 per cent. of pure metal. This is found in solution at the bottom of the mine, whence it is pumped up into cisterns, like tanners' pits, about two feet deep: of these pits there are many ranges, each range communicating with a shallow pool of considerable extent. Into these cisterns are put cast-iron plates, and other damaged iron vessels procured from Coalbrook Dale; when the sulphuric acid enters into combination with the iron, letting fall the copper in the form of a red sediment very slightly oxidized. The cisterns are cleared once in a quarter of a year, when the sulphate of iron in solution is let off into the shallow pool, and the copper is taken to a kiln, well dried, and is then ready for exportation. The sulphate of iron remaining in the pool partly decomposes by spontaneous evaporation, and lets fall a yellow ochre, which is dried and sent to Liverpool and London.

The sulphur produced in the roasting, after being melted and refined, is cast into rolls and large cones, and sent to London. The cones are used chiefly for the manufactory of gunpowder and sulphuric acid.

Green vitriol and alum are also made in small quantities by a separate company; but to these works strangers are not admitted.

The number of men employed by the two companies is 1200 miners, and about 90 smelters; the miners are paid by the piece, and earn in general from a shilling to twenty-pence per day.

The depth of the mine in the lowest part is 50 fathoms, and the ore continues as plentiful as ever, and of a quality rather superior to that which lay nearer the surface.

With regard to the annual quantity of ore raised, little certain can be mentioned. The Parys Mine has furnished from 5000 to 10,000 tons per quarter, exclusive of what is procured from the sulphate of copper in solution; and as the two mines employ nearly equal numbers of workmen, they probably afford about the same quantity of ore.

Adjoining to the smelting-houses is a rolling-mill, upon the same construction as malt-mills, for grinding the materials for fire-bricks; these consist of fragments of old fire-bricks, with clunch (a kind of magnesian clay found in coal-pits) procured from near Bangor-ferry.

The port of Amlwch is chiefly artificial, being cut out of the rock with much labour and expence, and is capable of containing 30 vessels of two hundred tons burthen; it is greatly exposed, and dangerous of access during high northerly winds, which drive a heavy sea up the neck of the harbour. The two companies employ 15 brigs from 100 to 150 tons burthen, besides sloops and other craft, all of which lie dry at low water.

The various articles, the produce of the mines, which are exported, are the following:

- I. Coarse regulus of copper from the smelting-houses.
- II. The richer copper ore roasted.
- III. The dried precipitate of copper from the vitriol pits.
- IV. Refined sulphur.
- V. Ochre.
- VI. Alum.
- VII. Green vitriol.

The town of Amlwch, which about 30 years ago had no more than half a dozen houses in the whole parish, now supports a population of four or five thousand inhabitants; and was at present, being market-day, thronged with miners and country-people. After dinner we walked along the sea shore, climbing the steep slate rocks, whence the water below appeared of a beautiful green, and so transparent as to shew the shelving rocks to a great depth beneath.

Having heard that at Camlyn Bay, about eight miles west of Amlwch, there were some marble quarries, and that it furnished asbestos, we resolved to spend this day in visiting it: the road lay in general about half a mile from the coast; the substratum was waved green magnesian slate. When we arrived at Camlyn Bay, we looked in vain for marble or asbestos, and proceeded homewards along the coast. The shore of Camlyn Bay consists entirely of green and purple waved magnesian slate rock, with large veins of quartz. Having arrived at a promontory that separates Cemmaes Bay from the former, we found it to consist of a fine blue-veined limestone, or common marble. Some way on, near the village of Cemmaes, this limestone is cut through by a stratum descending to the water, about 40 yards wide, of black slate, containing iron pyrites; and in the caverns dug in this, probably in a fruitless search after metals, are efflorescences of sulphate of iron and chalybeate springs. To this succeeds a beautiful water-grey sand, mixed with lime but of little coherence, on exposure to the air, taking an ochrey stain. Adjoining to this are a few yards of calcareous free-stone, and then a cliff of very hard white and water-grey marble; a range of sand and loose free-stone succeeds, and the bay terminates with a marble promontory. The soil of the land surrounding the bay is for the most part, especially near the village, a deep sand. The limestone terminates shortly after, and the green-waved magnesian slate continues the boundary of the island. This ridge of lime is in general higher than the slate, describing an irregularly indented line of coast, about four miles long: its breadth varies from a quarter to half a mile; and a narrow valley, forming its outline towards the land, separates it entirely from the asbestine slate, thus preventing any intermediate strata.

The whole of this coast is cut out into bays or recesses of various forms and dimensions, with lofty projecting promontories, which are for the most part fine sheep-walks. A number of islands also are formed by ledges of rock, many of them a good way out at sea, and at high water just appearing like black spots in the midst of the waves: many of these creeks are secure havens for small vessels, which are protected from west and south-west winds by the rocks. The village of Cemmaes stands upon a little creek opening into a most beautiful bay about a mile across; its entrance into the main sea is guarded on each side by a craggy promontory, the one of grey, the other of snow-white marble, glistening above the green sea, smooth as the surface of a mirror, and whose sparkling transparency baffles description. In the interior recess of the bay, the bank of black slate, mentioned above, was finely contrasted with a lofty irregular projecting arch of white marble, pierced by the constant dashing of the waves; while the sounds of laughter and merriment, proceeding from two boats' crews of young people, that had just pushed out of the creek on a party of pleasure, added double life and interest to this lovely scene. The land adjoining the cliffs, that overlook the sea, produces a great deal of corn, chiefly oats and barley. A golden tinge
already

already begins to appear, that will usher in the harvest, as soon as the crop of hay with which the farmers are now busied is safely housed.

As we approached Amlwch, we were much pleased with seeing the scars of rock between the town and sea occupied by numerous groupes of men, women and children, all neat and in their best clothes, it being Sunday, who were enjoying the mild temperature of a summer evening rendered refreshing by the neighbourhood of the sea. In one place we observed a circle of men gathered round a point of rock, in which was seated the orator of the party reading a newspaper aloud, and commenting upon it; on other little eminences were seen family parties, the elder ones conversing, and the younger children gamboling about them, or running races with each other: in a new-mown meadow close to the town, we passed by a large company of lads and lasses seated on a green bank, chatting, laughing, and full of mirth and frolic. To one who had been a spectator of the gross and riotous delight too frequent on holiday-evenings in the outskirts of the metropolis, or any large town in England, the contrast could not fail of being very striking, and much to the advantage of the inhabitants of Amlwch: out of the whole number we did not see one drinking party; the pleasures of society and mutual converse needed not the aid of intoxication to heighten their relish.

Mean time the song went round, and dance and sport,
 Wisdom and friendly talk, successive, stole
 Their hours away: while in the rosy vale
 Love breath'd his infant sighs, from anguish free,
 And full replete with bliss; save the sweet pain
 That inly thrilling but exalts it more.

Harmonious Nature too look'd smiling on:
 Clear shone the skies, cool'd with eternal gales,
 And balmy spirit all.

THOMSON.

I am acquainted with no place the manners of whose inhabitants are so unexceptionable (as far at least as a stranger is enabled to judge of them) as Amlwch; and the favourable opinion which I was led to entertain of them, on visiting the town last year, is confirmed by what I have observed at present. Not a single instance have I known of drunkenness; not one quarrel have I witnessed during two very crowded market days, and one of them a day of unusual indulgence, that I passed at this place; and I believe no gaol or bridewell, or house of confinement, exists in the town or neighbourhood. Most of the miners are methodists, and to the prevalence of this religious sect is chiefly to be attributed the good order that is so conspicuous. Men who have been long confirmed in habits of vice and irregularity, need arguments the most potent that can be offered to counterbalance the associated power of habit and inclination: were it possible forcibly to tear them from their connections, and to place them in an entirely different situation, reason might then be called in gradually to perfect the cure; but where this cannot be done, (and in most cases it is impracticable,) what argument can be urged of such overbearing force as to combat with and overthrow the most rooted propensities, even upon their own territory, unassisted by external coercion, except a strong and impressive appeal to their hopes and fears; and, by pre-

senting both exaggerated and in full contrast, to overwhelm the mind by surprise and alarm?

After supper we strolled up to the mountain, which now no longer resounded with the confused noise of pickaxes and hammers; all was hushed in profound silence; and the moon-beams, which were reflected bright from the sides of the vast excavations, could scarcely penetrate the deep abyss below. As we returned we were struck with the clear red vivid flames issuing in a large body from the long range of smelting-houses on the coast, and casting their rays to a great distance.

VIII.

A Method of disposing GUNTER'S Line of Numbers, by which the Divisions are enlarged, and other Advantages obtained.

OF the many ingenious instruments for computation which were in use during the last century, among mathematicians, scarcely any are to be found at present except the sector and the logarithmic line of Gunter. The ease and accuracy of computation by those admirable numbers have rendered the others of little importance; but the sector has maintained its station from its utility in graphical operations, and the Gunter's line is not only of great value in nautical and other proportions which do not exceed three places of figures, but also as a check to assure the truth of the leading figures in more extended calculations. About ten years ago I communicated to the Royal Society a method of extending the range of this last instrument, which I still consider as less generally known than its utility may perhaps claim. The principles depend on the following considerations:

1. If two geometrical series of numbers, having the same common ratio, be placed in order with the terms opposite each other, the ratio between any term in one series and its opposite in the other will be constant *.

2. And the ratio of a term in one series to any term in the other, will be the same as obtains between any other two terms having the same relative position and distance †.

3. In all such pairs of geometrical series as have the same common ratio, the last-mentioned property obtains, though the first antecedent and consequent be taken in one pair, and the second in any other pair ‡.

4. If the differences of the logarithms of numbers be laid in order upon an arrangement of equi-distant parallel right lines, in such a manner as that a right line drawn across the

$$* \text{ Geom. series } \begin{cases} a & an & an^2 & an^3 & an^4 \\ b & bn & bn^2 & bn^3 & bn^4 \end{cases}$$

$$\text{Then } a : b :: an : bn :: an^2 : bn^2, \&c.$$

$$† \text{ In the foregoing series } a : bn^2 :: an^2 : bn^4 :: an : bn^3, \&c.$$

$$‡ \text{ Geom. series } \begin{cases} a & an & an^2 & an^3 & an^4 \\ b & bn & bn^2 & bn^3 & bn^4 \end{cases}$$

$$\text{Geom. series } \begin{cases} d & da & da^2 & da^3 & da^4 \\ bd & bda & bda^2 & bda^3 & bda^4 \\ \frac{d}{a} & \frac{da}{a} & \frac{da^2}{a} & \frac{da^3}{a} & \frac{da^4}{a} \end{cases}$$

$$\text{Then } a : bn^2 :: da : \frac{bda^2}{a}, \&c.$$

whole shall intersect it at divisions which denote numbers in geometrical progression; then, from the condition of the arrangement, and the property of this logarithmic line, it follows, first, that every right line so drawn will, by its intersections, indicate a geometrical series of numbers*; secondly, that such series as are so indicated by parallel right lines, will have the same common ratio†; and thirdly, that the series thus indicated by two parallel right lines, supposed to move laterally without changing either their mutual distance, or parallelism to themselves, will have each the same common ratio; and, in all pairs of series indicated by such two lines, the ratio between an antecedent on one parallel and the opposite term on the other, taken as a consequent, will be constant‡.

5. In the foregoing paragraphs the logarithmic line has been considered as unlimited. On such a line, therefore, any antecedent and consequent being given, it would be possible to find both on the arrangement, and to draw two parallel lines, one over each number: and if the lines be then supposed to move without changing either their distance or absolute direction, so that the line, which before marked an antecedent, may in the second station

mark

* Let AB, CD, EF (Plate XVI. Fig. 1.) be portions of the logarithmic line arranged according to the condition; let GH be a right line drawn across, so as to pass through points of division e, c, a , denoting numbers in geometrical progression; then will any other line IK, drawn across the arrangement, also pass through points f, d, b , denoting numbers in geometrical progression.

DEMONSTRATION. From one of the extreme points of intersection f , in the last named line IK, draw the right line fg parallel to GH, and intersecting the arrangement in the points i, b ; and the ratios of the numbers $e : f, c : i$, and $a : b$, will be equal, because the intervals on the logarithmic line, or differences of the logarithms of these numbers, are equal:

$$\text{Or } \frac{e}{c} = \frac{f}{i} \text{ and } \frac{c}{a} = \frac{i}{b}.$$

$$\text{But } \frac{e}{c} = \frac{c}{a} \text{ by the condition.}$$

Therefore $\frac{f}{i} = \frac{i}{b}$; or the numbers f, i, b , are in the same continued ratio as the numbers e, c, a .

Again, the point f , the line id , and the line bb , are in arithmetical progression, and denote the differences of the logarithms of the numbers f and f, i and d, b and b .

The quotients of the numbers themselves are therefore in geometrical progression, that is,

$$\frac{f}{f} : \frac{d}{i} : \frac{b}{b}, \text{ or } \frac{i}{d} = \frac{db}{bi}.$$

$$\text{Or } \frac{i}{d} = \frac{di}{bf}, \text{ by substituting } \frac{i}{f} \text{ for its equal } \frac{b}{i}.$$

$$\text{Whence } \frac{f}{d} = \frac{d}{b} \text{ or } f : d : b.$$

Q. E. D.

† In the same manner, as it was proved that the line fg parallel to GH passes through points of division denoting numbers in the same continued ratio as those indicated by the line GH, it may also be shewn, that the line LM, parallel to any other line IK, will pass through a series of numeral points having the same continued ratio as the series indicated by that line IK to which it is parallel.

‡ Because the lines preserve their parallelism to their former situation, they will indicate geometrical series having the same common ratio as before; and, because their distance measured on the logarithmic line remains unchanged, the differences of the logarithms of opposite numbers, and consequently their ratio, will be constant.

mark a new antecedent, the other (by 2. and 3.) will mark a number at the same relative position and distance, which will be the consequent to this last antecedent, after the same ratio.

6. Suppose a logarithmic line to contain no more than a single range of numbers from 1 to 10, it will not be necessary, for the purposes of computation, to repeat it; for, if a slider or beam have two fixed points at the distance of the interval between 1 and 10, and a moveable point be made to range between these (always to indicate the antecedent), in this case, if the consequent fixed point fall without the rule, the other fixed point will shew the division it would have fallen on if the rule had been prolonged. This may be easily applied to the arrangement described, N^o 4.

7. If the arrangement consist only of the logarithms from 1 to 10, and the parallel cross lines intersect that geometrical series whose successive ratios altogether, with that of the last to the first, make by composition the ratio $\frac{1}{10}$, the contrivance N^o 6. may be applied to shew such consequents as fall, laterally, without the rule:

8. It will be convenient that the arrangement of the lines should be disposed so as to occupy a rectangular parallelogram; or, in other words, that the cross line, cutting the series last mentioned, may be at right angles to the length of the rule.

The construction of an instrument on the principles here explained will admit of various dispositions of the graduated lines and apparatus for measuring intervals upon them. In the Transactions I gave a figure of a rule consisting of ten parallel lines, equivalent to a double line of numbers, upwards of twenty feet in length, with a beam compass for measuring intervals.

Fig. 2. Plate XVI. represents a Gunter's scale, equivalent to that of 29½ inches in length, published by the late Mr. Robertson. It is, however, but one eighth part of the length, and contains only one-fourth of the quantity of division. In the slider GH is a moveable piece AB, across which a fine line is drawn; and there are also lines CD, EF, drawn across the slider, at a distance from each other equal to the length of the rule. The sketch No. 1. represents one face or side of the instrument, and No. 2. represents the opposite face. Each contains one-fourth part of the line of numbers. When it is used, the slider must be set so that the line on the piece AB may be placed at the antecedent, and one of the end marks CD, or EF, may be opposite the consequent. After this adjustment of the slider, the whole may be moved at pleasure, till the piece AB is set at any other required antecedent; and then the same line CD or EF, as before, will indicate the consequent at the same distance or position as before. But if the consequent mark of the slider should fall without the rule, the other line will indicate the required consequent upon the rule, though at the distance of one line on the rule farther off in position than the other consequent mark would else have shewn it. The operations are obvious and familiar upon the rule itself.

Another instrument was described in the Transactions, which was equivalent to the same rule, but of a circular figure, one inch and a-half diameter. The graduations were made upon three concentric circles, and a sector was applied instead of the slider. On account of the larger figure I have here given, it becomes unnecessary to describe this.

I approve

I approve of this construction as superior to every other which has yet occurred to me, not only in point of convenience, but likewise in the probability of being better executed, because small arcs may be graduated with very great accuracy, by divisions transferred from a larger original.

The circular instrument is a combination of the Gunter's line and the sector, with the improvements here pointed out. The property of the sector may be useful in magnifying the differences of the logarithms in the upper part of the line of sines, the middle of the tangents, or the beginning of the versed sines. It is even possible, as mathematicians will easily conceive, to draw spirals, on which graduations of parts, every where equal to each other, will shew the ratios of those lines by means of moveable radii similar to those on such an instrument.

After the publication of the account in the Transactions of the Royal Society, the mention of circular and spiral instruments brought to the recollection of the late Mr. George Adams, of Fleet-street, (who had seen the straight instrument some years before it was communicated to the Society,) that he had a spiral engraved on a brass plate by his father. He made me a present of the plate, and shewed me, by a manuscript, that it was constructed in the year 1748. The spiral has ten turns, and its external diameter is twelve inches. It contains the numbers, sines and tangents, the latter being twice repeated, and may with ease be used to compute to four places and an estimate figure; which is to the full as much as could be done with a common Gunter's scale of sixty feet in length.

Fig. 3. Plate XVI. represents the line of numbers drawn according to this system. The sector ACB is used to measure the ratios. One thread must be set to the antecedent, and the other to the consequent. If the antecedent thread be then removed without altering the angle to any other antecedent, the other thread will mark a consequent at the distance of the same number of turns of the spiral in the same direction. In case the number of turns should proceed without the system, it will be necessary to return and reckon onward from the opposite extremity of the spiral to complete the number.

IX.

On the Mechanical Projects for affording a Perpetual Motion.

IN consequence of the notice * taken of Mr. Varley's attempt to produce a perpetual motion, I have been requested by several correspondents to state how far the mechanical scheme for which Dr. Conrad Shivers took out a patent in the year 1790, for the same object, may be worthy of attention. I have, on that occasion, mentioned the difficulties which have prevented any clear general demonstration of the absurdity of this pursuit from being produced, though it has not been difficult to shew the fallacy of the individual plans. It does not indeed seem easy to enunciate the scheme itself. What in universal terms is

* Philof. Journ. I. 334.

the thing proposed to be done? Is it to cause a body, or system of bodies, to act in such a manner that the re-action shall be greater than the action itself, and by that means generate force by the accumulation of the surplus? Or, can the motion communicated be greater than that lost by the agent? Since these positions are evidently contrary to the physical axioms called the Laws of Nature, and frictions and resistances would speedily destroy all motions of simple uniformity, it may be presumed that s^t Gravefande, who thought that all the demonstrations of the absurdity of schemes for perpetual motion contained paralogism, would have stated the proposition under different terms. But without entering into this apparently unprofitable disquisition, it may be useful, as well as entertaining, to make a few observations on the mechanical contrivances which depend on a mistaken deduction from the general theorem respecting the balance, among which that of Dr. Shivers must be classed.

There is no doubt but numerous arrangements have been made, and still are laboured at by various individuals, to produce a machine which shall possess the power of moving itself perpetually, notwithstanding the inevitable loss of force by friction and resistance of the air. Little, however, of these abortive exertions, has been entered upon record. The plans of Bishop Wilkins, the Marquis of Worcester, and M. Orfyreus, are all which at this time occur to my recollection.

There is no doubt but the celebrated Wilkins was a man of learning and ability. His Essay towards a real character and a philosophical language is sufficient to render his name immortal. Twenty years before the appearance of that work he published his *Mathematical Magic*, namely in the year 1648, containing 295 pages small octavo, which, from the number of copies still in being, I suppose to have been a very popular treatise. It is in this work that I find, among other contrivances for the same purpose, a wheel carrying sixteen loaded arms, similar to that delineated in fig. 4, plate xv. in which, however, for the sake of simplicity, I have drawn but six. Each lever A B C D E F is moveable through an angle of 45° , by a joint near the circumference of the wheel, and the inner end or tail of each is confined by two studs or pins, so that it must either lie in the direction of a radius, or else in the required position of obliquity. If the wheel be now supposed to move in the direction EF, it is evident that the levers A B C D, by hanging in the oblique position against the antecedent pins, will describe a less circle in their ascent, than when on the other side they come to descend in the positions EF. Hence it was expected that the descending weights, having the advantage of a longer lever, would always predominate. Dr. Wilkins, by referring the weights to an horizontal diameter, has shewn that in his machine they will not. A popular notion of this result may also be gathered from the figure, where there are three weights on the ascending, and only two on the descending side; the obliquity of position giving an advantage in point of number, equal to what the other side may possess in intensity. Or if this contrivance were to be strictly examined, on the supposition that the levers and weights were indefinitely numerous, the question would be determined by shewing that the circular arcs AK, HI, are in equilibrio with the arcs AG, GL.

The simplest method of examining any scheme of this kind with weights, consists in enquiring whether the perpendicular ascents and descents would be performed with equal masses in equal times. If so, there will be no preponderance, and consequently no motion. This is clearly the case with the contrivance before us.

The Marquis of Worcester, who will ever be remembered as the inventor of the steam engine, has described a perpetual motion in the 56th number of his *Century of Inventions*, published in the year 1655, and since reprinted in 1767 by the Foulis's at Glasgow. His words were as follow:

"To provide and make, that all the weights of the descending side of a wheel shall be perpetually further from the centre than those of the mounting side, and yet equal in number, and heft to the one side as the other. A most incredible thing if not seen, but tried before the late King (of blessed memory) in the Tower by my directions, two extraordinary ambassadors accompanying his Majesty, and the Duke of Richmond and Duke Hamilton, with most of the Court attending him. The wheel was 14 feet over, and 40 weights of 50 pounds a piece. Sir William Balfour, then Lieutenant of the Tower, can justify it with several others. They all saw, that no sooner these great weights passed the diameter line of the lower side, but they hung a foot further from the centre; nor no sooner passed the diameter line of the upper side, but they hung a foot nearer. Be pleased to judge the consequence."

Defaguliers, in his course of *Experimental Philosophy*, vol. I. page 185, has quoted this passage, and given a sketch of a pretended self-moving wheel, similar to that of fig. 5, plate xv. as resembling the contrivance mentioned by the Marquis of Worcester. The description of this last engineer agrees, however, somewhat better with the contrivance fig. 4. It must of course be a mistake in terms, when he says the weight receded from the centre at the lower diameter, and approached towards it at the upper: the contrary being in fact necessary to afford any hope of success; and accordingly in the quotation it is so stated. I am therefore disposed to think that fig. 5 represents the wheel of Orfyreus at Hesse Cassel, much talked of about the year 1720, and which probably was made to revolve, during the time of exhibition, by some concealed apparatus. It consists of a number of cells or partitions distinguished by the letters of the alphabet, which are made between the interior and exterior surfaces of two concentric cylinders. The partitions being placed obliquely with respect to the radius, and a cylindrical or spherical weight placed on each, it is seen from the figure, that these weights will lie against the inner surface of the large cylinder, whenever the outer end of the bottom partition of any cell is lowest; and on the contrary, when that extremity is highest, the weight will rest on the surface of the interior cylinder. Let the wheel be made to revolve in the direction ABC; the weights in CDEFGHI being close to the external circle, and the weights KLMAB close to the inner, for the reasons last mentioned. As the cell B descends, its weight will likewise run out, at the same time that the weight in the cell I will run in, in consequence of its partition being elevated. By the continuation of this process, since all the weights on the descending side pass down at a greater distance from the centre, while those on the ascending side rise for a considerable part of their ascent at a less distance from the same point, it is concluded that the wheel will continue to maintain its motion. On this, however, it is to be remarked, that the perpendicular ascent and descent are alike, both in measure, and in time of performance; and that the familiar examination, even to those who know little of such subjects, is sufficient to show that the preponderance is not quite so palpable as at first it appears. For the weights G and F, H and E, I and D, are evidently in equilibrio, because at the same horizontal distance from the centre; and if the favourable supposition that the

weight B hath already run out be admitted, it will then remain a question whether these two exterior weights, B and C, can preponderate over the four inner weights K L M A. The more accurate examination of this particular contrivance will lead to the following theorem. In two concentric circles, if tangents be drawn at the extreme points of a diameter of the smaller, and continued till they intersect the larger, the common centre of gravity of the arc of the greater circle included between the tangents, and of the half periphery of the smaller circle on the opposite side of the diameter, will be the common centre of the circles. If, therefore, the balls were indefinitely numerous and small, the supposed effective parts of the wheel, fig 5, would be in equilibrio, as well as the parts beneath the horizontal tangent of the inner circle.

Fig. 6 represents the contrivance of Dr. Shivers, which, in a periodical publication, in other particulars respectable, has been said to continue in motion for weeks, and even months together. There is not the smallest probability that it should continue in motion for half a minute, or nearly as long as a simple wheel would retain part of its first impulse. The external circle denotes a wheel carrying a number of buckets, A B I L, &c. C represents a toothed wheel, on the same axis, which drives a pinion D; and this last drives another pinion E upon the axis of a lantern, or wheel intended to work a chain pump with the same number of buckets as in the large wheel A B I L. The lantern G is made of such a size as to raise the buckets a b i l with a due velocity. K represents a gutter, through which a metallic ball, contained in the bucket m may run and lodge itself in the bucket A of the wheel. Each of the buckets of the wheel B I L M, which are below the gutter, is supplied with a metallic ball, and so likewise are the ascending buckets a b i l m of the chain-pump. As the pump supplies the wheel, it is itself again supplied at M, where the balls fall into its ascending buckets. Now it is presumed that the balls in the wheel, I suppose on account of their distance from the centre of motion, will descend with more than sufficient force to raise those on the chain, and consequently that the motion will be perpetual.

The deception in this contrivance has much less seduction than in the two foregoing, because it is more easily referred to the simple lever. This, like the others, exhibits no prospect of success, when tried by the simple consideration of the equality of the ascent and descent in the whole time of the rotation of a single ball. It may also be shewn from the principles of wheel work, which are familiar to artificers, that whatever is gained by the excess of the diameter of the great wheel beyond that of the wheel C, is again lost by the excess of the lantern A beyond the pinion E.

The fundamental proposition of the simple lever or balance, that equal bodies at an equal distance from the fulcrum will equiponderate, but that at unequal distances the most remote will descend, has in these and numberless other instances led mechanical workmen and speculators to pursue this fruitless enquiry with labour and expence often ill-afforded, and with a degree of anxiety and infatuation which can hardly be conceived by those who have never suffered the pain of hope long deferred. For this reason chiefly, it has appeared desirable and useful to treat the subject in a familiar way, without descending to those expressions of contempt, which ignorance, harmless to all but itself, is surely not entitled to. If such reasoners were well convinced that the power of a machine is to be estimated by the excess of motion referred to the perpendicular, without any regard to the apparent centre

centre of the machine, and that in machines very little compounded it is possible to produce effects directly contrary to the rule which is true of the simple lever, they would probably renounce many flattering projects, grounded only on the supposition of its universality. Defaguliers contrived an apparatus in which two equal weights may be placed at any distance whatever from the centre of motion, and still continue in equilibrio. Fig. 3 represents this instrument, AD denotes a balance with equal arms, and EF another of the same dimensions. These move on the centres B and C, and are connected by the inflexible rods AE and DF; the motion being left free by means of joints at the corners. Across the rods AD, EF, are fixed two bars IK, LM. Now it is unnecessary to shew that the weight G will describe exactly the same line or circular arc, when the levers are moved into the position a d f e, or any other position, as it would have described in case it had been suspended at A, or K, or E; and that it is of no consequence in this respect at what part of the line AE, or IK, it be fixed. The same observations are true of the weight H on the other side. And accordingly it is found, that these equal weights may be suspended any where on the lines IK and LM without altering their equilibrium.

By this contrivance it is most evidently proved, to those who are totally unacquainted with the theory, that weights do not preponderate in compound engines, on account of their distance from the centre. Several other contrivances may be made to the same effect. The following combination of wheel-work presented itself to me as one which would most probably be mistaken for a perpetual motion. Fig. 2. Plate XV. The five circles represent the same number of wheels, of equal diameter and number of teeth, acting together. The middle wheel A is fixed between two upright pillars, so that it cannot revolve. The other four wheels are pinned in a frame HI, in which they can revolve, and through which the axis of A likewise passes. From the extremity of the axis of D, and also of d, proceed the horizontal levers HK and IL, which are equal, and point in the same direction parallel to the plane of the wheels. At the extremity of these arms hang the equal weights P and p. Let it now be imagined that the end I of the frame is depressed; the wheel B will turn round by the re-action of the fixed wheel A in the same direction as HI, and it will make one revolution, in the same time, relative to the frame, or two with regard to absolute space by reason of its being carried round. The action of B upon D will produce a rotation relative to the frame in the opposite direction during the same time. Instead therefore of two revolutions, like the wheel B, this wheel D, with regard to absolute space, will not revolve at all, and in every position of the apparatus the arm IL will continue horizontal, and point the same way. For similar reasons the arm HK will retain its position. Consequently, it is seen that the descending weight will move at a great horizontal distance from the centre N, while the ascending weight rises very near that centre. But there will not on this account be a perpetual motion: for the actions of the levers HK and IL upon the frame HI, by means of the toothed wheels, will in the detail be found precisely alike, and in the general consideration of the motions of P and p, the opposite motions in the circle EFG will be accurately the same.

It has always been considered as essential to a perpetual motion, that it should be derived from some energy which is not supposed to vary in its intensity. Such are the inertia, the gravity or magnetism of bodies. For an occasional or periodical variation of intensity in any force is evidently productive of motion, which requires only to be accumulated or

applied, and the apparatus for applying it cannot be considered as a machine for perpetual motion. Neither in strictness can any machine, whose motion is derived from the motion or rotation of the earth, and the consequent change of seasons and rotation of events, be so considered, because it does not generate, but only communicates. The perpetual flow of rivers, the vicissitudes of the tides, the constant, periodical and variable winds, the expansions and contractions of air, mercury, or other fluids, by daily or other changes of temperature, the differences of expansion in metals by the same change, the rise and fall of the mercury in the barometer, the hygrometric changes in the remains of organized beings, and every other mutation which continually happens around us, may be applied to give motion to mills, clocks, and other engines, which may be contrived to endure as long as the apparatus retains its figure.

X.

Useful Notices respecting various Objects.—Silver alloyed with Crude Platina.—Tempering of Steel.—Rifled Shot.

1. *Silver alloyed with Crude Platina.*

BERGMAN, in his Treatise on the Blow-Pipe, in one of the earlier editions, directed that the spoon for blow-pipe experiments should be made of silver alloyed with one tenth of platina, of which the purification was at that time little known. Dr. Lewis, in his Philosophical Commerce of Arts, mentions the fusion of silver with crude platina. The metals united but imperfectly, with a remarkable projection of particles of the metal, as if by a kind of ebullition over the inside of the crucible. Several years ago, being desirous of making such a spoon, I selected ten grains of crude platina, in particles possessing very little magnetism, and fused the same with one hundred grains of pure silver in a blast furnace, using a large proportion of nitre, with the intention of scorifying any of the baser metals. The effect which Lewis mentions took place; and the compound, when poured out, had a scabrous or unsound appearance. I thought the grains of platina might have been merely surrounded by the adherent silver; but this did not seem to be the case, for it bore laminating between two steel rollers very well. After this last process, I subjected it to fusion again with a stronger heat, and again laminated it into a thin plate. This operation of fusing and rolling was repeatedly performed, but still the metal appeared rough in certain parts of the surface. As a last effort, I therefore exposed it to the most violent heat I could urge, and determined to leave it to cool in the crucible beneath the nitre which flowed above it. The crucible in the ignited state was taken, from some motive I cannot now recollect, to a window, and set down upon a tile. As I stood attentively observing the appearance of the metallic globule through the transparent and tranquil bath of nitre, the ignition gradually went off, so as to be scarcely visible in that clear light. But on a sudden it recovered itself in an instant, and the nitre boiled up so as to fill the vacant space of the crucible. The button of metal, when cooled, was scabrous; but I

nevertheless formed it into a spoon, by rolling, hammering, and afterwards polishing it. It was then exposed to a low red heat, in a common fire, and became blistered all over.

The above facts possess the utility which attends unsuccessful experiments, namely, that the narration may save others from a repetition of the labour. But, philosophically speaking, there are no new experiments which would be unsuccessful if we thoroughly understood them. I think the decay and recovery of ignition in this is a curious instance of what seems to be a general law of the congelation of fluids. It is probable that all fluids, as well as water, are capable of being cooled below their freezing point, and afterwards become hotter by the escape of latent heat when they congeal. Thus water cooled below 32 degrees is suddenly raised to that temperature the instant ice is formed, because the ice gives out the difference of the heat which was latent in the fluid state. And so it appears to have happened with the fluid metallic compound in these experiments. As its temperature was diminished, it became less luminous or ignited; but, at the instant of congelation, that portion of heat which had been employed in maintaining the fluid state, was extricated, and became employed in raising the temperature. The effect of this increase was seen in the greater emission of light, and the boiling of the nitre.

2. Tempering of Steel.

INSTRUMENTS of steel are required to be hard, in order that they may penetrate and divide the substances intended to be cut; and tenacious, that they may not break during the operation. The hardest steel is the most brittle; for which reason, though hardness would in every case be a desirable quality, yet, for the sake of tenacity, it is in many instances necessary to diminish it. We see, therefore, that there must be a precise mean between too soft and too brittle, which will be best suited for the respective purposes to be accomplished. A spring must be tenacious, and need not be very hard. A knife for cutting leather, and soft substances, must be somewhat harder than a spring. Pen-knives and razors must be still harder; and files and tools for working metal must be hardest of all, though even in these care must be taken not to destroy their tenacity by making them too hard. Steel is hardened by ignition, and subsequent plunging in water. The chief art of this process consists in covering the steel with some mixture, which shall prevent its being degraded in the fire to the state of iron. The file-makers use the grounds of beer mixed with common salt. Others use the cementing mixture. No greater heat is to be used with any steel, than by experience is found sufficient to produce hardness at least equal to that of a file. More heat would render the grain coarse and open. Urine is thought to be better for quenching the steel than water, probably because it may be a better conductor of heat, and perhaps on account of its phosphoric ingredient, which is now with justice supposed* to be an essential part of steel. When steel is not intended to possess the utmost hardness, it is afterwards softened by the application of a lower degree of heat. This operation is called tempering. The greatest difficulty consists in applying the proper degree of heat uniformly over the whole mass. The common method is to judge by the colour assumed by the clean surface of the steel when thus heated. The heat may be applied by the fire, or a pan of charcoal, or the flame of a candle or lamp, or by laying the

* Philosophical Journal.

piece upon sand to be gradually heated, or upon melted lead. The saw-makers, and some makers of springs, heat the article, rub it with greafe, and then heat it still farther till the fumes take fire; this is called blazing, and affords a temper nearly the same as when the steel by heat has acquired a deep blue colour. When the temper is given from the colour, the first tinge which appears is a faint straw colour, which is suitable to pen-knives and hard cutting-tools. The next colour, which is purple, is rather too soft for a knife, and too brittle for a spring. After this follows the blue, of which there are several shades. The deepest is very soft, and is succeeded by a whitish yellow, which indicates too great a degree of softness for any cutting-tool.

Mr. Hartley, in the year 1789, took out a patent for a method of tempering steel. His specification, which is so general as perhaps to include no method at all in the way of monopoly, indicates that the heat is to be measured by a pyrometer or thermometer applied near the article. The actual practice of this method appears to consist in using oil and a mercurial thermometer. In this way many dozens of razors or tools may be tempered at once with the utmost facility. The different degrees of heat for various kinds of steel, and their several uses, may speedily be determined by experiment. For want of a thermometer graduated to the higher degrees, I have not yet made any experiments. The only fact I have at present to communicate is, that Mr. Stodart, who uses this method, states that the requisite temper on steel for a pen-knife is 450 degrees of Fahrenheit's scale*. I find, on trial, that a good pen-knife is as hard as any tool which can admit of tempering. Hard gravers, for turners' use, must not be softened at all.

3. *Rifled Shot.*

IN the latter end of the year 1789, I was, by various considerations, induced to think, that the effect which is produced by rifling musquetry might be produced in artillery by giving a suitable figure to the shot. It is almost needless to explain this effect. When a bullet is driven along the bore of a piece, it must be acted upon by the internal surface so as to cause a rotation, the axis of which motion will lie across the line of direction. In consequence of this, the re-action of the air will be stronger on one side of the bullet than on the other, and it will deviate from the intended course according to no certain rule. The method of rifling consists in cutting one or more spiral grooves in the hollow surface of the musquet, into which the ball is either forcibly rammed down, or else conveyed to its place by an aperture at the breach, or near the chamber. The lead is thus made to fit the internal screw, and usually takes about half a turn during its course through the barrel. The axis of this rotation being parallel to the line of direction, it must follow that the resistance of the air will be equal on all sides of the bullet, and it will fly with more certainty to the object of aim. It seemed to me, that if a cylindrical shot, with hemispherical ends, were thrown out of a common barrel, it might be possible, by means of certain spirals cut on the end surface, to cause the blast of the powder and the resistance of the air to concur in producing the same rotation.

For this purpose I took a wooden pattern, and cut the spherical surface into twelve spiral planes, by dividing the equator into the like number of equal parts, and drawing spirals from

* Philosophical Transactions 1795, p. 326.

the points of division obliquely towards the poles. The wood between every pair of contiguous spirals was then taken away, by cutting from the one line parallel to the axis, and from the other perpendicular to a plane passing through it. By this process, when the axis was set upright, there appeared, as it were, twelve roads sloping upwards from the equator towards the pole, bounded on the side next the wood by upright walls; and the shot, when suspended on an axis or centre point, could be blown round very swiftly by the breath directed towards the pole.

Shot of this kind were made and tried at a foundry in North Wales. By an experiment with a brass gun newly bored, it was ascertained that the shot did really revolve in its course along the bore; but the trials with shot of different weight and dimensions did not promise more accuracy of effect than was obtained by common spherical shot used at the same time. Particular notice was taken of the manner in which the shot struck the butt: the greatest number of times, it struck with the anterior end; sometimes the stroke was made with the broad side, and, in a few instances, the end which came last out of the gun arrived first at the mark. Hence it appears, that the very slight angular deviation at the mouth of the piece is more than sufficient to counteract any effect which might else have been derived from the subsequent action of the air upon a projectile duly figured.

It seems, nevertheless, that this principle might be applied to advantage in bar shot. If the ends of this projectile were chamfered or sloped with respect to the axis, it would pass through the air with a revolution of its extremities, instead of one end following the path of the other, as may sometimes be supposed to happen.

With regard to the execrable practice of war, I think it a decided question, that increase of power is, on the whole, in favour of rectitude and virtue; and that wars are likely to be fewer, less durable, and less pernicious, the more scientifically they are conducted.

MATHEMATICAL CORRESPONDENCE.

QUESTION IX. Answered by J. F.—:—:—R.

IN order to a solution of this problem, we have only to find the point in the plane of the horizon, which is perpendicularly under the elevated object; the distances of which point from the three stations are as the cotangents of the angles of elevation taken at each respectively. This will be effected by the following

CONSTRUCTION.

LET A, B, and C be the three stations (Plate XV. Fig. 1.) and a , b , and c the cotangents of the respective angles of elevation taken at each. Produce AB and CB towards E and F, and make BF = BA, and DE = BC. Take FG to FB as c to b , and EG to EB as a to b , and about the centres F and E with the respective radii FG and EG describe arcs intersecting in G. Draw FG, BG, EG, and in GB produced take BD to AB as BE to BG. I say the point D is the required point in the horizontal plane.

For,

For, draw AD and CD.—Then the triangle GBE being similar to ABD, and GBF to CBD, we have $AB (= FB) : \frac{c}{b} AB (= FG) :: b : c :: BD : CD$ and $BC (= BE) : \frac{a}{b} BC (= EG) :: b : a :: BD : AD$. Therefore AD, BD and CD being as a , b , and c respectively, the point D is rightly found. Q. E. D.

Hence also, if with AD ($: AB :: EG : BG$) and CD ($: CB :: FG : BG$) as radii, arcs be described about A and C, they will intersect each other, and each of them will intersect GB produced in the required point.

CALCULATION.

FROM the values of FB, BE, FE, FG, and EG, find BG trigonometrically, and from thence AD, BD, or CD.

THIS question was answered in nearly the same manner by ANALYTICUS, who observes, that if the three stations are at unequal distances from the object, its apparent altitude will vary at each of them, according to the effect of refraction, and of course occasion a three-fold error in the result; which may be avoided by taking the stations so that the angles of elevation shall be all equal, in which case a single refraction only will be concerned, which indeed is sufficient to render this, or any other similar method of determining the heights of terrestrial objects, very inaccurate. If, besides the angles of elevation, those formed by the base and hypotenuses drawn from each extremity of it, to the top of the object, be taken, there is no occasion for more than two stations; which being chosen in sets, in the way last mentioned, will enable us to ascertain the difference of refraction at different distances.



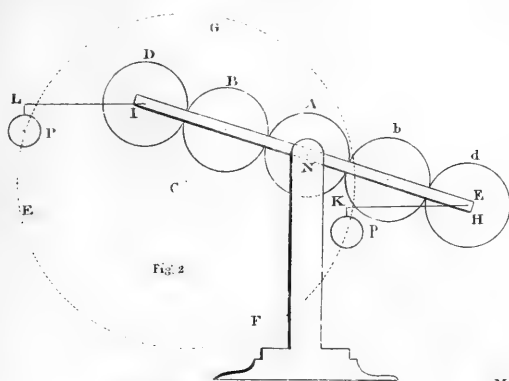
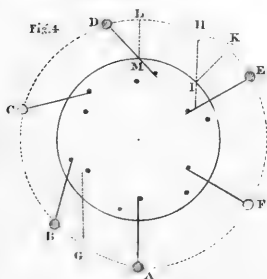


Fig. 2

By Bishop Williams



Schemes for
Perpetual Motion

By the Marquis of Worcester

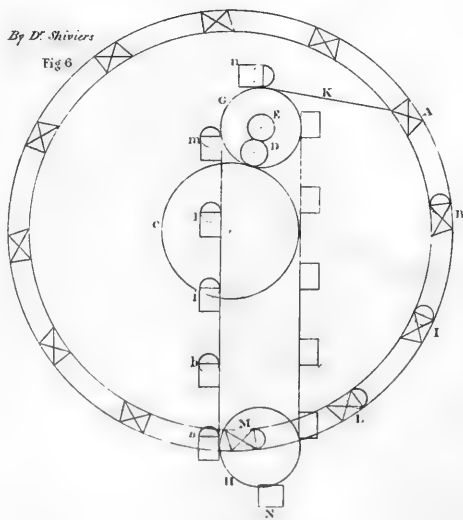
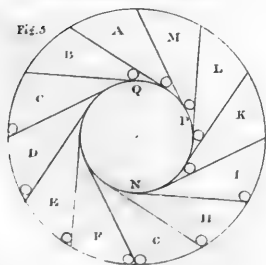
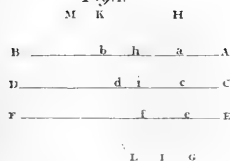




Fig. 1.



Scale equivalent to Gunter's Rule of 29 1/2 Inches long.

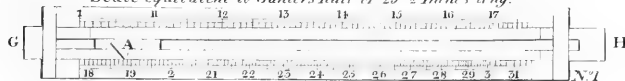
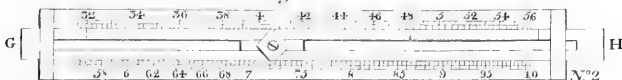


Fig. 2.



*Fig. 3. Spiral Instrument
equivalent to Gunter's Rule
of 40 feet long.*





A
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AND
THE ARTS.

DECEMBER 1797.

ARTICLE I.

Concerning the Spontaneous Action of Concentrated Sulphuric Acid on Vegetable and Animal Substances; its Action upon Alcohol, and the Formation of Ether. By Citizens FOURCROIR and VAUQUELIN.

IT has been long known that the concentrated sulphuric acid destroys the texture of organic matters, by converting them into coal, in consequence of an action which resembles that of an elevated temperature; but this action has not yet been properly explained, so as to determine in what it may consist, or the products it affords. The ancient theory of chemistry attributed this effect, in a loose manner, to the affinity of phlogiston for the sulphuric acid, and was content to compare it to combustion. The authors of the pneumatic theory discerned some of the data of this phenomenon, and arrived somewhat nearer the truth: but they did not comprehend the whole. They have even committed some mistakes, doubtless because they did not study all the details of this remarkable action, nor observe with sufficient care every thing that passes in the subjects of the experiments. My associate Vauquelin has long been employed, in conjunction with myself, upon various methods of analysing vegetables; and having submitted a great number to the energy of the principal re-agents among which the acids occupy the first rank, a long study of the action and curious effects of the concentrated sulphuric acid has led us to discover what had hitherto escaped the observation of chemists respecting its manner of action upon organic substances. Though the result of our enquiries in this respect belongs directly to the vegetable analysis, which we shall offer to the scientific world as soon as our labour shall have acquired all the maturity we are desirous it shall possess, we have nevertheless thought proper to detach this small part, because it appears worthy to interest the cultivators of chemistry, and at the same

time proper in some respects to explain and ameliorate their daily processes, particularly those which relate to several pharmaceutical operations.

When a dry vegetable substance, such as wood, straw, or gum, is plunged in the concentrated sulphuric acid, this matter becomes coloured and soft, appears to dissolve in the acid, and forms with it a magma or kind of brown or black imperfectly fluid matter, well known to every chemist as the product of their experiments. If, after the mutual action of these bodies is exhausted, the mixture be diluted with a sufficient quantity of water, two phenomena equally interesting are observed. The one is the precipitation of a black powder, possessing all the characters of carbone nearly pure; the other is the slight degree of heat which the mixture produces with the water. It is far beneath the temperature which an equal quantity of the sulphuric acid, originally employed, would have exhibited with a like quantity of water.

The vegetable matter is certainly much altered, and has undergone a great decomposition in this experiment; since the carbone which entered into its composition is separated, nearly pure, from the other principles with which it was combined. It is well proved at present, that vegetable substances are composed of carbone, hydrogen, and oxygen united in various proportions, and remaining in a constant equilibrium of composition, so long as new attractions do not come to destroy it. In the present case the equilibrium is manifestly broken, because the carbone is separated from the two other principles. It was at first imagined, and in this manner it is that Citizen Berthollet has particularly determined the cause of this phenomenon in several of his works; namely, that the hydrogen of the vegetable substance united with the oxygen of the sulphuric acid, and, by thus forming water and sulphureous acid, the carbonic principle was separated and thrown down. We have ourselves adopted and admitted the same explanation, until repeated experiments shewed us that this opinion is erroneous. In fact, in the case here mentioned, and observed for a great number of times with the utmost attention and care, we have not perceived that an atom of sulphureous acid was produced, but that the sulphuric acid remains entire without any alteration or change in the proportion of its constituent parts. Since, therefore, this acid is not decomposed in the cold by vegetable matters, it is necessary to conclude, that the changes produced in those bodies are the consequence of a re-action between their proper principles; a re-action of which the sulphuric acid is only the occasional or subsidiary cause.

But in order to determine by what energy this acid produces such an alteration in vegetable matters, it is requisite to examine with accuracy what the alteration may consist in; and this examination, to which we have a great number of times directed our attention, has shewn the error adopted with regard to the pretended decomposition of the acid, and the true cause of the changes thus produced in organic bodies. When the carbonic precipitate of the experiment above described is separated, it is found that the sulphuric acid which floats above it, is singularly weakened, and contains the acid of vinegar, which may be separated by distillation. Here therefore we see the vegetable matter converted into acetic acid and carbone. If the quantity of this last, together with that of the vinegar, be compared with the quantity of vegetable matter employed, a considerable loss is perceived in the mass of these products, compared with that of the substance which afforded them. Nothing can have been lost in the experiment, because no elastic fluid is disengaged; and as the sulphuric acid has lost much of its density, and becomes much less heated with
water,

water, since it is, in a word, extremely diluted, it is evident that the water it has acquired could not have been formed but at the expence of the principles of the vegetable, and that it contains the weight which is wanting to the carbone and acetous acid to represent the whole of the vegetable matter submitted to experiment. For it is very certain that an action perfectly the same, and a result accurately identical, takes place in closed vessels, as well as in such as are open; and that the water which saturates the sulphuric acid does not come from the atmosphere. We are therefore forced to conclude, from this examination, that the vegetable substance treated with the concentrated cold sulphuric acid, has suffered a decomposition or analysis by separating a portion of its carbone nearly pure, and that another part of carbone has united with hydrogen and oxygen to form the acetous acid, at the same time that a second portion of oxygen has united with a sufficient quantity of hydrogen to form the water which dilutes the acid. All these changes in the vegetable substance are therefore made at the expence of its own proper principles. Nothing has come to pass but a change of equilibrium between them, a separation and unequal combination of their component parts, which produce the three new products obtained.

In what manner does the concentrated sulphuric acid determine this change of equilibrium? How can it convert an homogeneous organic matter into acetous acid and coal? What is the force or forces whose energy or concurrence destroys the connection which united the principles of the vegetable substance, while it has no influence on the sulphuric acid? A single argument well applied, from the result of experiments, will lead us to the solution of this problem. Since a vegetable substance requires a new equilibrium of principles after the total action of the concentrated and cold sulphuric acid, and remains in the triple state of carbone, acetous acid, and water, at which period the sulphuric acid has become much less dense and concentrated than before;—we must conclude, that the affinity of this acid for water, as well as that of the oxygen for hydrogen and carbone, at the several proportions required to form vinegar, prevail over the attractions which held these three principles united in other proportions under the original form of homogeneous vegetable matter. But, in this new sum of elective attractions, the only cause which determines the change that takes place is the affinity of the sulphuric acid for water, of which the principles do not unite nor the composition take place, till the moment at which this affinity acts. When once this tendency of the acid for water is satisfied, the equation between the several principles is effected, an equilibrium is established, and no further alteration follows.

It seems, however, at first sight, that the imagination refuses to admit, in the sulphuric acid, an affinity for a body which does not exist completely formed, or to conceive the formation of the water as resulting from the attraction which the acid exerts upon it, though it do not yet exist in the vegetable matter, but in its principles. But whatever repugnance a result of this kind seems at first to produce, it does not appear less certain, since it is evident, as matter of fact in this experiment, that the acid originally concentrated becomes feeble and aqueous; that the water did not exist ready formed in the vegetable matter; that it was not taken from the atmospheric air; and that, when once the acid has become diluted, every subsequent action ceases. These four points are equally established, and there can be no doubt of their certainty. Besides which, a great number of facts are at present well known to chemists, which compel them to admit such affinities as are pro-

ductive of subsequent events, and have been denoted for some years by the name of disposing affinities. Thus it is that the attraction of acids for the metallic oxides favours the decomposition of water by means of the metals, which do not in fact become oxidized to unite to acids, but at the expence of the oxygen which they take from the hydrogen. Thus likewise it is, that the affinity of the caustic earths and alkalis for the sulphuric acid determines the combustion of the sulphur of sulphurets by means of water, and the seizure of oxygen from the latter, which would not take place without the existence of this disposing affinity. When the long consideration of chemical phenomena, the habit of beholding them, and of appreciating their results, has destroyed this first repugnance in the mind of the chemist, he soon conceives that it is not more difficult to admit and explain this disposing affinity, than it is in natural philosophy to adopt, and admit as proved, the attraction at great distances which takes place between the planetary bodies.

We may express the results of the chemical effect as it has here been at length explained by a simple formula, which in reality is nothing more than an argument disposed in a close arrangement, like all the algebraic formulae; and this method, by presenting chemical phenomena in a concise language, will possess the advantage, when more generally employed, of representing, with clearness and precision, every thing that happens in the most complicated operations. Thus we may say,

Aqueous sulphuric acid + liquid acetous acid + precipitated carbone = concentrated sulphuric acid + entire vegetable matter. In this manner it was that Lavoisier exhibited the whole mystery of the vinous fermentation in this simple form of an equation:—Juice of grapes = carbonic acid + alcohol; because, in fact, the result of this interesting observation, formerly so obscure, but at present so perspicuous, is the conversion of must, or liquid saccharine matter, into alcohol and carbonic acid.

If the effects be truly such as we have here explained, it must be necessary for their existence that the sulphuric acid should be concentrated, and the vegetable matters in the dry state: for, if they contained water ready formed, this, by its immediate application to the sulphuric acid, would saturate it, and prevent its re-action upon the principles of these substances. The same thing would happen if the acid employed was already weakened by the previous addition of water. This, in fact, takes place in both cases, as the experience of every chemist must shew. It must, however, be observed in this place, that in order that the action between vegetable matter and sulphuric acid combined with water should be nothing, the water ought to be enough to saturate the acid: for, if its tendency to combination with water be not entirely satisfied, its decomposing action on vegetable substances must be in proportion to its want of saturation. In fact, the decomposition here mentioned, namely the formation of water and vegetable acid with the precipitation of carbone, cannot be obtained but by increasing the quantity of acid. This also is confirmed by experience, which shews that the quantities of sulphuric acid necessary to produce the required decomposition, do not follow the simple ratio of the quantities of water they contain, but are at least in proportion to the squares of those quantities. So that if we suppose, for example, that the most concentrated sulphuric acid contains a quantity of water equal to two, and that another acid contain a quantity equal to four, there will be required not only the double of this last to produce the same effect upon a vegetable substance, but it will be found necessary to use at least sixteen times as much. And this agrees very well with reasoning, since it is known

that the affinity of the sulphuric acid for water diminishes in proportion to the square of the quantity of water it contains.

Though we have reduced the general action of the concentrated sulphuric acid upon vegetable matters to the formation of water, of acetic acid, and the separation of carbone, we must here remark, that in some cases the action becomes more complicated; that sometimes, also, vegetable acids are formed; that the formation of a small quantity of alcohol, even in substances not saccharine, such as gum, unsized paper, &c. likewise occasionally accompanies the foregoing phenomena; and, lastly, that there is in many circumstances a disengagement of carbonated hydrogen gas more or less abundant. But these multiplied products do not take place so frequently as the composition of water, of vinegar, and the precipitation of carbone, the constancy of which is invariable during the action of concentrated sulphuric acid upon vegetable matters. They are in some measure phenomena necessary to the former. They depend upon the different proportion of the principles which compose vegetable substances. They take place more especially in those which contain a greater quantity of hydrogen. The description of these, and the considerations to which they may lead chemists, belong to the general and complete history of vegetable analysis which we propose to publish when our work shall be finished. Lastly, these productions of alcohol, of various acids at the same time, and of carbonated hydrogenous gas, indicate a more profound alteration in the vegetable matters treated by the sulphuric acid; a complication of effects, which, though it originates in the change of equilibrium occasioned by the powerful attraction of this acid for water, cannot be well developed until after the explanation of the new processes of analysis, the methods and results of which we shall hereafter present to the chemical world.

Besides this multiplication of effects and complicated combinations which are sometimes observed in vegetable matters treated with the concentrated sulphuric acid, another alteration sometimes takes place in this last. When the substances placed in contact with it contain too small a proportion of oxygen to form the water required for its saturation, their hydrogen then unites with a portion of the oxygen of the acid itself, and the sulphurous acid is formed. This effect is more particularly remarked in oily substances. It takes place only till the quantity of water necessary for the saturation of the acid is afforded, and ceases as soon as this saturation is effected.

This action of the concentrated sulphuric acid is still more complicated on animal than on vegetable matters, because the animal matters themselves are more compounded. Though the affinity of the acid for water be the cause, the formation of this liquid is not only accompanied with that of a vegetable acid and the precipitation of carbone, but ammoniac is likewise formed. No escape of any volatile principle is perceived. The azote, which forms part of these substances, unites with a portion of hydrogen, and constitutes the volatile alkali, which combines with the sulphuric acid, or with the acid which is at the same time composed, while another part of the hydrogen forms with the oxygen of the same substances that water which is requisite to saturate the sulphuric acid. But in these cases, as in the experiments with vegetable substances, the quantity of carbone, which exceeds that required to compose the new acid, together with a portion of hydrogen and oxygen, will be set at liberty, and falls down in the form of magma, of soot, or of black powder. In this manner it is that in the case of burns, frequently so dangerous, which are produced by the concentrated

sulphuric

fulphuric acid, there is formed, at the expence of the skin, water, vegetable acid, and ammoniac, while carbone is precipitated, and a complete destruction of the burned parts is the result of these simultaneous effects, as appears by the brown or black scar which afterwards falls off, and is separated as a foreign body. Hence may be deduced how great the disorganizing power of this concentrated acid must be on the coats of the stomach or the œsophagus, and the utility of presenting to it, at the moment it is swallowed, a mucous liquid substance, on which its action may be directly and speedily exercised.

From the whole of what is here stated concerning the reciprocal action of concentrated sulphuric acid, and the ternary or quaternary compounds which belong to the vegetable and animal kingdoms, we may perceive that its tendency to destroy these complicated compositions, and convert them into others more simple, must open a new path in the vegetable analysis. We may perceive that, since its effects are confined to those here described, while the substances and the sulphuric acid are permitted to act spontaneously and without foreign assistance, the energy of the acid will proceed much further in this decomposition when it is increased by the addition of new quantities of this fluid, or the application of accumulated heat. But the new results thus produced would carry us from the subject here treated, and to which we at present mean to confine ourselves.

In this accurate description of the spontaneous action of sulphuric acid upon vegetable and animal matters, our intention was to shew more particularly that this knowledge may have a direct influence upon pharmaceutic operations. Our associate Vauquelin has shewn in another memoir, which forms the continuation and useful result of the present, that the principles here explained are alone capable of developing the theory of the formation of ether, hitherto so vague and uncertain; and at the same time to perfect the operations proper to afford these important products. This first work, which we offer jointly to the school of pharmacy, will be a confirmation of the truth consigned in the discourse on the indissoluble union of chemistry and pharmacy, namely, that the former cannot make the slightest advance, without the latter receiving immediately a proportional degree of perfection.

Concerning the Action of the Sulphuric Acid upon Alcohol, and the Formation of Ether.

THE preparation of ether is a complicated pharmaceutic operation, the results of which are no less known than the theory is obscure. This is proved by the different explanations given by the chemists who have treated of this operation before the establishment of the pneumatic doctrine.

The greater number of modern philosophers are in general of opinion, that the whole mechanism of the formation of ether consists in the decomposition of the sulphuric acid, and the transition of part of its oxygene to the elements of the alcohol. According to this hypothesis, water, carbonic acid, and sulphureous acid are formed. This opinion contains the same error as was pointed out in the foregoing memoir. In order to explain the operation with perspicuity and effect, we shall describe its circumstances from the beginning to the end. And while we thus treat on a subject familiar to every chemist, we hope to prove that this operation, so common, and so often repeated, does nevertheless offer, in the series of its phenomena, certain facts which have not yet been properly described, and of which the accurate knowledge alone can afford a solid theory of etherification.

Experiment

Experiment I. Equal parts of concentrated sulphuric acid and rectified alcohol being mixed together, disengage a quantity of caloric capable of elevating the temperature of the mass to 70 degrees (190 Fahrenheit). Bubbles of elastic fluid are formed, the liquor becomes turbid, assumes an opal colour, and at the end of several days becomes of a deep red colour.

Experiment II. A combination of two parts of sulphuric acid and one part of alcohol elevates the temperature to 75 degrees (20. Fahrenheit), becomes immediately of a deep red colour, which changes to black a few days afterwards, and emits a smell perceptibly ethereal.

Experiment III. When we carefully observe what happens in the combination of equal parts of alcohol and concentrated sulphuric acid exposed to the action of caloric in a proper apparatus, the following phenomena are seen:

1. When the temperature is elevated to 78 degrees (208 Fahrenheit), the fluid boils, and emits a vapour which becomes condensed by cold into a colourless, light and odorant liquor, which, from its properties, has received the name of ether. If the operation be properly conducted, no permanent gas is disengaged until about half the alcohol has passed over in the form of ether. Until this period there passes absolutely nothing but ether and a small portion of water, without mixture of sulphureous or of carbonic acid.

2. If the receiver be changed as soon as the sulphureous acid manifests itself, it is observed that no more ether is formed, but the sweet oil of wine, water, and acetous acid, without the disengagement hitherto of a single bubble of carbonic acid gas. When the sulphuric acid constitutes about four-fifths of the mass which remains in the retort, an inflammable gas is disengaged which has the smell of ether, and burns with a white oily flame. This is what the Dutch chemists have called carbonated hydrogen gas, or olefiant gas, because, when mixed with the oxygenated muriatic acid, it forms oil *. At this period the temperature of the fluid contained in the retort is elevated to 88 or 90 degrees (230 or 234 of Fahrenheit).

3. When the sweet oil of wine ceases to flow, if the receiver be again changed, it is found that nothing more passes but sulphureous acid, water, carbonic acid gas; and that the residue in the retort is a black mass, consisting for the most part of sulphuric acid thickened by carbone.

The series of phenomena here exposed will justify the following general inductions:

1. A small quantity of ether is formed spontaneously, and without the assistance of heat, by the combination of two parts of concentrated sulphuric acid and one part of alcohol.

2. As soon as ether is formed, there is a production of water at the same time; and while the first of these compositions takes place, the sulphuric acid undergoes no change in its intimate nature.

3. As soon as the sulphureous acid appears, no more ether is formed, or at least very little; but then there passes the sweet oil of wine, together with water and acetous acid.

4. The sweet oil of wine having ceased to come over, nothing further is obtained but the sulphureous and carbonic acids, and at last sulphur, if the distillation be carried to dryness.

The operation of ether is therefore naturally divided into three periods; the first, in which a small quantity of ether and water are formed without the assistance of heat; the

* Philosophical Journal, I. 44.

second, in which the whole of the ether which can be obtained is disengaged without the accompaniment of sulphureous acid; and the third, in which the sweet oil of wine, the acetic acid, the sulphureous acid, and the carbonic acid are afforded. The three stages have no circumstance common to all, but the continual formation of water, which takes place during the whole of the operation. The principles established in the preceding memoir, and the comparison of the phenomena here described, would in strictness be sufficient to explain how the sulphuric acid acts upon alcohol in the formation of ether; but as this operation includes several particular circumstances which are not comprised in the exposition of the general principles sketched out in the first memoir, we shall in this place enter into some details necessary for the solution of the proposed problem.

The ether which is formed without the assistance of caloric, and the carbene which is separated without decomposition of the sulphuric acid, prove that this acid acts on alcohol in a manner totally different from what has hitherto been supposed. It cannot, in fact, be affirmed, that the acid is altered by the carbene, because daily experience shews that no sensible attraction takes place between these two bodies in the cold; neither can it be affected by the hydrogen, for in that case sulphureous acid would have been formed, and it is known that no trace is exhibited during this first period. We must therefore have recourse to another species of action, namely, the powerful attraction exercised by the sulphuric acid upon water. It is this which determines the union of the principles which exist in the alcohol, and with which the concentrated acid is in contact: but this action is very limited if the acid be small in quantity; for an equation of affinity is soon established, the effect of which is to maintain the mixture in a state of repose.

Since it is proved that ether is formed in the cold by the mixture of any quantities of alcohol and sulphuric acid, it is evident that a mass of alcohol might be completely changed into ether and vegetable acid by using a sufficient abundance of sulphuric acid. It is equally evident that the sulphuric acid would not by this means undergo any other change than that of being diluted with a certain quantity of water. This observation proves, at the same time, that alcohol contains oxygen, because water cannot exist without this principle, which must be afforded by the alcohol only, since the sulphuric acid suffers no decomposition. It likewise shews that the sulphuric acid here, instead of favouring the decomposition of water as in many other cases, does, on the contrary, determine its formation; so that its action on the oxides of carbene and hydrogen, which constitute vegetable matters in general, is absolutely the reverse of that which it exercises upon the simple combustible bodies, sulphur and metals, for example, of which it effects the oxidation by the decomposition of water.

We must not however imagine, from these facts, that ether is alcohol minus oxygen and hydrogen. Its properties alone would contradict this, for a quantity of carbene proportionally greater than that of the hydrogen is at the same time separated. It may in fact be conceived, that the oxygen, which in this case combines with the hydrogen to form water, not only saturates that hydrogen in the alcohol, but likewise the carbene. So that, instead of considering ether as alcohol minus hydrogen and oxygen, we must, by keeping an account of the precipitated carbene and the small quantity of hydrogen contained in the water which is formed, regard it as alcohol plus hydrogen and oxygen. We must observe, that the comparative analysis of alcohol and of ether, of which the details

would

would be out of place in this memoir, correspond perfectly with the result afforded by the theory.

The foregoing are the effects produced by a combination of alcohol and sulphuric acid, spontaneously produced without foreign heat. Let us in the next place observe how this combination is effected when caloric is added. The phenomena are then very different, though some of the results are the same.

In the first place we must observe, that a combination of sulphuric acid and alcohol in equal parts does not boil at less than 78 degrees of temperature, while that of alcohol alone boils at 64. Now since ebullition does not take place till the higher temperature, it is clear that the alcohol is retained by the affinity of the sulphuric acid, which fixes it more considerably. Let us also consider that organic bodies, or their immediate products, exposed to a lively brisk heat, without the possibility of escaping speedily enough from its action, suffer a partial or total decomposition, according to the degree of temperature. Alcohol undergoes this last alteration when passed through an ignited tube of porcelain. By this sudden decomposition it is converted into water, carbonic acid, and carbene. The reason, therefore, why alcohol is not decomposed when it is submitted alone to heat in the ordinary apparatus for distillation, is, that the temperature at which it rises in vapours is not capable of effecting the separation of its principles; but when it is fixed by the sulphuric acid or any other body, the elevated temperature it undergoes without the possibility of disengagement from its combination is sufficient to effect a commencement of decomposition, in which ether and water are formed, and carbene is deposited. Nothing more therefore happens to the alcohol in these circumstances than what takes place in the distillation of every other vegetable matter in which water, oil, acid, and coal are afforded.

Hence it may be conceived that the nature of the products of the decomposition of alcohol must vary according to the different degrees of heat, and this explains why at a certain period no more ether is formed but the sweet oil of wine and acetous acid. In fact, when the greatest quantity of the alcohol has been changed into ether, the mixture becomes more dense, and the heat which it acquires previous to ebullition is more considerable. The affinity of the acid for alcohol being increased, the principles of this acid become separated; so that, on the one hand, its oxygene seizes the hydrogen and forms much water, which is gradually volatilized; while, on the other, the ether retaining a greater quantity of carbene, with which at that temperature it can rise, affords the sweet oil of wine. This last ought, therefore, to be considered as an ether containing an extraordinary portion of carbene, which gives it more density, less volatility, and a lemon-yellow colour.

During the formation of the sweet oil of wine, the quantity of carbene which is precipitated is no longer in the same proportion as during the formation of ether.

What we have here stated concerning the manner in which ether is formed by the simultaneous action of the sulphuric acid and heat, appears so conformable to truth, that nearly the same effects may be produced by a caustic fixed alkali. In this case also a kind of ether and a sweet oil of wine are volatilized, and coal is precipitated. It is therefore only by fixing the alcohol that the sulphuric acid permits the caloric to operate a sort of decomposition. It may also be urged as a proof of this assertion, that the sulphuric acid, which has served to make ether as far as the period at which the sweet oil of wine begins to appear, is capable of saturating the same quantity of alkali as before its mixture with the alcohol.

With regard to the formation of the oil of wine by the augmentation of temperature from the concentration of the sulphuric acid, it is so true, that if water and alcohol be added in the same proportion as the volatile product comes over, oil of wine will never be formed, and all the alcohol will be converted into ether.

It is to the same cause, namely the heat at a higher degree of intensity, that we must attribute the development of carbonated hydrogen gas, or the olefiant gas, which seems to be nothing else but the sweet oil of wine, with a small portion less of oxygen and more of caloric.

CONCLUSION.

From the facts and observations contained in this memoir, it follows:

1. That the formation of ether is not owing, as has been thought, to the immediate action of the principles of the sulphuric acid upon those of the alcohol, but to a true reaction of the elements of the alcohol upon each other, and particularly the oxygen and hydrogen, occasioned merely by the sulphuric acid.
2. That we might in strictness change any quantity whatever of alcohol into ether, without the assistance of heat, by sufficiently increasing the proportion of sulphuric acid.
3. That the operation is divided into two principal stages, in one of which ether and water are only formed; and in the other, the sweet oil of wine, water, and the acetous acid.
4. That so long as ether is formed, the sulphuric acid is not decomposed, and there is no product of sweet oil of wine; but as soon as this last appears, little, if any, ether is afforded, and the sulphuric acid is at the same time decomposed.
5. That the formation of sweet oil of wine may be prevented, by keeping the temperature of the mixture between 75 and 78 degrees; which may be easily done, by suffering a few drops of cold water to fall from time to time into the retort.
6. That alcohol differs from ether by containing more carbone and less of hydrogen and oxygen, and that the sweet oil of wine is to ether nearly what alcohol is to the oil*.

II.

On the Multiplier of Electricity. By the Inventor, Mr. T. CAVALLLO; with Observations.

To Mr. NICHOLSON, Editor of "The Journal of Natural Philosophy, &c."

SIR,

HAVING a few days ago seen for the first time your Journal of Natural Philosophy, Chemistry, &c. I found in the third article of No. 1. an erroneous account of my Multiplier of Electricity, a description of which is contained in the third volume of my Treatise on Electricity; and trusting that your candour will allow a correction of this error to come before the Public, I have taken the liberty to send you the following explanation, with a request that you will be pleased to insert this letter in your next number.

In the first paragraph of the above-mentioned article, you compare an electrical instrument of your contrivance (which you describe in the sequel) with my multiplier, in the following words:—"I then (viz. in the year 1787) mentioned my intention to construct an instrument on that principle, which soon afterwards I did. It was shewn to Sir Joseph Banks and his

* These Memoirs are translated from the *Annales de Chimie*, XXIII. 186.

friends,

friends, at his house, nearly at the same time, and in the same year transferred to the celebrated Mr. Van Marum of Haerlem, who now possesses it. From various other avocations I was prevented from causing any others to be made. It is not therefore wonderful that the same thought should since have occurred to so great a master of the subject as Mr. Cavallo, who, in the third volume of his *Electricity*, published in 1795, gives a description and engravings of an instrument very different in form, but the same in principle, &c."

Now the error I allude to consists in the assertion *that the two instruments are the same in principle*; and I flatter myself that the following explanation will be sufficient to shew that they are so far from it as to be totally different in principle and in effect.

Your instrument does nothing more than collect a considerable quantity of diffused electricity into a small compass, which is exactly the same office as is performed by Mr. Volta's condenser, or by my collector of electricity, which is described in the *Philosophical Transactions* for the year 1788; whereas my multiplier renders an absolutely small quantity of electricity manifest, by accumulating a considerable quantity of the contrary electricity. Or, to be more explicit, your instrument cannot communicate to the electrometer a greater quantity of electricity, or rather not nearly so much of it, as is contained in the electrified body which is to be examined; whereas my multiplier accumulates a quantity of electricity many times greater than that of the body in question, and of course much more perceptible.

Suppose, for instance, that a certain body, having its surface equal to that of the electrometer which belongs to your instrument, contain 100 parts of electricity; and suppose, likewise, that the said electrometer requires 200 such parts of electricity in order to shew any divergency. Now your instrument will be found incapable of manifesting the electricity of the given body; for, when the upper tinfoil segments have conveyed the greatest portion, or we may even say all the 100 parts of electricity from the given body to the hook with the electrometer, they cannot, in the subsequent rotations of the instrument, collect any more. The electrometer, therefore, being at most loaded with 100 parts of electricity, cannot shew any divergency; since, according to the supposition, at least 200 parts of electricity are required in order to produce any divergency in it.

But if the above-mentioned electrified body be applied to the plate A of my multiplier, and the instrument be worked, the plate C, and of course an electrometer which is connected with it, will soon acquire 300 or 400 parts, or, in short, a much greater quantity of electricity than is sufficient to cause a divergency in the electrometer; because, by working the instrument, the original quantity of electricity in the plate A is not diminished, or removed from it; but the contrary electricity (which is repeatedly and unlimitedly induced upon the plate B by the plate A) is accumulated upon the plate C, &c.

Sir, I remain your obedient humble Servant,

Wells-Street, Oct. 30, 1797.

T. CAVALLO.

AFTER returning my sincere thanks to Mr. Cavallo for the detection of the error he mentions, into which I certainly should not have fallen if I had consulted his *Treatise* at the time the paper he alludes to was written, I shall take the present opportunity of making some remarks on the instruments we at present possess for manifesting the presence and kind of weak electricities. These are: the condenser, the simple doubler, the doubler with me-

chanism, two instruments by Mr. Cavallo, and the instrument described at p. 17 of this Journal.

After the discovery of the method of insulation by Stephen Gray, the first form in which compensated electricity was observed appears to have been in the combination called the electric jar; in the explanation of which so many of the earlier electricians have shewn their ingenuity. The next instance consisted in the application of conductors to each other; by which it was shewn, that electricity is not equally diffused over the surface of a conducting body, while any part of the electrified mass which differs in intensity is separated from the rest by the interposition of air or other non-conductors. The cann and chain of Franklin, and the electrical well of Beccaria, exhibited the principle of compensation in another light; and the subject was rendered still more difficult when the strong opposite electricities of ribbands and silk stockings, placed in contact, exposed to friction in that situation, and then separated, were shewn in the experiments of Cigna and Symmer. The vindicating electricity of Beccaria appeared to be still more remote from the simple general laws which had been applied to explain the effect of the jar; and the electrophore of Wilcke presented difficulties scarcely less considerable.

The first process for accumulating electricity, which differs from that of mere friction, appears to have been invented by Professor Lichtenberg*, and also by Dr. Klinckock†, of Prague. It was performed by means of two resinous plates like those of the common electrophore, and one metallic plate with an insulating handle. When one of the resinous plates had been slightly rubbed, it was used to produce the opposite electricity in the metallic plate. This plate was then conveyed to the other resinous plate, and made to deposit its electricity by applying its edge, or otherwise, to the non-conducting surface. The last-mentioned surface could then be used in the ordinary manner to give an electricity to the metallic plate of the same kind as that of the original resinous surface, to which it was again applied by the edge. The electricity of the first resinous plate became thus increased, and, by the first-mentioned process, the metallic plate was returned to the other resinous surface in a higher state of electrization. By a repetition of these manœuvres, the two plates were speedily put into as high a state of electricity as they were capable of retaining.

An accidental circumstance directed the attention of Professor Volta to the advantages which might be derived from the increased electrical capacity of the metallic plate of the electrophore, when placed not upon the resinous plate, but on an imperfectly conducting substance‡. If the last-mentioned substance were a good conductor, it would at some part of its surface be so far in contact with the upper plate as to carry off any electricity communicated to this last; and if it were a non-conductor, it might, and in most cases probably would, possess an electric state of its own which would influence that of the upper plate. But the imperfect conductor will not readily produce the first effect, and is incapable of the latter. It therefore serves only to assume the contrary state of the upper plate whenever electricity is communicated to this last, and renders it susceptible of absorbing a much greater quantity than it else could have done; the nature and intensity of which becomes per-

* Journal de Rozier, January 1780, p. 20.

† Philosophical Transactions, LXVIII. p. 1029.

‡ Journal de Physique for May and August 1783; and also the Philosophical Transactions, Vol. LXXII.

ceptible when the plate is lifted up. Mr. Cavallo improved this instrument, by communicating the electricity of the metallic plate, when raised up, to another plate of the same kind, but much smaller, and in contact with an imperfectly conducting surface. And the Rev. Mr. Bennett adapted the same to his electrometer *. This is the condenser of Volta; and the imperfectly conducting plate may be a piece of dry marble, or a common wooden table, or any kind of plate covered with thin silk.

Mr. Bennett was, I believe, the first who improved the condensers of Volta, by making use of a manipulation similar to that of the Professors Lichtemberg and Klinckock. His instrument, called the doubler, consists of three metallic plates, capable of being applied to each other with their flat surface, but prevented from contact in this situation by a thin coating of varnish. They have insulating handles at the side, and may be brought into actual contact edgewise. Let the three plates be called A, B and C; and the manipulation for increasing the intensity of one of them, for example A, will be as follows:

The plate A being insulated, place B upon it. In this situation communicate electricity to it, while B is touched with the finger. The consequence will be, that A will receive a much larger quantity of electricity than it would else have absorbed if B had not been present.

Remove the communication from A, and the finger from B, and raise the latter by its insulating handle. A and B will then exhibit the opposite electric states more strongly than while together.

Place C upon B, and touch C with the finger. It will assume the opposite state to B, or, in other words, the same state as A.

Place B upon A, as before, touching B with the finger; and at the same time apply C edgewise to A. In this situation, C, being without compensation, will give the greatest part of its electricity to A.

Remove C; take the finger from B, and separate B from A. The opposite electricities will be stronger than before, on account of the increase afforded by C.

Place C upon B again, as in the third stage of the original process, and repeat the subsequent manipulations. In each of them the intensities of A and B will be nearly doubled.

Though this process is simple and evident, yet it requires to be learned, and takes a certain time for the performance. It therefore appeared a desirable object to form an instrument which should by some very simple movement complete this small series of operations. In the month of December 1787 Mr. Partington lent me an instrument contrived by Dr. Darwin, and consisting of four metallic plates, two of which were moveable by wheel-work into positions which required them to be touched with the hand in order to produce the effect. It appeared to me that the whole operation, including the touching, might be done by a simple combination without wheel-work by the direct rotation of a winch. This was soon afterwards effected, and communicated by me to the Royal Society in the year 1788. The instrument is also described in Mr. Bennett's *New Experiments on Electricity*, published in 1789, and in other books.

Mr. Bennett and Mr. Cavallo observed, soon after the discovery of the doubler, that it never fails to exhibit an electric state by the mere operation, without any communication

* Philosophical Transactions, LXXVII. p. 32.

of electricity having been previously made. The former made several valuable observations and experiments to investigate the causes and remove the uncertainty produced by this spontaneous electricity; and the latter presented to the Royal Society * an account of a new instrument called a collector of electricity, in which, as he asserts, this imperfection does not take place. It consists of a plate of tin, supported by two upright sticks of glass; on each side of which plate are two frames of wood, covered with gilt paper, which do not touch the tin plate, but stand parallel to it at a small distance. These frames are fastened to the platform of the instrument by hinges; so that if electricity be communicated to the plate, it will receive a large quantity without any considerable intensity, because its capacity is much augmented by the vicinity of the plane of gilt paper on each side. But if these planes be thrown back into the horizontal position, which is easily done by means of their hinges, the electricity, which before was compensated in the plate, will have its intensity greatly increased. An electrometer connected with this plate will therefore shew signs of electricity by means of a communication made between a large stock of electricity, and the tin plate in its first position, though the intensity of that stock may have been too small to have affected the electrometer without this contrivance.

It does not appear, in the author's description of this instrument, that it removes the equivocal effect of the doubler; for it is evident that it does not, in its simple process, enter the province of the doubler in which this effect takes place. The doubler requires six or seven turns before it will exhibit spontaneous electricity; at which period the first charge is magnified above twelve thousand times: but his simple instrument will scarcely exceed one hundred times, and therefore requires the electricity to be one hundred and twenty times as strong as that which causes the uncertainty of the doubler. Whence it may be inferred, that the doubler would have acted unequivocally with all such electricities as this instrument is capable of exhibiting.

But the spinning instrument, as Mr. Cavallo in the foregoing letter justly observes, does nothing more than collect a considerable quantity of diffused electricity into a small compass, though it does not appear to be exactly the same office as is performed by M. Volta's condenser, or his collector, as described by their respective inventors. For those instruments, if very small, may exhibit very little intensity in conjunction with an electrometer of considerable surface; whereas the spinning instrument, besides its facility in performing the operations, may be considered as possessing a surface indefinitely great. These advantages, such as they are, constitute all the difference I perceive between them.

Mr. Cavallo's multiplier, described in the third volume of his *Complete Treatise on Electricity*, which I had mistakenly compared with the spinning instrument, consists of four metallic plates, A, B, C, and D. The plate A is supported on a stick of glass fixed in the platform of the instrument. The plate B is also supported on a stick of glass which is fixed on a radius or arm, by means of which it can be brought very near and parallel to A, and again removed, so as to touch the third plate C, by means of a wire projecting from B. The plate C is supported, like A, by a stick of glass fixed in the platform. And, lastly, the plate D is fixed by a metallic stem upon a sliding-piece, by means of which it can be brought very near and parallel to C, and occasionally removed.

* *Philosophical Transactions for 1782.*

The operation is this:—Let B be brought as near to A as the construction will admit. In this case it must be remarked that the wire proceeding from B will touch another wire that communicates with the earth. Let electricity be then communicated to A. The plate A will receive about one hundred times as much electricity when B is thus applied at about 1-40th of an inch distance, than it would have otherwise acquired at a like intensity, and B will possess the contrary state nearly equal. Let B then be removed so as to come in contact with C, of which the capacity is no less increased by the vicinity of D. The electricity of B will be shared between itself and C in proportion to their capacities; that is to say, about 99 parts in C, and 1 in B. At the next repetition B will bring 100 parts, which, together with the 99 parts in C, will be shared in the same manner; that is to say, 197 in C, and 2 in B. In this manner, without entering into the consideration of series where the data cannot pretend to minute accuracy, it is clear that the process, indefinitely prolonged, can never produce a greater charge in C than 100 times the quantity conveyed at each single motion by B; that is to say, 100×100 times, or 10,000 times the intensity of the original opposite electricity given to A, provided D be drawn back to leave C in a discharged state.

It has always appeared to me that the doubtful results of the doubler must arise from the natural or spontaneous state of the plates A and B, at their first conjunction. If this charge be too strong to be destroyed by the energy of the communicated electricity, the doubler must in the result shew the same electricity as it would have shewn without such communication, though somewhat later. And if the same circumstance were to obtain in the plates A and B of Mr. Cavallo's last instrument, or any other contrivance operating by a charge, I do not see but that the results must be equally uncertain. If the electricity of B, instead of being accumulated in C of the multiplier, had been employed to produce the contrary state in D, and thence by communication to A, as in the doubler, it does not appear that any uncertainty could follow, provided only that the first effect of the conjunction of A and B had been governed by the communicated electricity. And if this reasoning be true, it will follow from that and the facts, that the uncertainty of the doubler exists also in the condenser, the collector, the spinning instrument, and the multiplier, though the first-mentioned instrument alone is delicate enough to shew it; and that in all electricities which are strong enough to affect any of the latter, the doubler may be used without fear of an equivocal result.

III.

A Memoir on certain Methods of Economy and Improvement in the Manufacture of Hats.
By Citizen CHAUSSIER*.

THE progress of the arts has been long retarded by prejudice, and the servile disposition which attaches itself without enquiry to the ancient practices of business; and more particularly by the spirit of individual interest and suspicion which have prevailed among artists, and induced them to conceal their operations under a kind of mysterious veil. By this means

* Journal Polytechnique, cahier I. p. 160.

the knowledge of the arts was in a great measure confined to those who actually practised them. Error was propagated under the shade of secrecy, at the same time that useful processes and improvements, which fortunate experiments had discovered, were also confined to the limits of individual works, beyond which they were never extended. At present, however, the arts cannot but participate in the salutary reform which has taken place on the soil of equality. Every effort ought to be directed to the same object, and every petty interest ought to be sacrificed to the public welfare. Those mysterious reservations, those practices so carefully concealed under the name of secrets, ought to be disclosed. The means of amelioration and improvement ought not to remain concentrated in any particular workshop. As soon as experience shall have confirmed their efficacy, they ought to become the public property *. These considerations induce me to point out certain processes relative to the art of the hatter, which for some time past have been employed with much success in a large manufactory in the department of La Côte d'Or.

The art of the hatter is well known to consist of a method of forming, with the wool or fur of different animals, a kind of stuff of a dense compact texture, capable of assuming and preserving any figure which may be given to it. To obtain this object a variety of processes are employed, partly chemical and partly mechanical, which may be reduced to four principal operations; namely, felting, fulling, dyeing, and preparation.

I shall not here describe the operation of felting, nor the previous preparations which several kinds of hair demand in order that they may be used in manufactures. This would lead us too far from our present object, and would be merely a repetition of what is generally well known; especially since the publication of the interesting memoir† of Citizen Monge, in the sixth volume of the *Annales de Chimie*.

I shall

* The learned author has been carried a little too far by attending to one side of the question only. No private property can be better grounded than a man's right to the results of his own intellect; and there is none that he can better defend. How far the public may acquire a right of the kind here mentioned, by proposing an adequate equivalent for individual exertions and expence, is a question of great difficulty, whether it be morally or politically considered. N.

† As this memoir is not generally known in England, it will be useful to insert an abstract in this place. *Annales de Chimie*, VI. 300.

1. When a single hair is inspected by the microscope, under whatever magnifying power, it appears smooth and polished: 2. Yet it is certain that the surface is not equally smooth when rubbed in different longitudinal directions, but is composed either of scales like those of fish, or imbricated zones like the horns of animals. For, 3. when a hair is held by the root, and drawn between the finger and thumb, there is little friction, and no noise; but, on the contrary, when it is held by the point, and drawn in the opposite direction, the resistance is more considerable, the motion tremulous, and attended with a chirping noise. And again, 4. If an hair be held between the finger and thumb, and rubbed, by alternately moving them in the direction of its length, a progressive motion will be produced, which is always with the root end foremost. 5. The same structure may be shewn by tying two hairs together, and then giving the knot a few blows between the palms of the hands; for the knot will either untie itself, or draw closer, accordingly as the asperities of the surfaces are placed in the tying. 6. From this mechanism, which is common to wool and every other kind of hair, may be deduced the harsh feel of woollens against the skin compared with linens, and their irritating effect upon wounds. 7. It is the disposition to progressive motion endwise which causes hair to entangle and felt itself together, when pressed by the hatter between two lincn cloths; to which last they do not unite, because its fibres are smooth. 8. Cut hair is better than such as is plucked, because the bulbous roots of the latter prevent the progressive motion. 9. The fibres of wool, being crooked, must naturally move in curve lined directions; but those of the hare, the rabbit,

I shall confine myself to the observation, that a workman, by means of the bow, beats, spreads, and distributes equally in small flocks upon a plane surface the quantity of wool he has selected: that he then covers it with a linen cloth slightly moistened, and by means of a gradual pressure in every direction he connects the different fibres, which, by inter-twining and crossing each other, form a piece of stuff of a soft and spongy texture. Upon this first piece he applies another formed in the same manner, and sometimes a third or fourth, according to the nature of the materials, and the intended thickness and consistence of the work. These different pieces are successively brought together, and disposed in a form suitable to the article * he proposes to fabricate; and, in order to effect the cohesion, he uses a number of pressures and alternate motions in different directions, during which he preserves the suppleness and flexibility of the material by slight aspersions of warm water.

This work, which is entirely mechanical, forms the felt, which is a kind of soft spongy stuff, of greater or less thickness, and in its first state of a loose imperfect texture. The fibres * it is composed of, are yet weakly connected, and would soon disunite again. In order to give the requisite density and consistence, it is necessary that it should undergo the operation of fulling.

This operation, which is in a certain respect the completion of felting, has for its objects the intimate connection of the fibres, and a more perfect and durable cohesion of the whole mass. But for this purpose the mere mechanical act of pressure is insufficient. In this way the result would be a formless mass, without consistence. Experience has long shewn, that for the fulling it is necessary to make use of a bath of water heated nearly to ebullition, into which are put ten or fifteen pounds of lees of wine (*lie*) for each hundred pounds of water. The heat is kept up during the whole time of working, and every three or four hours a new quantity of lees is added.

Into this bath the workmen plunge their felt, and begin their second process. The felt

rabbit, and the beaver, being straight, cannot be used alone in felting till after a previous operation, which consists in rubbing them with a nitrous solution of mercury, by means of a brush, before they are separated from the skin. The solution, acting on one side of the hairs only, renders them crooked. 10. The straight hairs are used for facing hats, in which operation the action must be continued for a determinate time only, otherwise they would pass through and come out on the opposite side. 11. The disposition of wool to felt itself is an impediment to the carding and spinning processes; in which the operators therefore use oil, which diminishes the power of the hairs to act on each other. 12. But at the fulling-mill soap and mangle are used, which carry off the oil, and restore the wool to its former state; in consequence of which the fulling process takes place, and the cloths are rendered narrower, shorter, closer, stronger, and thicker. 13. The balls of hair in the stomachs of certain animals which lick one another are felted by the action of the stomach. 14. To which I may add, that a similar cause is the chief reason why beds of cock and hen feathers are inferior to those of geese feathers. The latter being much straighter do not form the hard flat balls which abound in the former. If a machine were constructed either to cut off the quill part of cock and hen feathers, or to break the stem in three or four places, it would probably render the article of much more value. This last operation might be done by two fluted cylinders. N.

* Felt may be usefully employed in a variety of objects. Some time ago an artist imagined it might be applicable to the soles of shoes, and made several pair. They were of good use in dry weather; but, notwithstanding his having placed a resinous substance between the inner and outer sole, they easily absorbed moisture, and became heavy and unpleasant. But it may be practicable, by a further attention to the subject and new expedients, to give more compactness and less penetrating to the felt. The trial deserves to be made; for which reason I have mentioned it in this place. C.

is dipped in, and immediately again taken out and squeezed bended and rolled, by pressure in different directions, sometimes with the hand defended by leather, and sometimes by means of a roller or other similar instrument. The immersion and working of the felt is repeated, and the operation continued until the stuff is well condensed, and has acquired the requisite solidity.

Such are the processes used in manufactories, and they constitute the whole of what is explained in the *Description des Arts*, and in the new edition of the *Encyclopédie Méthodique*.

Since the operation of fulling is employed to form a dense and compact stuff with the fibres or hairs, and to determine the intimate cohesion of its component parts; and since the mere mechanical operation is not sufficient for this purpose, even with the assistance of a water-bath at the boiling-heat, without the addition of the lees as a necessary condition;—this last must be considered as a chemical solvent, which acts directly on the substance of the hairs themselves, and produces, either by softening or swelling them, an alteration necessary to insure the cohesion of the different fibres of the stuff. But the lees being composed of the mucilaginous and colouring parts which are separated, together with a great quantity of tartar, or the acidulous tartrate of pot-ash, it became necessary to ascertain, in a positive manner, what might be the principle of its action.

The editor of the *Encyclopédie* has not hesitated to affirm, that it is the alkali or pot-ash of the lees which determines the fulling. But in order to shew how erroneous this assertion is, nothing more is necessary than to dip a piece of blue paper into the bath, by which the former becomes instantly red; and if, after several hours work, the state of the bath be again examined, it is found that the acidulous tartrate of pot-ash is partly exhausted, and the workmen soon perceive, from the difficulty of continuing their work, that a new quantity is required to be added. And again, if we consider the sparing solubility of the acidulous tartrate of pot-ash in cold water, it is easily seen why in this process the water must be kept nearly boiling. Whence it is evident that it must act by the portion of acidule it contains.

This first observation induced me to think that the sulphuric acid might be advantageously substituted in the place of the lees; and as twelve pounds of lees are usually added to one hundred of water, I estimated by approximation that one gros of sulphuric acid would be equivalent to at least one pound of the lees, and consequently that twelve gros of sulphuric acid would be sufficient for one hundred pounds of water.

Experiment soon confirmed my conjectures; and after a first trial, which the manufacturer did not consent to make without trepidation, it was ascertained that the use of the sulphuric acid is greatly to be preferred to that of wine-lees; that it is not only much more economical, but still more convenient in the use; and, what is still more important, the health of the workman is not injured by the excess and duration of the heat, the thick vapours, and the disgusting odour which exhales from the bath, particularly when the lees have been altered by mouldiness and putrefaction, which is very common in these manufactories*.

* I cannot avoid mentioning a small inconvenience which was experienced the first time the sulphuric acid was used. A bottle was filled with the quantity requisite for the bath intended to be made use of; and as the acid was poured out from some height without any precaution, some drops were dispersed, which, falling on the hands or clothes of the operator, occasioned spots by corrosion. To avoid this, it is only required that the bottle should be plunged in the bath, and its contents poured out, so as to occasion no scattering of drops. C.

In fact, when the sulphuric acid is employed, it is useless to keep the bath nearly boiling, as was formerly done. A degree of heat of twenty-five or thirty degrees (90° or 100° of Fahrenheit) is sufficient for good fulling. The saving of fuel is an object of importance in manufactories; and as very little fire is necessary when sulphuric acid is used, cauldrons of lead may be substituted instead of copper boilers, the first cost and annual repair of which are very considerable.

The felts prepared by the new process are also of a very superior quality to those which have been worked in the bath with wine-lees. In fact, the mucilaginous and colouring matters of the lees, which are suspended in the bath, penetrate the texture of the stuff, and adhere with more or less force; and when after having passed the hats through the dye they are beaten, a fine black dust flies off in great abundance, which not only weakens the texture of the felt, but by diffusing itself through the manufactory greatly incommodes the workmen, and frequently occasions coughs and disorders of the throat.

The manufacturer, accordingly, who made this first trial, continues to employ it in preference to the old process; and, according to his report, the advantages are incalculable: "Hats felted in this manner," says he, in one of his letters, "are not only clear of the powder which abounds in the others, but they take the dye better, *ils s'ejarrent mieux*, and are cleaner. The workman has not his bath so hot, which is to him a convenience no less valuable than the saving of fuel to the master.

Besides these advantages, the wine-lees formerly made use of by the hatters in large quantities will remain for other uses, in the fabrication of saltpetre, soft soaps, and for other desirable purposes*.

When I had ascertained that the fulling of hats was principally determined by the portion of acid which exists in the lees, I determined to substitute the oil of vitriol, or sulphuric acid, because it is found in the market at a reasonable rate. But though all the acids have common properties in which they resemble each other, they have also others by which each is peculiarly distinguished. It would therefore be of importance to examine, among the numerous class of acids, whether some one might not have a more direct action upon the stuff than any of the others, so as to shorten the labour, without prejudice to the article or the economy of its preparation. Could not acid liquors be prepared in the manufactory itself, by fermenting barley, or bran, or some such material? This is an object of research, to which we may invite manufacturers, and others who are attached to the cultivation of the arts.

I shall not here describe the processes of dyeing. In the manufactory of La Côte d'Or, the nut-gall is not used; and oak-bark has been substituted with advantage: but these methods are known, and have been already published. I proceed to the fourth and last operation of hat-making.

It consists in lining the inner surface of the crown, as well as of the brim of the hat, with a glutinous substance, which in drying gives firmness to the work, and preserves its form. The usual composition is made of gum arabic, common gum, and Flanders glue, which are

* The sulphuric acid, more or less diluted with water, might be advantageously used in many of the arts. Thus the braziers usually employ lees to clean and brighten their work; and most certainly they would obtain the same effect by simply dipping the piece in water slightly acidulated with sulphuric acid, which would also save them the trouble of rubbing the work for a long time, as they are obliged to do when they use lees, &c. C.

dissolved together in a sufficient quantity of water, and brought to the requisite thickness by boiling.

This preparation, simple and easy as it appears, is not indifferent with regard to the beauty and duration of the work. If it be too tenacious, it renders the stuff dry and brittle, and after some months' use, a kind of greyish incrustation is formed on the surface, which alters the texture. It appeared to me, that this effect was caused by the gum arabic which is added to the glue. I therefore sought among the plants of our own country for a simple preparation, which might be substituted instead of these natural and friable gums. The mucilaginous principle abounds in a great number of plants, and may be easily extracted by ebullition; and a gum may even be formed by evaporation, which preserves its suppleness and flexibility. These considerations induced me to recommend, instead of the usual preparation, a solution of gluc in a decoction loaded with the mucilage of linseed-oil. This preparation has long been used with economy in the manufactory, and advantage in the excellence of the work.

Since that time Citizen Margueron having communicated to me his observations on the mucilage which may be extracted from the leaves of the horse-chestnut-tree (*marronier d'Inde*), and having ascertained how great a portion of mucous and adhesive matter these leaves afford, especially when the foliation is in its vigour; a strong decoction of these leaves has been used with much success to make the preparation with gluc.

There are a great many other native plants, which would be equally proper to afford fictitious gums, and of which the use would be very advantageous in the arts. We are at present busied upon this object, and hope in the course of time to present the results of our researches.

P. S. Since this memoir was written, a philosopher attached to the arts, and who has observed much, has informed me that oil of vitriol or sulphuric acid is used in some foreign manufactories in the fulling of hats, and that the process is considered as a great secret. I was ignorant of this. The publications I have examined make no mention of it, and the workmen I have consulted had no knowledge of its use. Besides which, truth is always new while it continues unknown, and useful observations require to be repeated in order to make them generally adopted.

IV.

Doubts concerning the Existence of a new Earth in the Mineral from New South Wales examined by WEDGWOOD in the Year 1790.

A PORTION of the same mineral from Sydney Cove in New South Wales as was formerly analysed by the late Mr. Wedgwood, and had been presented to M. Haidinger, Counsellor of Mines, by Sir J. Banks, has been lately examined by the celebrated M. Klaproth*. As the result seems to have inclined this able chemist to reject the new earth of Wedgwood, the subject appears sufficiently interesting to admit of being stated to the public.

* Beytrage zur Chemischen Kenntniss der Mineral. Körper, b. ii.

M. Klaproth had two samples; the first consisting for the most part of black brilliant scales, which might have been taken for plumbago or carburet of iron, but appeared to him to be ferruginous mica; the second, which contained much less of this substance, was considered by him as purer, and subjected to experiment.

Thirty grains were reduced to fine powder. The greyish colour it naturally possessed became bluish by levigation. Upon this powder the concentrated muriatic acid was boiled, and decanted off when cold. New acid was boiled upon the residue, and this operation was three times repeated.

The acid after filtration through double paper was not rendered turbid by water gradually added in sufficient quantity. The mixture, when exposed to heat, preserved its transparency.

By saturating the acid with carbonate of pot-ash, a small quantity of precipitate was afforded in light flocks, which, being collected on the filtre,edulcorated and dried, weighed 3.25 grains. This precipitate being added to diluted sulphuric acid, afforded crystals of alum, and left a small portion of siliceous earth.

The residue, which was not acted upon by the muriatic acid, was treated in the dry way with three parts of pot-ash, and afterwards with the muriatic acid. The gelatinous insoluble portion, separated by the filtre,edulcorated and dried, weighed 19.5 grains. It was siliceous.

The muriatic solution, effayed by prussic acid, afforded a blue precipitate, corresponding with about 0.25 grains of iron.

From this solution, when decomposed by carbonate of pot-ash, a quantity of alumine was separated, which, whenedulcorated, and dried by heat, weighed 8.5 grains, and upon being combined with sulphuric acid was totally converted into alum.

The whole of what this chemist obtained from the sand of Sydney Cove was alumine, siliceous, and iron, without the least indication of any other principle. Though he operated upon thirty grains only, and it was impossible for him to repeat his analysis, he observes that the result is sufficient to render the presence of a new earth doubtful. He refers the solution of this doubt to time; and on the supposition of error in Mr. Wedgwood's experiments, he is disposed to think he did not filtre his acid solution before he added water to it, and that the earth deposited by the diluted acid was probably nothing but siliceous united to alumine.

Previous to a discussion of the probability how far Wedgwood could be mistaken in his facts and deductions, and on what the difference between his results and those of M. Klaproth might depend, I had purposed to give an abridgment of the paper of the former chemist from the eightieth volume of the Philosophical Transactions. But after having made the same, and begun my comparative remarks, I found that so much of these last would depend upon the strict statement of experiments, that I determined to give so much of the entire paper as relates to the new earth. And this I do the more willingly, as it is not, as I am aware, to be found in any work of general circulation. What follows, therefore, is in the words of Mr. Wedgwood*.

Analytical Experiments on a Mineral from Sidney Cove in New South-Wales.

THIS mineral is a mixture of fine white sand, a soft white earth, some colourless micaceous particles, and a few black ones, resembling black mica or black lead, partly loose; or detached from one another, and partly cohering together in little friable lumps.

* Phil. Trans. LXXX. 307.

None of these substances seem to be at all acted upon by the nitrous acid, concentrated or diluted, nor by oil of vitriol diluted with about equal its measure of water. In the cold, or in a boiling heat, the mineral remained unaltered in its appearance, and the acids had extracted nothing from it that could be precipitated by alkali.

Oil of vitriol boiled upon the mineral to dryness, as in the process of making alum from clay, produced no apparent change in it; but a lixivium made from this dry mass with water, on being saturated with alkali, became somewhat turbid, and deposited, exceeding slowly, a white earth in a gelatinous state, too small in quantity for any particular examination, but which, from its aspect, from the manner in which it was obtained, and from the taste of the lixivium before the addition of the alkali, was judged to be the aluminous earth.

The marine acid during digestion seemed to have as little action as the other two; but on pouring in some water, with a view only to dilute and wash out only the remaining part of the acid, a remarkable difference presented itself; the liquor became instantly white as milk, with a fine white curdly substance intermixed; the strong acid having extracted something which the simple dilution with water precipitated.

The white matter being washed off, more spirit of salt was added to the remainder, and the digestion repeated with a long tube inserted into the mouth of the glass, so as nearly to prevent evaporation. The acid, when cold, and settled fine, was poured off clear, and on diluting it with water the same milky appearance was produced as at first.

The digestion was repeated several times successively with fresh quantities of the acid, till no milkiness appeared on dilution. The quantity of mineral employed was 24 grains, and the residuum, after the operations, washed and dried, weighed somewhat more than 19 grains, so that about one-fifth of it had been dissolved. In some parcels of the mineral, taken up promiscuously, the proportion of soluble matter was much less, and in none greater. It is only the white part, and only a portion of this, that the acid appears to act upon; the white sand, much of the white soft earth, and all the black particles remain unaltered.

To try whether this tedious process of solution could be expedited by triture or calcination, some of the mineral was rubbed in a mortar; and in doing this, it appeared pretty remarkable, that though the black part bore but an inconsiderable proportion to the rest, yet the whiteness of the other was soon covered and suppressed by it; the whole becoming an uniformly black, shining, soft, unctuous mass, like black lead, rubbed in the same manner with a few gritty particles, perceptible on pressing hard with the pestle. A pennyweight of this mixed, spread thin on the bottom of a porcelain vessel, was calcined about an hour, with a fire between 30 and 40 degrees *; it became of an uniform, dull, white or grey colour, excepting a very few, and very small, sparkling, black particles, suspected to be those which had eluded the action of the pestle; it lost in weight six grains, or one-fourth.

The mineral thus ground and calcined was found to be just as difficult of solution as in its crude state, with this additional disadvantage, that the undissolved fine particles are indisposed to settle from the liquor.

* By degrees of fire or heat *above ignition*, I mean those of my thermometer; and some idea may be formed of their value by recollecting that they commence at visible redness; and that the extreme heat of a good air-furnace of the common construction is 160° or a little more. W.

In all the experiments of dissolution, as often as the heat was at or near the boiling point of the acid, frequent and pretty singular bursts or explosions happened, though the matter lay very thin in a broad-bottomed glass. They were sometimes so considerable as to throw off a porcelain cup with which the glass was covered, and once to shatter the glass in pieces. In a heat a little below this, the extraction seemed to be equally complete, though more slow; but a heat a little below that in which wax melts, or below 140° of Fahrenheit's thermometer, appeared insufficient.

To determine the degree of solution necessary for the precipitation of the dissolved substance, and whether the precipitation by water be total, a measure of the solution was poured into a large glass, and the same measure of water added repeatedly. The third addition of water occasioned a slight milkiness, which increased more and more to the sixth. The liquor being then filtered off, another measure of water produced a little fresh milkiness, and an eighth rather increased it; a ninth and a tenth had no effect. The liquor being now again passed through a filter, solution of salt of tartar did not in the least alter its transparency; so that after the solution has been diluted with eight or nine times its measure of water, there is nothing left in it that alkali can precipitate.

From the manner in which the solution is necessarily prepared, it cannot but contain a great redundancy of acid; for the small quantity of acid sufficient for holding the soluble part suspended, would be foked up or entangled by the undissolved part, so as scarcely to admit of any being poured off; and it cannot be diluted or washed out but by the strong acid itself. The solution with which the above experiment was made, was reckoned to have only about six grains of the soluble matter to three ounces of spirit of salt, having been prepared by digesting that quantity of the spirit, by half an ounce at a time, on thirty grains of the crude mineral.

A saturated solution was obtained by digesting, on a small portion of the solutions thus prepared, the precipitate thrown down by water from the larger portions till the acid would take up no more. A solution thus saturated cannot bear the smallest quantity of water; a single drop, on the first contact, producing a milky circle round it.

Examination of the above Substance, extracted from the Mineral by Marine Acid, and precipitated by Water.

THIS substance washed and dried is indissoluble in water, as indeed might be expected from the manner of its preparation.

Nor is it acted upon by the nitrous or vitriolic acids, concentrated or diluted, cold or hot, nor by alkaline solutions, mild or caustic, of the volatile or fixed kind.

It is dissolved by strong marine acid, but not without the assistance of nearly the same degree of heat that is necessary for its extraction from the mineral. From this solution it is precipitated by water; and after repeated dissolutions and precipitations it appears to have suffered no decomposition or change.

Spirit of nitre, added to the saturated solution, makes no precipitation; and if the quantity of nitrous acid exceeds, or at least does not fall much short of, that of marine acid in the solution, the mixture suffers no precipitation from water. Nor does any precipitation happen,

happen, though the nitrous spirit be previously mixed with even a large quantity of water, provided the quantity of solution added to it does not exceed that of the nitrous spirit in the mixture. The appropriate menstruum for this substance (that is, for keeping it in a state of dilute solution) appears, therefore, to be aqua regia; and the due proportion of the two acids, of any given strength, might be determined, if necessary, with greater accuracy and facility for this than for any other body I know of; because, if there be even a very minute surplus of marine acid in the solution, that surplus will instantly betray itself, on dropping a little into water; all that was dissolved by it, and no more, being precipitated by the water. It may be observed, however, that where an addition of nitrous acid is used, a saturated solution cannot be obtained (unless by subsequent evaporation), the same quantity of marine acid being necessary with as without that addition: the change or modification which the nitrous acid produces in the marine, serves, in the present instance, not for effecting the solution, as in the case of gold and some other metals, but merely for enabling it to bear water without depositing its contents.

Oil of vitriol dropped into the saturated marine solution, occasions no change till its quantity comes to be about equal to that of the solution; a considerable effervescence and heat are then produced, the liquor becomes milky, and the marine acid is extricated in its usual white fumes. The mixture, heated nearly to boiling, becomes transparent, and afterwards continues so in the cold. This vitriolic solution is precipitated by water, and the precipitate is redissolved by marine acid.

The saturated marine solution is indisposed to crystallize. By continued evaporation in gentle heat it becomes thick and butyraceous, and in this state it soon liquefies again on exposure to the air. The butyraceous mass, in colour whitish or pale yellow, is not corrosive, like the similar preparations made from some metallic bodies, nor is it more pungent in taste, but rather less so, than the combination of the same acid with calcareous earth. In a heat increased nearly to ignition, the acid is disengaged, and rises in white fumes, which, received in a cold phial, condense into colourless drops, without any appearance of sublimate. From the remaining white mass, spirit of nitre extracts so little as to exhibit only a slight milkiness on adding alkali; a proof that nearly all the marine acid had been expelled; for, while that acid remains, the whole is dissoluble by the nitrous.

The substance in question is not precipitated by Prussian lixivium. A drop or two of the lixivium do indeed occasion a little white or blueish white precipitation in the saturated marine solution; but in the more dilute no turbidness appears till the quantity of lixivium is such as to produce that effect by its mere water; and when the precipitate has at length been formed, it redissolves in marine acid as easily as that made by water; whereas the precipitates resulting from the union of the Prussian matter are not acted upon by acids till that matter has been extracted from them by an alkali. For further satisfaction, in this important point, the experiment was repeated with a solution in aqua regia. Here the Prussian lixivium, in whatever quantity it was added, occasioned no precipitation at all (only the usual bluishness arising from the iron always found in the common acids); and pure alkali added afterwards precipitated the original white substance unchanged.

The following experiments of precipitation by alkalis were made with the marine solution before the effect of an addition of nitrous acid had been discovered; and they were made with so much care and attention, that it was not thought necessary to repeat them afterwards.

afterwards. To obviate as much as possible the equivocal results that might arise from water contained in the precipitants, the different alkalis were applied in the driest state I could reduce them to; viz. pure salt of tartar, kept for some time in a heat just below redness; crystals of marine alkali melted and dried in the same manner; volatile alkali in crystals, a little surplus acid being in this instance previously added to the solution to counteract the water of crystallization in the alkali; *salt of tartar causticated* by quicklime, and hastily evaporated to dryness; the *marine alkali causticated* in like manner; and the vapour of caustic volatile alkali, arising with a very gentle heat from a retort into a phial containing the solution. All these alkalis occasioned copious precipitations. All the precipitates, after washing and drying, were found to redissolve in marine acid; and from all these solutions the original substance was precipitated unaltered on diluting them with water.

In strong fire, from 142 to 156 degrees, this substance discovers a much greater fusibility than any of the known simple earths. In a small vessel made of tobacco-pipe clay, it melted, and glazed the bottom; and on a bed of powdered flint, pressed smooth in the manner of a cupel, it did the same. Magnesia or chalk would indeed vitrify in the clay vessel; but on flint no one of the known earths shews any tendency to vitrification in that heat*. In a cavity scooped in a lump of chalk, this substance, in the heat above mentioned, ran into a small round bead, smooth, whitish, and opaque, not in the least adhering to the calcareous mass. On a bed of powdered quick-lime, it formed a brownish scoria, which in great part had sunk into the lime, and seemed to have united with it. On Mr. Henry's magnesia, uncalcined, it melted and sunk in completely, leaving only a slight brownish stain on the surface where it had lain. On beds of the baro-selenite, and barytic quick-lime, it likewise melted and sunk in, leaving a discoloured spot behind; but whether it really united with the substrata, or only penetrated into their interstices, could not be determined with certainty, on account of the smallness of the quantity of the mineral I had to work upon.

On a bed of powdered charcoal, in a crucible closely luted, this substance likewise melted: and therefore it may be presumed not to have owed its fusion in the above experiments to the same cause to which some of the common simple earths, in certain circumstances, owe theirs, namely their union with the matter of the vessel or support; that is, with an earth or earths of a different kind from themselves; but to possess a fusibility strictly its own, which takes place in a fire of 150 degrees, or perhaps less.

As charcoal in fine powder assumes a kind of fluidity in the fire, similar to that which powdered gypsum exhibits in a small heat, its surface had changed from concave to horizontal, and the bead had sunk to the bottom; it was rough and black on the outside, and whitish within. On repeating the experiments in a cavity scooped in a piece of charcoal,

* It may be proper just to mention that I find this to be a very commodious and sure method of trying, in small, whether any given earthy body be fusible with other earths. If the body is disposed to vitrify with any proportion of clay or flint, for instance, it will equally vitrify when a little of it is applied, or even dusted only, on the bottom of a small cup made of clay, or on a smooth close bed of finely powdered flint. The body, in this mode of application, seems to unite with only just so much of the matter of the substratum as is requisite for their most perfect fusion together, and has nothing else in contact with it, so that no deception can arise; whereas, if mixed with the same matter, there might be no appearance of fusion, unless certain favourable proportions of the two should chance to be hit upon; and even then, if the quantity be small, it would not be certain but that the fusion might have originated from the matter of the crucible. W.

the result was a blackish bead like the former, only smooth on the outside, with something of metallic brightness, not unlike that of black-lead. Both beads were very light, and had a considerable cavity within. All the internal part was whitish, without the least metallic aspect; and the external glossy blackness appeared to be only the stain which charcoal powder communicates in strong fire to some earthy bodies that have a tendency to vitrify. By boiling in concentrated marine acid, a part of the beads was dissolved, precipitable, as at first, by water; but an accident prevented the process from being continued sufficiently to determine whether the whole could be dissolved or not.

By this solubility in the fire, solubility in one only of the common mineral acids, and parting with the acid in a heat below ignition, precipitability by water, and non-precipitability by Prussian lixivium, this substance is strongly discriminated from all the known earths and metallic calces. And as it suffers no decomposition from any of the alkalis in any of the usual modes of application, I presume it cannot be considered as a combination of any of those earths or calces with any of the known acids; for all the combinations of this kind would, in one or other of the above methods of trial, have had the earth or metal disengaged from the acid.

Whether this substance belongs to the *earthy* or *metallic* class, I cannot absolutely determine; but am inclined to refer it to the earthy; because, though brought into perfect fusion in contact with inflammable matter and in close vessels, it does not assume the appearance which metallic bodies do in that circumstance.*

The black substance, which seems to have composed about one-fifth part of the crude mineral, was found to resemble plumbago in its leading properties, but its residue did not appear to be iron. The remaining three-fifths of the mineral, which resisted the humid attacks in Mr. Wedgwood's experiments, was probably siliceous; but he does not speak of any direct examination of its properties by fusion with alkalis, the sparry acid, or otherwise.

On the preceding experiments of M. Klaproth and the late Mr. Wedgwood, the question which in the first place appears to demand solution, is, whether the same mineral was examined by both chemists? If this should not appear probable, it will be unnecessary to enter upon the subsequent enquiry, which set of experiments is most likely to be erroneous?

On the historical evidence, as far as regards the sameness of the parcels of mineral, I think there is little to be said. Both were obtained from Sir Joseph Banks; the first directly, and the latter with the intervention of the respectable, and as it may be concluded careful, M. Haidinger. Nevertheless, if the obvious description and decided experiments should point out a difference, it will afford more than a presumptive proof that they were not the same.

1. M. Klaproth's mineral was greyish, and became bluish by levigation. Mr. Wedgwood's was a fine white sand, a soft white earth, with some colourless micaceous particles, and a few resembling plumbago. When levigated, it became black, shining, soft, and unctuous.
2. Klaproth's marine solution, after filtration, afforded no precipitate by the addition of water. Wedgwood's solution in the same acid, when cold, settled fine; and, decanted off clear, afforded a white precipitate with water to the whole amount of its contents. After six measures of water had caused much of the precipitate to fall, the liquor was filtered,

* Here ends the extract in Mr. Wedgwood's own words.

and more water threw down a little more. 3. Klaproth's marine solution afforded a precipitate by the addition of an alkali which was soluble in vitriolic acid, and afforded alum. Wedgwood's precipitate by means of water was insoluble in the nitrous or vitriolic acids. The indirect solution of this matter in vitriolic acid was precipitated by water. 4. M. Klaproth's experiments shewed nothing but filix, alumine, and a minute portion of iron. He thinks it probable that Wedgwood's precipitate by water was filix combined with alumine. But in Wedgwood's experiments this matter was fused without addition in an hole scooped in charcoal, and also in an hole in a lump of chalk, to which it did not adhere. Kirwan * found equal parts of alumine and filix infusible at 160° of Wedgwood; and Achard found them infusible in a porcelain furnace, in all proportions; not to mention that very refractory vessels are made out of the same two earths mixed together. 5. I do not see how Wedgwood's method of subsidence and decantation could have left any thing suspended which Klaproth's filtre could have detained.

Hence it seems fair to conclude that the two minerals were not the same, however this may have happened; and that the existence of the new fusible earth of Wedgwood stands on the same evidence as before, namely, his experiments, which have not yet been repeated, that I know of.

V.

A Philosophical Memoir, containing—1. Experiments relative to the Propagation of Sound in different Solid and Fluid Mediums.—And 2. An Experimental Enquiry into the Cause of the Resonance of Musical Instruments. By M. PERROLE †. With Annotations.

AS the transmission of sound through water ‡, through air of greater or less density §, and through different gaseous substances ||, has added to the amount of our philosophical knowledge, I have thought an abundant harvest of new facts might be obtained by causing sound to pass through a great number of bodies of different kinds, as well solid as fluid, and comparing their effects together.

These are the views which have directed the experiments of which I shall give an account in the first part of this memoir; and in the second I shall avail myself of the results in an endeavour to ascertain the cause of the resonance of bodies.

P A R T I.

AS all the trials depend upon the following experiment, it is necessary to give particular attention to its detail.

Experiment I. and chief. Close the ears with chewed or mashed paper; suspend a watch by an hook. Place the ear at the distance of two lines from the watch, and you will not hear its

* Mineralogy, i. 58.

† Mem. de l'Acad. Royale de Turin, v. 195.

‡ Nolle, Mem. de l'Acad. Royale de Paris, 1743.

§ Muschenbroek, n. 1442.—Nolle, Leçons de Physique, iii. 355.

|| Priestley's Experiments and Observations, &c. and my Experiments in the Turin Memoirs for 1786, 1787. P.

vibrations. Then take a solid body, such as a small cylinder of wood, of one foot or one foot and a half in length, and one or two lines in diameter. Place it in contact at one extremity with the watch, and apply the other end to one of the numerous parts of the head which propagate sound by the touch*; for example, with the cartilaginous parts of the ear; you will hear the sound much better than if the ear had not been closed, and the sonorous body had been placed in the air at the smallest distance from the organ.

As the sound was not perceptible at the distance of two lines in the first disposition, and was very strongly perceived in the second; it is evident that the small cylinder propagated sound better than atmospheric air.

When we reflect on this experiment, and the result it affords, we may, without difficulty, perceive, that, in order to ascertain the respective force of the propagation of sound through solid bodies, nothing more is required to be done than to procure substances of different natures, to give them the same form, and subject them to an experiment of this kind. In this manner I made the following experiments:

Experiment II. I procured small cylinders of the dry wood of fir, oak, box, cherry-tree, chestnut-tree, and logwood, each one line in diameter, and one foot in length. The ears, being then closed, I placed them successively in contact with the watch, and at the same time with the cartilaginous part of the ear, as in the preceding experiment.

The different cylinders transmitted the sound very well; but its tone† seemed to vary whenever a new cylinder was used, and the intensity appeared to be never exactly the same. We had no means of ascertaining the difference of the tone (*timbre*), but the intensity appeared in the following order, beginning with those cylinders which appeared to propagate sound with the greatest activity: fir, logwood, box, oak, cherry-tree, chestnut.

Experiment III. I determined to extend my researches to the metals, and constructed metallic cylinders similar to the former. When subjected to the same trial, they transmitted the sound in general rather worse than the cylinders of wood.

The nature of the sound appeared likewise to differ in the cylinders of wood and those of metal. The tone was not exactly the same in the different metals, and the intensity followed this order: iron, copper, silver, gold, tin, lead.

Experiment IV. I hung my watch successively to strings of silk, of wool, of hemp, of flax, hair, and gut, which were nearly of the same diameter and exactly of the same length as the solid cylinders. One extremity of the string was applied by the hand in contact with the cartilage of the ear, while the watch rested on the opposite extremity of the string, which touched no part of the body. The strings thus extended propagated the sound with less

* Almost every part of the head propagates sound, when in immediate contact with a sonorous body. This may be easily proved by applying a watch to the several parts after having closed the ears. See my *Dissert. Anat. &c.* and my *Recherches sur l'Organe de l'Ouïe et la Propr. des Sons*, tom. iii. des *Mém. de la Soc. R. de Méd. et le Journal de Physique*, 1773, tom. ii. P.

† *Son timbre*, which does not, as I apprehend, imply any difference of acuteness or gravity in sound, but another relation for which I know no English word but tone. Thus the tone (*timbre*) of the hautboy differs greatly from that of the flute when both are in unison, and so likewise do the different tones (*tons*) of the same kind of instrument. It does not appear from the text, whether the acuteness or gravity of these transmitted sounds did differ from each other. N.

force than solid bodies, and modified it in a remarkable manner. The tone in each of the strings appeared to be different, and the intensity followed this order:—gut, hair, silk, hemp, flax, wool, cotton.

From the foregoing experiments it follows:—1. That hard bodies and stretched strings transmit sound much better than atmospheric air. 2. That each of these mediums transmits it in a manner which is peculiar to itself, so that the kind and intensity of the sound are never exactly the same, as far as can be judged on trials which do not always present very striking results*. 3. That in general wood transmits sound very well; that the metals transmit it with somewhat less energy, and that stretched strings occupy the third place in the scale of power in this respect.

Experiment V. Being determined to extend my researches, I caused the sound of the watch to pass through pieces of zinc, antimony, glass, sal gem, gypsum, dried clay, and marble. As I could not give the same form to these different substances, I was unable to determine with precision their respective powers of transmission; but I observed that all these bodies transmitted sounds better than air, and that it was modified in a particular manner by each of them. Marble was remarkable for the little force with which it transmitted the sonorous movements. Two pieces of this substance, of different form and size, propagated it in a weak and almost insensible manner.

These are the experiments I have made with solid bodies. To complete the series, it remained to submit fluids to similar trials.

I have already published my experiments on æriform substances†. In this place I shall relate those I have made upon liquids. This part of my operations required a different process:

Experiment VI. I closed all the joints of my watch with soft wax, and then suspended it by a silk thread. In this state I hung it by an iron branch fixed in the wall, so that the watch remained suspended in the middle of a glass vessel five inches in diameter, and seven inches high, taking care that neither the watch nor the thread touched the vessel in any part. I remarked the kind of sound afforded by the watch, and the distance at which I ceased to hear it. After having marked this point, I then filled the vessel with water, into which I again suffered the watch to descend, with the same precaution of not suffering it or the thread to touch the vessel.

The tone (*timbre*) was changed in the water in a striking manner. The sound was propagated in so lively a manner that the glass, and a small table of wood, on which it stood at a distance from the wall, seemed to undergo direct percussions from a solid body. But what appeared still more astonishing was, that in the midst of all these agitations the fluid in which the watch was plunged was perfectly tranquil, and its surface not in the slightest degree agitated.

By substituting different liquids in the place of the water, I had results in general analogous to those I had obtained with that fluid; but each medium gave a different modification to the sound, of which the intensity was noted as follows:

* Such chiefly are those which were obtained with the cylinders of wood, and stretched strings. P.

† *Mém. de l'Acad. Roy. des Sc. de Turin*, 1786, 1787.

Intensity of Sound observed in different Fluids.

	Fect.
1. In the air serving as the term of comparison, it ceases to be heard at the distance of _____	8
2. In the water, at that of _____	20
3. Oil-olive _____	16
4. Oil of turpentine _____	14
5. Spirit of wine _____	21

It is proper to observe, that on repeating these trials I observed some variations in the intensity, which appeared to depend on the organ of sense or accidental noises.

From the experiments made upon liquids, it follows:—1. That these as well as solids do transmit sounds much better than air, and that even the fat oils are not to be excepted.*.

2. That each fluid upon trial is found to modify the sound in a peculiar manner.

3. Philosophers maintain the opinion that sound is propagated in the air by means of certain motions or undulations, which the transparence of that fluid prevents our seeing. My experiments with fluids which do not elude the sight, and in which no motion was perceived, notwithstanding the very effectual transmission of sound, may render this in some respect doubtful.

4. Lastly, the experiments on solids, fluids, and those I have published on the gases †, afford the probable conclusion that all mediums produce particular modifications in the tone (*timbre*) and the intensity of sound; or otherwise, that the same sound varies as often as it passes through a different medium. I shall now pass to the experiments which constitute the second part of this memoir.

P A R T II.

EVERY one has observed, that if a watch be placed upon a table, the sound is very evidently increased. The difference between the sound of a tuning-fork, when it vibrates without being placed in contact with any solid body, and that which it affords when its handle is pressed against a solid body with a large surface, is also well known. The experiments related in the former part of this memoir having led me to expect that the increase of force and harmony in these circumstances was owing to the property possessed by wood of propagating sound better than the external air, and modifying its tone; I determined to ascertain the truth by experiment.

The different powers I had observed in wood and marble, with regard to the propagation of sound, appeared proper to throw some light on this important question. In fact, if the modifications of the sound of the tuning-fork and the watch, when placed on a table of wood, be owing to the energy with which this substance transmits the sound, and the marble propagating it very weakly, it would follow that a table of marble ought not to fortify the

* Morhof, *Sten.* pag. 104. affirmed that fat oils could not transmit sonorous modulations. P.

† Loco citato.

found, or at least to produce very little effect on these sonorous bodies. On these considerations I made the following experiments :

Experiment I. I applied the tuning-fork upon a table of wood, and when it had ceased to vibrate I applied my watch to the same surface. The sound in each was increased in proportion to their respective intensities. I then removed the wooden top of the table, and substituted in its place another of marble, of the same dimensions. The tuning-fork and watch were applied as before. The sound of the former was augmented, though much less than when it was applied on the wooden covering. The sound of the watch was scarce perceptibly increased. I could perceive little difference between its sound, whether it touched the marble or hung in the air at the same distance from the ear.

Though this experiment strongly confirms my conjecture, I determined, nevertheless, to subject it to another proof :

Experiment II. I placed my watch on the wooden table, and closed my ears with chewed paper. When the ear was at a few lines distance from the table, I could not hear the vibrations of the watch. I then placed my ear in contact with one of the small wooden cylinders used in the experiments already described. The opposite end of the cylinder was placed in contact with the table.

The sound of the watch immediately and forcibly struck the ear. I made this application to every part of the table, not excepting even the feet ; and the sound was in every case distinctly heard. The experiment was then repeated with the marble covering instead of the wood. The vibrations of the watch were heard in an imperfect or indistinct manner, and only when the wooden cylinder was applied at a short distance from the sonorous body. I did not use the tuning-fork in this experiment, because, in spite of every precaution, it was not possible to close the ears so perfectly but that the sound still remained perceptible.

In order to establish my conjecture on the most solid basis, it remained to repeat the experiments upon tables of all the substances used in the experiments related in the first part of this memoir, and to shew that the resonances follow the proportion of the conducting powers of the bodies. But the difficulties which opposed the execution of this plan induced me to confine my enquiries to ascertain whether the resonance does not vary in different bodies, as well as the power of transmitting sound.

Experiment III. I therefore placed the tuning-fork successively upon plates of earthenware, of porcelain, on plates of glass, and upon thin isolated plates of copper and tinned iron. The sound was fortified by these bodies, and the tone never appeared to be exactly the same in any two experiments.

These trials naturally led me to examine the same sounds upon musical instruments. With this view I applied first the tuning-fork and afterwards the watch, upon bases, violins, mandolins, guitars, harpsichords, and horns. Both sounds were proportionally increased.

They appeared to acquire more force and melody by means of the musical instruments than on the other bodies. The intensity seemed directly in proportion to the volume of the instrument.

From these experiments it follows :—1. That all the substances which were tried, which possessed extended surfaces, fortify the weak sounds produced by bodies which touch them, and modify the tone in a manner peculiar to each.

2. That

2. That these effects arise from the transmission of sound by solid bodies being in general better than by the air, and the peculiar modification of the tone by each.

3. That the resonance of musical instruments is more particularly to be attributed to this cause*.

4. The experiments with musical instruments afford reason to conclude that the volume of bodies has an influence on their resounding properties.

5. M. de Maupertuis † has affirmed that the resonance of musical instruments is owing to the instrument containing fibres of every possible length; each sound puts those in motion which are either in unison or concord with itself, while the others remain motionless ‡. But the second experiment, in which it is shewn that there is no part of any resounding body which does not transmit the sound, will not admit of the adoption of the ingenious thought of that celebrated author.

6. As marble in some degree extinguishes sound, and bears the same rank among solid bodies as inflammable air among fluids, it is not advisable to use it in the construction of churches, concert-rooms, theatres, or other edifices in which the propagation of sound is desirable.

Such are the principal results of experiments which, on the whole, have engaged my attention for a number of years; notwithstanding which, I have not been yet able to extend them, particularly those belonging to the second part, as far as they are capable of being carried. I shall not, however, have the mortification to think my endeavours useless, if the society to whom my works are addressed shall think they may constitute an addition to the sum of the discoveries in science which they continually present to the world.

Annotations to the Paper of M. PERROLE on Sound.

THE philosophy of sound is still exceedingly imperfect. Mathematicians have established theories upon the simple data that sonorous bodies vibrate, and that these vibrations are communicated to the surrounding elastic fluid, of which the density and spring are measurable. M. Perrole's experiments shew that the object of research comprehends much more than this. The following queries and remarks may perhaps prove of some advantage to the curious enquirer:

1. There can be no doubt but that undulation in the air accompanies its propagation of sound, and that sonorous bodies are put into a vibrating state by percussion. The proofs of this are too numerous to be detailed. Sound propagated through fluids seems to diminish in proportion to the distance, and so probably it does in solid bodies. Its intensity must depend upon the elasticity and density jointly. The military practice of lying down on the ground to listen for footsteps is well known; and Dr. Franklin relates that he heard the blow of two stones struck together at the distance of near a mile, smart and strong as if close at the ear §. We have no information respecting the velocities of sounds propagated through solid bodies.

* The numerous surfaces which these instruments present must likewise render them more sonorous. P.

† Acad. Paris, 1724.

‡ The memoir of M. de Maupertuis relates to stringed instruments only. P.

§ Experiments and Observations. London, 1774; p. 445.

2. As sound passes through the air with a velocity of 1142 feet in a second, and the swiftest wind does not pass with a velocity of 90 feet in the same time; it might naturally be inferred that the velocity of sound with or against the wind ought to be somewhat affected, but its intensity very little. The contrary, however, is the case; from which, and other circumstances, some philosophers have been disposed to conclude that the medium by which sound is transmitted is not air, but another fluid of greater subtilty.

3. Sounds seem more intense, and are heard to a greater distance, by night than by day. In a still night, the voices of the workmen at the distillery at Battersea may be heard at Westminster Bridge, over an interval of about three miles; the watch-word at Portsmouth may be heard at Ride in the Isle of Wight, the distance of which is between four and five miles; and numerous instances are related of the propagation of weak sounds to much greater distances, without mentioning those of which the intensity is greater. It is a practical question of some importance to ascertain whether this difference may arise from the different state of the air, the greater acuteness of the organ, or the absence of the ordinary noises produced in the day. By attentive listening to the vibrations of a clock in the night, and remarking the difference between the time when no other noise was heard, and when a coach passed along, it has appeared clear to me that this difference arises from the greater or less stillness only, and that no voluntary effort or attention can render the near sound much more audible, while another noise acts upon the organ. In this situation the ear is nearly in the state of the eye, which cannot perceive the stars in the day time, nor an object behind a candle.

4. It would be impossible to use optical instruments in the day time, if the light of other objects besides those to which the attention is directed were suffered to mix itself with the rays admitted into the field of view. In the same manner it seems that we have little to hope with regard to the improvement of acoustic instruments for day use, if the first requisite, namely, the exclusion of unnecessary sound, cannot be obtained.

5. Numerous experiments have shewn that sound can be reflected, and that the impression on the ear is greater or less, according to the disposition of the reflecting bodies. Optical instruments are disposed in tubes of such a length, that the rays of light which arise from a small portion of the visible hemisphere can alone reach the organ of perception. All the others strike the surface of the tube, and after one or more reflections are almost totally absorbed or lost. It remains to be ascertained from reasoning and experiment, how far the same effect may be produced with regard to sound. With a cylindrical wooden pipe, three inches in diameter and eight feet in length, at the distance of two miles from London, I listened to the noises which came from that capital. I think I did not deceive myself by any prepossession, when I distinctly heard the noise and agitation of wheels on the pavement much more strongly than any other kind of sound. Nearer sounds, not in the direction of the tube, were less perceived; and such as were loud assumed a musical tone; most probably from the reiterated reflections under the several angles of its reception. I consider this experiment as of little other value than as serving to convey an hint, that a tube lined with cloth might defend an acoustic instrument from sounds out of the line of its direction, while the instrument itself might magnify and render distinct the sound required to be heard.

6. M. Perrole conjectures, at the end of the first part of his memoir, that sonorous

bodies do not vibrate during the propagation of sound, because the water in his curious experiment of the watch was not seen to move. He has overlooked the very great number and minuteness of the vibrations required to produce sound. They cannot be visible but in cases of extreme simplicity and intensity. The string of a musical instrument emits sound long after its vibrations have ceased to be visible. He likewise, at the end of his second part, objects to the theory of Maupertuis, which supposes that sonorous and resonant bodies subdivide themselves so as to vibrate differently in different parts. But the body in question may be considered under different points of view. 1. As the medium of sound, it may conduct or transmit every sound indifferently. 2. If its simplicity of figure and texture be such as to produce very few sounds at a time, it will, upon the whole, emit a musical tone. For I consider a musical tone as one or more simple sounds in concord with each other; and a noise, as a greater number not possessing the same relation. Thus, the noise produced by pressing down an indiscriminate number of contiguous keys in the organ, is so far from being musical, that it seems astonishing that the aggregate of pipes so melodious should produce a sound so harsh. 3. Or the body in question may assist or impede the sound of another simple body, such as a string with which it is in contact. When it assists that sound, it does not seem improbable that the effect pointed out by Maupertuis may really take place. For the division of bodies, his supposition requires, is known to take place in the single string which gives the trumpet notes, and has on that account been called the trumpet marine; and also in bells, which not only give a set of distinct contemporaneous tones differing in acuteness, but to a certain extent are found to alter the system according to the plan of percussion. And again, it will readily be imagined by those who take so much pains in experiments for fixing the sound post of the violin, that the resonance which is so much affected by this disposition, is more probably of the whole instrument than merely of its parts as conductors of sound.

7. The figure of the external ear, which is made up of a series of concavities with stops or bridges interposed, is an object which, as far as I know, has never yet been explained or enquired into. In the cat and other animals this structure appears to be very complicated. After the sound or aerial undulation has been modified and conveyed into the ear through this apparatus, it is received upon the stretched membrane called the tympanum. This, as well as the other membranous parts of the internal ear, seems evidently adapted to vibrate by the action of sound; and most probably after the manner that a musical string vibrates when another string is made to sound in its vicinity, and emits a tone in concord not too remote from unison. From this fact and the formation of the ear, a question may be proposed of some consequence with regard to the construction of acoustic instruments: Whether the sound produced by a remote body or that which is emitted by a corresponding vibrating body, at a less distance from the organ, be the most perceptible? For example, if two strings, A and B, be tuned in unison, and the string A be ten feet from the ear, while the string B is placed at the distance, for example, of one inch from the same organ;—whether the secondary sound of B, when A is struck, may not be more perceptible than the original sound of A? It is unnecessary to enter into considerations of the mechanical circumstances under which, from theory, this or the contrary effect may take place. It may be sufficient in this very remote view of the subject to remark, that in point of fact the influence of sounds may extend further than the organ can perceive them. I was once in a room facing the

the street, in London, when I perceived the window to be agitated with certain tremulous motions, attended with a considerable sound. The reiterations were short and distinct, three or four at a time; and then ceased till a second and a third repetition of the same effect took place. This remarkable process continued to increase in intensity of sound, and engaged my attention for about a quarter of an hour; after which the nearer approach of one of those instruments called a tambourin, composed of parchment stretched on a hoop, and played upon by rubbing the extremity of the finger upon its surface, evinced that the agitation and sound emitted by the window had been caused by the vibrations of that instrument. Hence it should appear, that the judicious construction of an instrument for receiving and magnifying sounds would not only require a scientific arrangement of an external part to exclude foreign sounds, and reflecting surfaces to modify and augment the direct undulations; but that the last effect should be received on a tympanum capable of adjustment in its tension, and thence conveyed by a proper vestibule to the organ of perception itself. If the science of receiving and augmenting sounds were once improved to the degree here sketched out, there would probably be no difficulty in magnifying sounds intended to be conveyed from one place to another.

VI.

Concerning the Steam-Engine as originally invented by the Marquis of WORCESTER, and the Improvements since made in Steam-Engines without the Piston or Lever. With a Description of an Engine of this Kind constructed by Mr. PETER KIER, of Kentish Town.

THE Marquis of Worcester is the undoubted inventor of the steam-engine, which is described in his *Century of Inventions*. From the title, he appears to have constructed one before the year 1655. His words, No. 68, are as follow:

“An admirable and most forcible way to drive up water by fire, not by drawing or sucking it upwards; for that must be, as the philosopher calleth it, *intra sphaeram activitatis*, which is but at such a distance. But this way hath no bounder, if the vessels be strong enough; for I have taken a piece of a whole cannon whereof the end was burst, and filled it three quarters full of water, stopping and screwing up the broken end, as also the touch-hole; and making a constant fire under it, within 24 hours it burst and made a great crack; so that, having a way to make my vessels so that they are strengthened by the force within them, and the one to fill after the other, I have seen the water run like a constant fountain stream forty foot high. One vessel of water rarefied by fire driveth up forty of cold water. And a man that tends the work is but to turn two cocks, that, one vessel of water being consumed, another begins to force and re-fill with cold water, and so successively; the fire being tended and kept constant; which the self-same person may likewise abundantly perform in the interim between the necessity of turning the said cocks.”

Plate XVII. Fig. 1, represents the steam-engine which was made by Captain Savery, and generally supposed to be the same as the Marquis of Worcester's. A represents a boiler containing water, the steam of which may be transmitted into either of the vessels B and C by means of the cocks D E. The vessels have a communication at bottom with the vertical

tical pipe F G. These communications are made between two valves H I and K L opening upwards; and the effect is as follows: Imagine the upper part of the pipe to be filled with water for the purpose of rendering the valves tight, and let the cock D be turned. The steam from the boiler, being lighter than the air included in the vessel B, expels that air, which, not being permitted to pass through the valve H, issues upwards through I. In this situation, let the cock D be shut, and cold water be thrown upon B, or, which is much better, spouted into it; and the steam will become condensed, and leave an empty space into which the pressure of the atmosphere at F will force the water upwards through the valve H. While this is performing, the cock E is to be opened, and the air from the vessel C expelled by the steam in the same manner. This is the regular series of operations, and is repeated a second time by shutting the cock E, cooling the vessel C, and opening the cock D. By these means the water rises from F into the vessel C, while the steam from the boiler presses the water out of B, through the valve I, to G. The whole process therefore, simplified by attending to one of the vessels only, is found to consist in expelling the air by steam; forming a vacuum by condensation into which the water rises, and then forcing that water upwards by new steam, which, being condensed as before, is replaced by new water; and so forth.

To what practical extent the experiments of the Marquis of Worcester were carried, does not appear; but it was near the end of the seventeenth century, when Captain Savery proposed this engine as an invention of his own, for raising water and draining mines. Prony, in his *Architecture Hydraulique*, i. 564, says, he published his *Miner's Friend* about 1699. Desaguliers, in his *Course of Experimental Philosophy*, 3d edition, ii. 465, does not scruple to affirm, that Savery had this invention from the Marquis of Worcester's book; but has not, it must be confessed, given to his assertion all the proof it might require. He says, that Savery bought up all the copies of the *Century of Inventions* which he could procure, and burned them; and that he used to relate a story as the first hint of his invention: that having drank a flask of Florence at a tavern, and thrown the empty flask upon the fire, he called for a basin of water to wash his hands; and perceiving the little wine left in the flask had filled it with steam, he took it by the neck and plunged its mouth in the water, which was immediately driven up by the pressure of the air. On this incident, Dr. D. remarks, that it could not have so happened; because when he himself had repeated the experiment by boiling half a glass of wine in a flask, and, putting on a thick glove to defend his hand, had plunged its mouth beneath the surface of water, the pressure of the atmosphere was so strong as to beat the flask out of his hand against the ceiling—a circumstance with which he does not doubt but that the Captain would have embellished his tale, if he had made the experiment.

It appears to me, nevertheless, that somewhat more than these incidents ought to be required to establish the charge of deception against Savery, who, in the course of probabilities, might have invented the steam-engine half a century after the Marquis of Worcester, whose description, clear as it now seems to us who know the engine, was not perhaps enough so as to attract much attention, in company with one hundred other enigmatical proposals: Captain Savery being a patentee, and having probably a considerable interest depending, might be as strongly urged to suppress the Marquis of Worcester's book on that account, as for the reason asserted by Desaguliers. And with regard to the experiment of the flask, it is hardly allowable

allowable for a philosopher to conclude that the experiments of another are false or impossible, because they do not agree with his own. I suppose Desaguliers made his experiments with more wine and more heat than Savery; and if I were disposed to reason against the relation of a positive fact, I should be inclined to doubt the projection of the flask against the ceiling by an exertion of atmospherical pressure, which must have been as strong on the outer as on the inner surface of the vessel. And in this doubt I should be still more settled, from having myself very often repeated the experiment without any such result. But the integrity of Desaguliers forbids any such insinuation. Under certain circumstances the flask might have been beaten out of his hand; but it does not follow that those circumstances were present in Captain Savery's experiment.

The Marquis of Worcester was the first inventor of the steam-engine. Savery was either the second inventor, or he had the ingenuity to discern a valuable invention in the midst of loose hints, and to give it organization and effect.

We do not know how the Marquis of Worcester condensed his steam, or, indeed, whether he condensed it at all; for it certainly does not require condensation unless for what is called sucking. It is said, that the condensation in the first engines was effected by water on the outside of the vessel; but in Savery's engines it was performed within the vessel. Several of his engines were made with only one steam-vessel, and Desaguliers found by experiment, that this kind is considerably better than such as have two; because the action of the steam is rendered more sudden, and the chemical condensation during the process of forcing is less. This excellent philosopher and practical engineer made several other improvements in this engine and its parts, which may be seen in his work last quoted: but the subsequent invention of the steam-engine with a piston and lever, and the improvements which have been made therein, seem to have greatly retarded the progress of the original simple machine.

Dr. Papin, well known for his invention of the digester, was busied in experiments on steam. Prony, in his *Architecture Hydraulique*, i. 566, mentions a work of his, printed at Cassel in 1707, under the title of *Nouvelle Maniere d'élever l'Eau par la Force de Feu*, in which a steam-engine is described which differs from that of Savery, but may equally accord with the description of the Marquis of Worcester. From the engraving, fig. 268, in his second volume, it appears to have consisted of a spheroidal boiler, a cylindrical steam-vessel into which was fitted a float or piston, and an air-vessel which received the water from each stroke previous to its being forced by re-action to a greater height. The water flowed through a pipe with a valve opening downwards, whence it passed beneath the piston which floated upon it. The descent of the piston was effected by the steam, and its ascent by the action of the water from the original stock; at which period the steam was suffered to escape into the air by means of a cock, and the communication between the boiler was shut off.

In these engines it may readily be apprehended that the action of the direct steam on any definite surface, such, for example, as a square inch, will be accurately equal to the re-action of the water which is forced up; and consequently, that Savery's engine will require steam more elastic than the air of the atmosphere, in every case except that wherein the water is raised by suction, and afterwards suffered to flow out of the bottom of the vessel into a channel or cistern. But if this action of steam be, by the intervention of some mechanism, transmitted to the place where it is intended it shall operate, it will be possible to regulate the proportions in any desired manner. It was very early in the present century

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that Newcomen and Cawley began their trials of improvement of the steam-engine with a cylindrical steam-vessel and piston depressed by the weight of the atmosphere, by which means they obtained a safe application of power for raising water to heights far beyond any in which the old engine could be trusted, on account of the extreme strength required in the boiler and steam apparatus. This event is probably the cause why so little has since been done to remedy that leading imperfection, or to adapt the original machine to work itself without an attendant. It is certain, however, that the weight of the apparatus, and frictions of the parts, in all steam engines with a piston and lever, have prevented their being extensively useful in small undertakings, such as the blowing of small furnace-bellows, the drawing of boats and carriages, with numberless other operations where forces not exceeding one, two, or three horses are wanted. In these operations it is highly probable that the engine of Savery would be very useful, if it were made to work without an immediate attendant; for Defaguliers found the advantage greatly in favour of Savery's engine, on a small scale, compared with Newcomen's lever-engine. He had an engine of the former construction in his garden, which raised ten tons of water an hour, about thirty-eight feet high; and a friend of his erected a working model of the lever-engine on the same spot. The boiler of this last was exactly of the same size as that of Defaguliers, and his cylinder was six inches bore and about two feet in length. It raised four tons per hour into the same cistern. It cost three hundred pounds; but the engine on Savery's construction, having all copper pipes, cost but eighty pounds.

It must not be overlooked, however, that this account affords no statement of the quantity of fuel consumed under each boiler. It might perhaps have been the case, that Savery's boiler could have fed a larger cylinder. But at all events there is no doubt of the fact, that the best lever-engines cannot be advantageously used when smaller than a determinate size; and that, on account of the charge of attendance to open and shut the cocks, no trials have been made to shew upon how small a scale Savery's engine might be rendered useful.

An engine upon Savery's principle, with various judicious improvements, was erected four years ago by Mr. Kier, at his manufactory of coach axle-trees near Pancras, where it has almost constantly been worked without repair. It is perhaps on too large a scale to afford much information respecting this question; but is certainly of considerable value, not only because it works without an attendant, and regulates its own motions, but also because, as might naturally be expected, the wear and tear is altogether inconsiderable. He has permitted me to give an account of this engine.

The sketch Fig. 2. Plate XVII. represents this engine, without extreme pretensions to accuracy, but upon a scale of a quarter of an inch to a foot. R represents an oval boiler seven feet long, five inches wide, and five deep. The proprietor considers it as being of dimensions sufficient to work a larger engine; a circumstance which must, in a certain degree, diminish the effects of the present. It feeds itself in the usual manner, with water conveyed through a pipe at the end of which is a valve. This valve does not open until the fall of the water within the boiler has suffered a float to subside, which by its actual weight assists to draw it open, but by its tendency upwards, as the water in the boiler rises, serves effectually to close it. The boiler, therefore, remains constantly at or near the same degree of fullness. The steam is conveyed by a pipe TAV to a box B, through which, by the opening and shutting of a valve, it can be conveyed to the working chamber E. The axis C

serves as a key to open and shut the valve. NO is a cistern of water, from which the supply is made through the vertical pipe in which the valve Q is placed; and GG is another cistern into which the water is delivered through the pipe F, which is provided with a valve H opening outwards. IM represents an overshot wheel eighteen feet in diameter, moving on the axis KL, and communicating its motion to the lathes and other rotatory engines of the manufactory. The water in both the cisterns becomes warmer than the hand after working a short time; for which reason the injection-water is forced up by a pump from a well supplied by the small stream on which these works are established. A leaden pipe passes from this forcing pump to the upper or conical part of the chamber E, for the purpose of injecting cold water at the proper time. Neither of these could be represented with convenience in the present section.

The manner in which the steam and cold water are alternately admitted into the chamber E, remains to be explained. Upon the extremity K of the axis of the overshot wheel there is fixed a solid wooden wheel about four feet in diameter, represented in fig. 3, as seen in front, and also in profile, where the small letters denote the same things as are marked by the large letters of the alphabet in the front view. ABCD are four cleats, all or any number of which may be fixed on the wheel at a time. Each cleat has its correspondent block EFGH on the opposite surface of the wheel. The use of these is to work the engine. Suppose the wheel IM or any part of the revolving apparatus be drawn round by hand, one of the cleats meets in its rotation with a lever which opens the steam-valve by a bar of communication reaching to the handle of the axis C, fig. 2. The steam consequently passes into the chamber E, and the steam-valve shuts again as soon as the cleat has passed. Speedily after this the correspondent block on the other side of the wheel meets another lever, which is similarly attached to the handle of the forcing pump, and therefore throws a jet of cold water into the chamber, and condenses the steam. The pressure of the atmosphere then forces the water from the cistern NO through the valve Q towards the chamber E. When the engine has been long out of work, I suppose two or three strokes may be necessary to raise the water to the top of the chamber. As soon as this is the case, the injection of the steam suffers the whole body of water above the valve H to overcome the pressure of the atmosphere and rush out. The water which is raised is suffered to flow upon the overshot wheel through a sluice, and by that means keeps the work in motion, and replenishes the lower cistern.

Hence we see that in effect this engine is the same as figure 1. excepting that it is not applied for the immediate purpose of raising water, but gives motion to other apparatus. It is therefore unnecessary to insist more largely upon the mere operation; but the peculiarities of Mr. Kier's engine are deserving of notice. In the first place it may be observed, that he uses no reservoir for his injection water, but drives the requisite quantity up at each stroke. The advantage of this is, that the pump sustains the action of a very short column of water, though it apparently forces to the height of about 26 feet. For the column in the pipe and chamber PE by its re-action takes off the effect of an equal length of column which would else have acted against the forcer, and thus renders the injection more easy and quick. In the second place it may be observed, that he has proceeded directly opposite to the first remark of the Marquis of Worcester, who rejects drawing or sucking upwards. For this engine does not force at all. The water merely falls out of the chamber, and consequently

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never requires steam stronger than the atmosphere. From the effect of this engine under circumstances of such advantage, it may fairly be concluded that the action of steam against water in forcing can never be beneficial except at a place where fuel could be had extremely cheap. Thirdly, it was found at the first construction of this engine, that the consumption of steam by contact with the water was so great that it could not be worked with the smallest advantage. This defect was remedied in the present engine, as well as in another at Norwich, by fixing a small air-valve in the steam-box, which was struck for an instant immediately before the admission of the steam. It may be presumed that the air occupied a space above the water, and prevented their coming together. Mr. Kier, however, is disposed to think that the effect does not take place in this manner, but by some mixture and sudden dilatation of the two fluids; because he imagines the mischief from the wet cylinder would be the same upon descending steam. Much, however, may be said in defence of the opposite opinion. The air-valve is not at present used, because the engine does very well without it; which is supposed to arise from its being less air-tight than it was at first. Fourthly, the motion of the overshot wheel is regulated by an apparatus called a-governor, invented, as I think, by Mr. Watt, and represented in figure 4. The bar HI revolves by communication with the engine, and carries round the balls A and B, which move on a fixed joint C. When the rotation is very quick, the balls fly out, and draw down the points DE, and consequently F, which is so constructed as to slide upon the bar or axis HI. A lever, FG, connected with the sluice of the upper cistern GG, fig. 2. is therefore made to fall or rise accordingly as the velocity is greater or less. By this disposition, when the wheel moves very speedily, from lightness of work or any other cause, the quantity of water thrown down from the upper cistern is immediately diminished; and, contrarywise, the quantity of water is rendered greater when the slowness of the movement shews that it is wanted. When I saw the engine at work, it had but one cleat and block upon its wheel, and two men were at work at strong lathes, roughing out certain pieces of iron about two inches in diameter. Whether it were the unsteadiness of this work, or the want of sufficient celerity of communication, I know not: but the variations in this governor were upon the whole more considerable than I have remarked in a lever-engine with a fly and this apparatus also. Fifthly, as the injection-water makes a constant addition to the sum of the water contained in the cisterns NO and GG, the surplus is allowed to overflow. If it were to overflow the lower cistern, it would warm the water in the injection well; for which reason a contrivance becomes necessary to prevent that effect. Mr. Kier's contrivance is this:—The water of the upper cistern, when at a certain elevation, overflows into a communication which conveys the water to the lower; for which reason its contents can never rise above a certain level. There is also a pipe from the bottom of the upper cistern, which is recurved upwards to communicate with a gutter a little lower than the place of overflowing just mentioned. It is clear, therefore, that the upper cistern would always overflow at this pipe sooner than at the place of communication with the lower cistern, if there were not a valve within the upper cistern, over the mouth of the recurved pipe. Now it is so managed by means of a float in the lower cistern, that this valve in the upper shall be opened whenever the water below rises to a certain level. And consequently, as soon as ever the lower cistern has received this portion, it can receive no more, because all the subsequent addition of water will pass away by the recurved pipe.

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This engine consumes six bushels of good coals in twelve hours' work when in its best state, or seven bushels when at the worst. Under these circumstances it gives ten strokes per minute, each throwing out seven cubic feet of water, at an aperture twenty feet above the water beneath. This quantity, namely 70 cubic feet per minute, will weigh 4345 pounds, which being doubled, to reduce it to Defaguliers's standard height of ten feet, will amount to 8690. And this divided by 580, the number of pounds in a hoghead, will give a quotient of 15, representing so many men, according to the estimate of that author, which he reckons equivalent to three horses. This result is not more than half what is performed by the improved engines with a piston of such a size as to be equal to 20 or 25 horses. But how far those engines would prove effective upon the small scale of three horses, or the still smaller scale of one or even less, remains to be decided.

The engine here described has been at work four years, and from the simplicity of its construction has yet exhibited no proofs of wear. Mr. Kier thinks it a profitable engine to himself, and that it would be serviceable for raising water where coals are cheap. A contrivance he made in the year 1783 might, perhaps, with proper modifications, be used to obviate the necessity of forcing where the heights are considerable. It is liable, however, to some strong objections, and certainly requires to be maturely weighed with regard to dimensions and effects before it should be attempted to be put in practice.

Fig. 5. represents the contrivance for forcing, as I find it in the original drawing; and I thought it unnecessary to make any alteration to adapt it to what is called sucking, which might, if required, be easily done by any person slightly acquainted with the subject. A is a boiler, and B a steam-vessel. This last communicates with the vessels M L K T, each of which, except the lower one, consists of two vessels; an interior vessel, closed on all sides excepting where it communicates with B; where pipes P, O, N, I, enter the upper part of each; and also where there is a valve at the bottom, opening upwards: the pipes I N O likewise communicate with the three exterior vessels K, L, M. If steam be let pass from A to B, it is inferred that the air will be driven from B, and press upon the water in the lower vessel T, which will be driven into the exterior vessel of K, but not into the interior vessel, because the pressure of the air through the tube H is more than sufficient to keep the valve of the apparatus K shut. The next step in the operation consists in closing the cock C, and opening D, out of which the re-action of the water forcing itself into T to its natural level, and into the interior vessel of K on the same account, will drive a portion of steam. D is then to be closed, and C opened; in consequence of which, the contents of T will be forced up to K, as before; and the interior vessel K will evacuate its contents into the exterior of L. The steam being shut off at C, and the cock D being opened as before; the vessel T, the interior vessel K, and the interior vessel L will fill as before, and a larger portion of steam will issue from D. A third repetition of the process will drive the contents of these three interior vessels a step higher; and a fourth repetition will cause the contents of the upper interior vessel M to flow out at P; after which every alternation of the work with the cocks C, D will throw out the same quantity from P.

I shall only remark on this contrivance, that the vessel B must necessarily contain a quantity of air capable of occupying the whole interior space contained in the closed vessels T K L M, with an allowance for the loss of bulk in condensation under the pressure of a

column of water equal to one of the lifts, and that the quantity of steam to be discharged at each stroke must occupy a space equal to that of all the water moved at each stroke, and must in all cases be considerably stronger than the atmosphere.

VII.

On the Mechanism by which the Mariner's Compass is suspended.

IF a bar of iron be rendered magnetical, by rubbing it on a natural magnet, or by any of the well-known processes for that purpose; it acquires the property of disposing itself nearly in a north and south line, whenever it is suspended so much at liberty that the energy of this power is sufficient to overcome the friction or other impediments to its motion. The mariner's compass is an apparatus in which a bar of this kind, called the needle, is supported, for the highly useful purpose of determining the position of the meridian at sea, and consequently of enabling ships to steer their course by day or night, without observation of the stars, or any other external objects, as was necessary before the discovery of this instrument. In a well-constructed mariner's compass the needle is defended from the impulse of the air, and is little subject to be disturbed by the external motions or agitation of a ship at sea. As this disturbance is, however, the chief impediment to the convenient use of the compass in a boat, where the motions are sudden and short, or in a ship, when the waves are very turbulent; and as the artists in this branch have endeavoured to persuade the world that certain pieces of mechanism were much superior in their use to others differently disposed;—I thought it might be of some utility to explain the simple principles of a good suspension.

When the needle of the compass disposes itself in the magnetical meridian, there is a certain line within the piece of steel, which joins its two poles, and may be considered effectually as the needle itself. But as this line is not visible, the admeasurements of position must be made with regard to some marks on the extremity of the needle; which marks will be truly placed when the needle is found to occupy the same position with respect to a fixed point, upon being reversed, so that the lower side shall become the upper. If the magnetical power had been found on experience to occupy the same parts of the needle with proportional intensity, during its decay, to that it possessed immediately after the touch, these marks, once made, would continue to shew the true magnetical points as long as the needle possessed any directive force. But it is well known that soft steel loses its magnetism sooner than hard; and consequently it may be inferred that, unless both sides of a needle were equally hard, the magnetic power would deviate in process of time towards the harder side. These considerations lead to an obvious method of diminishing such growing error. It consists in making the needle flat and thin, and suspending it with its edge, and not its flat side, uppermost, as is more commonly done.

The needle is usually supported on a steel point, which occupies the axis of a cylindrical box called the compass-box. For this purpose there is formed in the needle itself a cap or hollow conical centre of brass, steel, or hard stone, which is applied over the point. The tendency of the needle to be disturbed by agitation will greatly depend upon the position of

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the vertex of the conical cavity. It is necessary that it should be above the centre of gravity; but this distance must be so small as that the libration of the needle when one end is depressed shall be very slow, and yet speedy enough to recover the horizontal position in a reasonably short time. In fact, the whole of the steadiness of the compass and its box appears to depend on this principle of slow vibration. For, if a needle perform its vertical vibration in eight seconds, it will be very little disturbed by an alternate action that lasts but a second or two.

The greater number of workmen have imagined that the agitation of the compass is communicated by friction at the points or edges of suspension, and have accordingly exerted their ingenuity to diminish this friction, by contrivances similar to that of a conical cap balanced on a point, and itself affording another point to support the needle. But it is very readily proved by experiment, as well as argument, that the greatest disturbance of the needle is produced by the quantity of horizontal progressive motion, and not by the mere inclination or angular motion. A compass-needle supported on a simple point will suffer very little agitation from any angular motion or moderate deviation from perpendicularity in the pin; but it will instantly begin to vibrate if moved horizontally. Thus the common experiment of tilting the compass-box in all positions while its centre remains immoveable is fallacious, and there are very few compasses indeed which will bear to be slid backwards and forwards upon a table.

It appears therefore that the steadiness of a needle which vibrates slowly, is the consequence not only of the length of time it allows for alternate actions to operate, and destroy each other; but also of the difficulty with which it yields to such impressions. If the centres of suspension and of gravity in the needle were coincident, no angular motion would be produced by any action of the pin, excepting by the effects of friction; and the angular motion produced in other cases will be less, the shorter the distance between these two centres, or the lever by which it is propagated.

The simple suspension of the needle on a point has been applied to the compass-box, for which it is little suited, not only because of the wear upon so small a surface, but also because it admits the box to traverse horizontally; an effect which is inconvenient, and cannot be remedied by any means not calculated in some respect to increase the effects of agitation. The method most generally received, and in fact the best adapted to this instrument, are the gimbals.

This well-known contrivance consists of an hoop supported upon two pins diametrically opposite each other, and issuing from the external surface of the ring in such a direction that both lie in the same diametrical line. When the hoop is suspended on these pins, it is at liberty to turn freely round the diameter of which they constitute the prolongation. The notches or holes of support are disposed horizontally. The compass-box itself is placed in a similar ring with two projecting pivots; and these pivots are inserted in holes made in the former ring at an equal distance from each of its pivots. If, therefore, we suppose the whole to be left at liberty, the compass-box may vibrate upon the diametral line of the outer ring, and also upon a line formed by its own pivots, at right angles to that diametral line. The consequence of this arrangement is, that the centre of gravity of the compass-box will dispose itself immediately beneath the intersection of both lines on which it is at

liberty to move:—that is to say, if the weight of the box or its parts be properly disposed, the compass will assume a position in which its upper surface shall be horizontal.

The same principles which were applied to the single centre of the magnetic needle will also apply to the axis of the gimbals. If the centre of gravity of the compass-box be so placed with respect to either axis as that its vibrations shall be quick, every horizontal action will greatly disturb it, and it will not speedily settle. The most favourable position of the pivots or edges of support in the gimbals will be when they all lie in the same plane, and the centre of gravity of the compass-box is very little below that plane.

The practical application of these inferences appears to be, that, without pretending, as has been done, that any peculiar secret or great discovery is required to give stability to this useful instrument, nothing more is required than good workmanship, and a proper adjustment of the weight with regard to the centres or axes of suspension. The needle ought to be adjusted either by means of its cap, or by proper filing away, or else by additional pieces to the card, so that it shall vibrate very little, and that slowly, when placed upon a point and moved horizontally, whether in the direction of the needle or at right angles to that direction. The card is then ready for the compass-box. The box itself must be adjusted with the card in its place, so that it shall exhibit the same steadiness when moved in the line of direction of the outer pivots. And lastly, the same disposition must be made with regard to the motion in the direction of the inner pivots. It is scarcely necessary to add, that the means of this adjustment consist in shifting the pivots themselves, or, which is much better, in altering the disposition of weight about the compass-box. An external ring of metal, encircling the box, and raised or lowered until the proper place for fixing it is found, would perhaps afford the most convenient method.

Upon the whole, the reader will perceive, that the leading aim of the present paper is to enforce the truth, that the compass is very little disturbed, at sea or elsewhere, by tilting the box on one side, but very much by sudden horizontal changes of place; and, consequently, that a scientific provision against the latter is the chief requisite in a well-made instrument of this kind. And again, that nothing is more easy than to ascertain whether a compass possesses stability; since nothing more is requisite than to slide it upon a table in the several directions above-mentioned, and remark how far it is disturbed. The good workmanship of the cap and pin of the needle may be ascertained by inspection with a magnifier, and also by drawing the card with a small key or other piece of iron, a very little quantity, for example a quarter of a degree, out of its station or position, and remarking whether it returns accurately to its original station.

Before I quit this subject, I must take notice, that, as the suspension on a point has been applied to the compass-box, so, on the other hand, the gimbals have been applied to the needle. This was done by the late Dr. John Lorimer, who disposed a dipping needle on its own axis between the cheeks of a frame parallel to the diameter of a circle of brass graduated to shew the angle of dip. This circle or meridian had pivots at its zenith and nadir, which moved in holes diametrically perforated in another circle. The needle was by this means not only permitted to dispose itself in a line forming the angle of dip with the horizon, but was enabled to carry the meridional circle into the line of the magnetic meridian. It does not appear from the Doctor's paper and drawing in the 65th volume of the *Philosophical*

phical Transactions, that he had added the simple expedient of an horizontal gimbal-hoop to the cross circle which supported the meridional pivots. Such a circle, divided, instead of the fixed horizon in his figure, would have rendered it a complete instrument for general purposes. It is probable however, that for ordinary naval use the simpler compasses of the present construction would be preferable.

VIII.

On the Maintaining Power in Clocks and Watches.

IN a former paper on the compensations for change of temperature in pendulums, a slight view was taken of the methods by which a train of wheels might be made to move either uniformly or by equal intervals of progression, so as to afford a measure for the lapse of time. The pendulum and the balance are at present the regulators which for just reasons are preferred to every other contrivance. If we suppose a body to be suspended at the extremity of a string, its gravity, as is well known, will cause the string to point towards the centre of the earth's gravitation at that part of the surface. If the body be then drawn aside out of this perpendicular position, it is equally clear that it will be removed further from the earth's centre, towards which, when again set at liberty, it will fall through the arc of a circle, which is the only line the string will permit it to describe. At the instant the perpendicular position is thus regained, the body will not be at rest, but will possess a portion of velocity in a direction at right angles to that of gravity. It will not, therefore, be in the power of gravitation to affect that velocity; and consequently the body will proceed onwards on the opposite side of the perpendicular, in a second circular arc, which elementary writers demonstrate to be in theory perfectly similar, and equal to the arc of descent:—that is to say, the action of gravity will more and more oppose the ascending motion, as its direction becomes more remote from the horizontal line, and will at length not only destroy the motion of ascent, but again generate a falling motion precisely the same as the first. Such a pendulum would, therefore, if no other circumstances presented themselves than have yet been stated, vibrate for ever; and its vibrations would be performed in times precisely equal. But the truth is, that such a body must in actual experiment give motion to the air through which it passes; and must also overcome a certain degree of rigidity to which the string, or any other substitute in nature which might be proposed, is subject. On these accounts, the quantity of motion in the pendulous body will be continually diminished, and at length become insensible. It may also be easily apprehended, that a pendulum, though constructed with such delicacy as to move for many hours before it comes to apparent rest, could not be of much practical utility without some ready means to keep an account of its vibrations. For both reasons, therefore, a pendulum without any other addition can scarcely be used as a time-keeper.

A train of wheels, urged by a weight or spring, is therefore attached to the pendulum for these two purposes, namely, to maintain its motion by a small impulse given at each vibration, or at most at each second vibration, and to keep account of the number of times. The principle of the balance is, that its similar vibrations are performed in equal times, like those of the pendulum. The methods of adapting the train of wheels to one of these regulating organs will not therefore essentially differ from those of connecting it with the other.

other. In most of these contrivances, the pendulum or balance is, during some part of its motion, brought in the way of one of the teeth of the last wheel of the train, at the time when the whole system of wheels are at liberty to move. The tooth, therefore, strikes either the pendulum, or a part of the movement connected with it, and then slips off, or escapes from that obstacle. The whole set of parts in a time-piece by which this alternate action is effected, is for that reason called the escapement.

[To be continued.]

MATHEMATICAL AND PHILOSOPHICAL CORRESPONDENCE.

ANSWER, TO THE PHILOSOPHICAL QUESTION, p. 284.

To Mr. NICHOLSON.

SIR, FROM the description of the divergent rays seen in agitated water surrounding the shadow of the observer's head, which forms part of the question in your sixth number, I did not fail to look for the appearance on the first opportunity of sunshine which offered. I observed the fact as you describe it, and found that the radiations appear without being modified in any respect by the distance of the observer from the surface of the water. This circumstance indicated a probability, that the cause which I could not investigate by direct reasoning, might be developed by a nearer inspection than could with convenience be made on the bank of a river. With this view I placed a vessel of water, which was three feet deep, in an out-house, in such a manner that the sun shone upon its surface. When I placed myself so that the shadow of my head fell upon the water, I perceived no radiations whatever; but upon agitating the water with my hand, the water appeared full of the lines called sun-beams, parallel to each other no doubt, but which, on account of the smaller optical distance between their remoter than between their nearer extremities, had the appearance of convergence towards the shadow. As it is perfectly intelligible from the well-known truth in perspective, that all lines parallel to the line of sight will vanish in the point of sight; it remains only to be shewn how these distinct parallel beams are produced.

When the surface of the water is at rest, and forms one plane, the sun's light forms one uniform mass in which no lines are distinguishable; but when the agitation takes place, a number of convexities and concavities are produced. These scatter the rays in all directions, except at that small portion where the contrary curvatures join. The same physical effect is produced as from so many small planes; that is to say, the rays pass straight through each surface, and afford the parallel distinct beams of light. And though these small planes are continually forming and disappearing, yet there are always a certain number in existence, and the effect of their transient individual variations is nothing more than to produce an undulation in the general system of radiations.

And now, Sir, after having solved your question, as I think, satisfactorily; I shall take the liberty to propose another in the same department of science.

When the sun shines, if two bottles, for example hands or fingers, be gradually moved, so that their shadows may meet; it is observed that the shadow of the body farthest from the

the sun will swell out, and join the other shadow when the distance becomes diminished to a certain quantity, while the shadow of the body nearest the sun undergoes no alteration. What is the nature of the inflection of light which occasions this appearance?

I am, Sir, yours, &c.

A. Y.

* * The fact mentioned in the last paragraph of this letter has been stated and explained elsewhere, though I cannot now recollect where I have seen it. It does not depend upon inflection, but upon the different widths of the penumbra. When the penumbra first interfere, a faint lenticular shade is produced, which is nearer the shadow of the most remote body than that of the other; and as soon as the remoter body itself comes in contact with the bounding ray of the greater penumbra, it begins totally to intercept the sun's light on the side next its own shadow. The time employed in passing from one extreme ray of the penumbra to the other will be less the nearer the bodies are to each other, because the distance of those rays is less the nearer the vertex. The shadow of the remoter body will consequently run over the penumbra with greater speed than that of its former progress; and if any part be convex, it will advance beyond the others.

To Mr. NICHOLSON.

SIR,

HAVING observed in the 4th Number of your Journal *, and likewise in the Annales de Chimie, an extract of Mr. Prevost's Memoir on the means of rendering visible the emanations of odorant bodies; I take the liberty of communicating an observation which occurred to me a few years ago, while making experiments with different substances exposed to the oxygenated muriatic acid-gas. That which afforded the curious phenomenon to which I allude, was some highly-rectified animal oil. This matter, immediately on being exposed to the gas (which was in a very dry state), was seen to emit a copious steam, the particles of which rising to the height of about four inches above the small phial in which the oil was contained, were then observed gradually to descend, forming a very curious and pleasing appearance.—Although struck at the time with the singularity of this phenomenon, and endeavouring to account for the formation of the vapour by a supposed union of the hydrogen of the emanating matter with the super-abundant oxygen of the gas; yet I omitted at the time, and have since neglected, to endeavour to verify my conjectures by further trials.—The circumstance, however, revived in my recollection on reading your Journal; and conceiving it to be an experiment that affords a nearer approach to the absolute perceptibility of an odorant vapour, than those stated in the Memoir of Mr. Prevost, I impart it for your further investigation, if thought worthy your attention, and am

Your obliged reader;

Nov. 24, 1797.

W. HOWARD.

N. B. The oil was the only odorant substance tried: perhaps many others might afford a similar or varied appearance.

* * The philosophical consideration of odorant bodies is somewhat obscured by the old method of generalising, or referring the properties of bodies to some distinct principle or

thing supposed capable of being separated from the body itself. Thus the odors of bodies have been supposed to depend on a substance imagined in a loose way to be common to them all and separable from them. Hence the terms, principle of smell, spiritus rector; and even in the modern nomenclature we find *aroma*. There does not in effect seem to be any more reason to infer the existence of a common principle of smell than of taste. The smell of ammoniac is the action of that gas upon the organ of sense; and this odorant invisible matter is exhibited to the sight when combined with an acid gas. But in the same manner as ammoniac emanates from water and leaves most part of that fluid behind, so will the volatile parts of bodies be most eminently productive of this action; and very few, if any, natural bodies will be found which rise totally. The most striking circumstance in the effect is, that an act of such power should be attended with a loss by exhalation which is scarcely to be appreciated by weight, or in any other method during a short interval of time. But we know so little of nervous action, and of other phenomena of electricity, of galvanism, or even of heat, which strongly affect the senses but elude admeasurement by gravitation, that the difficulty of weighing the effluvia of odorant bodies becomes less astonishing.

QUESTION XI. Answered by J. F.—:—:—R.

LET H h be the altitudes of the two signals or other objects, A their direct angular distance, and a their difference of azimuth, as stated in the question. It is proved by the writers on spherics, that, in any spherical triangle, $\text{Cof. any angle} : \text{radius} :: \text{radius} \times \text{cof. opposite side} - \text{rectangle cosines including sides} : \text{rectangle sines including sides}$; which analogy, applied to the triangle formed by the zenith-distances and direct angular distance of the elevated objects, gives, $\text{Cof. } a : \text{radius} :: \text{radius} \times \text{cof. } A - \text{sine } H \times \text{sine } h : \text{cof. } H \times \text{cof. } h$; so that $\text{cof. } A \times \text{radius}^2 = \text{cof. } a \times \text{cof. } H \times \text{cof. } h + \text{sine } H \times \text{sine } h$; or, putting radius = unity, $\text{Cof. } a = \text{cof. } a \times \text{cof. } H \times \text{cof. } h + \text{sine } H \times \text{sine } h$. Q. E. D.

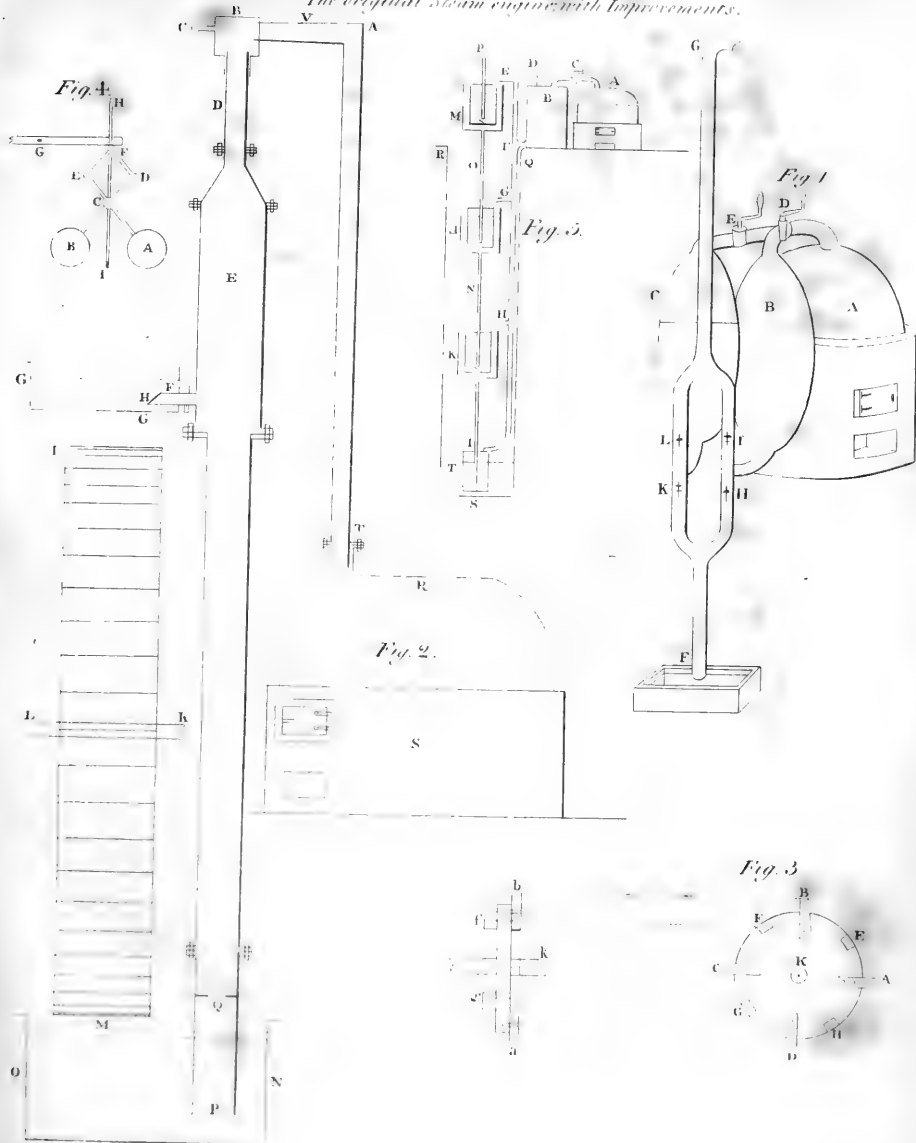
QUESTION XII. Answered by Mr. WILLIAM CASTILAU, of Uffington, Salop.

LET Z P \odot represent the zenith, pole, and sun respectively; x the cof. of $\angle PZ \odot$; then, per data and a well-known theorem in spherics, $SZ \odot \times SPZ \times \text{cof. } \angle \odot ZP + \text{cof. } Z \odot \times \text{cof. } PZ = \text{cof. } P \odot$ (rad. being 1); that is (putting d for sine of the sun's dec. $15^\circ 9'$ or .26135) $x \times x \times x + 1 - xx = d$: this equation ordered is $x^3 - x^2 = d - 1 = -.73865$, whence $x = -.66588 = \text{cof. } 131^\circ 45'$. $48^\circ 15'$ will be the azim. when equal to the lat. and alt. on the given day. The $\angle ZP \odot$ also = $2^h 3' 54''$. Per Naut. Alm. the sun's declination for noon, on the given day (at Greenwich), is $15^\circ 18' 28''$, and its change for $24^h = 18' 7''$. As $18' 7'' : 24^h :: 15^\circ 18' 28'' - 15^\circ 9' : 12^h 32' 27''$ the time from noon at Greenwich when the observation was made. The latitude of the place of observation, then, is $48^\circ 15' N.$ and its longitude ($12^h 32' 27'' - 2^h 3' 54''$) $10^h 28' 33'' E.$ of Greenwich.

☞ The same was answered by J. F.—:—:—R.



The original Steam engine with Improvements.





A
JOURNAL
OF
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AND
THE ARTS.

JANUARY 1798.

ARTICLE I.

Experiments made with a View to ascertain the Cause of Buildings, which have Metallic Conductors belonging to them, being struck by Lightning. By Lieutenant-Colonel HALDANE.

IT is not necessary, in the present communication, to enquire what may be the nature of electricity; whether it exist in the form of one fluid only, and all its phenomena depend upon the presence of a greater or less quantity than naturally belongs to all bodies; whether its effects may depend upon the influence of opposite polarities, and the operations of attractive and repellent powers in a single fluid; or, lastly, whether it exists in the form of two different, though always co-existing, fluids, produced by the union or the separation of certain gases, which belong to electric and non-electric substances, according to their various elective attractions, and which may be brought into action by the attrition of those substances, either in the form of heat or of electricity, according to their different combinations.

These, it must be admitted, are points of curiosity and importance; but at present it is only necessary to admit, what was clearly ascertained by Dr. Franklin, and since confirmed by the observations of many other ingenious and able philosophers, that there is a sameness between the matter of lightning and the electrical fluid. And electrical experiments, although the results of them cannot be compared with the effects produced in the vast and extensive operations of nature; yet, by exhibiting to our view similar appearances, they may furnish us with notions of, at least, the proximate, though we may for ever remain ignorant of the more remote, causes which produce those most wonderful, and sometimes most dreadful, effects.

Electricity, attached to the same kind of substances, shews itself in two different forms,
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which have been distinguished by the terms *positive* and *negative* electricity. These algebraical terms are well suited to describe either two different fluids, or two different states of the same fluid; for it is well known by experiment, that electricity, under these different forms, has the property (like positive and negative quantities in algebraical computations) of destroying the effects of each other, when united in equal quantities.

The enquiries hitherto made upon the subject of this paper, have in general been confined to what relates to the operation of one fluid only; but in these experiments the joint operations of the positive and negative electricities are considered, to which probably many appearances, that have hitherto occasioned much controversy amongst electricians respecting the forms of conductors, may be ascribed.

It is a known fact in electricity, that if the surfaces of two metallic bodies be placed opposite and near to each other, the one insulated, and the other connected with the earth; if a thin plate of any electric substance be introduced between these surfaces, and extending beyond their extremities, if electricity be then communicated to the electric by means of the insulated metallic substance, the other metallic substance connected with the earth has the power of putting the side of the electric with which it is in contact into the opposite state of electricity; and the positive and negative electricities on the opposite surfaces of the electric, by their attractive powers, retain each other in their respective situations. And, in solid electrics, even after the removal of the metallic substances, if the same, or other conducting substances, having a conducting communication between them, be applied again to the opposite sides of the charged electric, all electricity will disappear, silently, or with an explosion. These are the phenomena of the electric jar.

It is also well known, that if a body of atmospheric air, included between metallic surfaces, be the electric employed in the experiment above described, similar effects will be produced.

Hence it may be inferred, that the effects of lightning may arise from the discharge of large bodies of electrified air; the upper surface of which will be in one state of electricity, and the surface adjoining the earth and terrestrial bodies will be in the opposite state; and when these bodies of charged air pass over lofty buildings capable of forming a conducting communication between their opposite surfaces, the explosion of lightning takes place.

The lower surface of a body of charged air may extend over the whole, over a part, or over no part, of a building, at the time it is struck by lightning; the building forming only a part of the conducting communication.

Buildings in general have many metallic substances belonging to them: it is to these substances, and to their imperfect connection with the earth, that most of the mischief that arises from the effects of lightning is generally ascribed. To prevent this mischief, metallic conductors have been attached to buildings.

The advantage expected from these conductors is founded upon a supposition, that the matter of lightning will always prefer, in its passage to the earth, a continued conducting substance to one that is broken or interrupted by non-conducting substances; and therefore it is supposed that a conductor, constructed of a perfectly continued metallic substance, and extending from above the upper surface of a building into the earth, below its foundation, will receive and convey the matter of lightning into the earth, and prevent the mischief that

that might happen by its striking any metallic substance which is not continued to the earth.

But buildings furnished with such conductors have been struck by lightning : it is obvious, therefore, that the lightning must either have passed to the damaged part without approaching the conductor, or it must have struck from the conductor to the damaged part : for, although all metallic substances belonging to a building may be placed at a distance from the conductor, yet it cannot be supposed that any distance within the limits of a building can exceed the *striking distance* of lightning.

The apparatus with which this experiment was performed, consists of an electrical machine having a glass cylinder of about 18 inches diameter, constructed by Mr. Nairne, in the form of his patent electrical machines.

Two hollow cylinders *, made of thin wood covered within and without with tinfoil, are placed, in a vertical position, upon two insulated circular boards ; the glass pillars which support the insulated boards are about a foot in height, and are fixed into thick circular boards, covered also with tinfoil, and resting upon the floor of the room.

The interior diameter of each of these cylinders is 18 inches ; the height from the insulated board upon which it stands, eight feet six inches, including an hemispherical top ; and the distance between them is about five feet.

From the centre of each of the boards, that rest upon the floor, rises a thick glass pillar, which passes through a hole of a foot diameter in the centre of the insulated board into the hollow of the cylinder. This pillar of glass extends about six inches above the insulated board, and supports, in a vertical position, another cylinder made of wood, and covered with tinfoil.

This interior cylinder is one foot in diameter, and its height is six feet six inches, including an hemispherical top. It is placed exactly parallel to the exterior cylinder, the interval in every part being three inches. To this interior cylinder is fixed a metallic chain, by which it can be connected with the exterior cylinder, so as to form only one insulated body covered with tinfoil ; or can be connected with the earth, whilst the exterior cylinder remains insulated.

The apparatus has also a moveable insulated body, made of cork covered with tinfoil, to represent a cloud moving in the air.

And also two metallic rods placed upon insulated stands, that they may be connected with, or detached from, the earth at pleasure. These rods are used to represent the conductors, or any metallic substances that belong to buildings ; their forms are varied by fixing balls or points upon their upper extremities ; and the stand in which they are placed being hollow, they can be placed at any height by means of a screw.

To shew the effects of the passage of the electrical fluid, two insulated metallic balls are placed within the *striking distance* of each other ; and, in the interval between them, is put some inflammable substance, such as dried powdered rosin mixed with cotton.

* It may be of advantage to the reader to consult the literal references at the end of this memoir before he proceeds. N.

EXPERIMENT.

THAT this experiment might resemble, as much as an electrical apparatus is capable of exhibiting, the operations of nature, it was performed by charging and discharging plates of air.

Arrangement of the Apparatus.

THE metallic chains affixed to the interior tin-foiled cylinders are connected with the tin-foiled boards which rest upon the floor of the room. A metallic communication is then made between these boards, and another metallic communication between the exterior cylinders, which remain insulated.

The exterior cylinders, thus united, are connected with one of the conductors of the electrical machine (in this experiment it is with the positive); the other conductor (or rubber) being connected with the earth.

The electrical machine being put in action, the exterior tin-foiled cylinders become electrified; and, the interior cylinders being connected with the earth, the body of air between them becomes charged; and, if a circuit be made between the exterior and interior cylinders, an explosion takes place, and a strong electrical shock may be felt.

The exterior cylinders represent the upper surface, and the interior cylinders represent the lower surface of a body of charged air, in the atmosphere.

The moveable body, representing a cloud moving in the air, is connected with the exterior cylinders, by which means it becomes a part of the upper electrified surface of the body of charged air, and can be made to pass over the building at pleasure.

One of the metallic rods, placed in perfect communication with the earth (but separate from the communication of the interior cylinders with the earth), represents a metallic conductor attached to a building.

The insulated metallic balls, with the rosin placed in the interval between them, are supposed to represent any metallic substances belonging to the building.

A glass tube filled with water, and about 18 inches in length, is placed between the conductor and one of these balls, to represent a conducting communication, which must frequently occur during a thunder-storm, between the conductor and other metallic substances belonging to the building.

Since a conductor may be raised to any height at pleasure, it is, in this experiment, supposed to be raised rather beyond the distance at which any pointed metallic substances belonging to the building might, by their influence on the operations of the electrical fluid, endanger the building.

This experiment may be divided into three cases. 1st, The electricity opposite to that of the cloud may extend over the conductor, and all other metallic substances belonging to the building. 2^{dly}, It may extend over some metallic substances, but not over the conductor. 3^{dly}, It may not extend over the conductor, or over any part of the building.

CASE I.

THE electricity opposite to that of the cloud, that is, the lower surface of the body of charged air, is supposed to extend over the conductor, and over all metallic substances belonging to the building.

The conductor is therefore connected with the communication between the interior cylinders; and one of the insulated balls being connected by means of the glass tube with the conductor, let the other ball be connected with the interior cylinders.

RESULT.

WHEN the conductor was terminated by a point, and the cloud passed near it, an explosion took place upon the point, and the electrified air between the cylinders was instantly discharged. When the cloud passed at a greater distance, no explosion took place; but the electricity disappeared from the tin-foiled cylinders, and the plate of air between them was silently discharged, as was shewn by the electrometer.

When the conductor was terminated by a ball, and the cloud passed near it; the ball was struck, and the plate of air between the cylinders discharged as before: but when the cloud passed beyond the *striking distance* to the ball, the tin-foiled cylinders continued electrified, as was shewn by the electrometer; and the plate of air remained in a charged state.

In this case no sparks passed between the balls where the rosin was placed.

CASE II.

THE lower surface of the body of charged air is now supposed to extend over some of the metallic substances belonging to the building, but not over the conductor.

The communication between the conductor and the interior cylinders being removed, the remainder of the apparatus was arranged as in the first case.

RESULT:

WHEN the conductor was terminated by a point, and the cloud passed near to it; a spark took place on the point, and the rosin in the interval between the balls was instantly set on fire. When the cloud passed at a greater distance, the electricity of the charged air disappeared silently; as in the first case.

When the conductor terminated by a ball, and the cloud passed near to it; it was struck, and the rosin between the balls instantly set on fire: but when no spark passed between the cloud and the ball, no sparks passed where the rosin was placed, and no decrease of electricity appeared upon the tin-foiled cylinders.

CASE III.

THE lower surface of the body of electrified air is now supposed not to extend over any part of the building, and consequently not over the conductor, or over any other metallic substance belonging to it.

Therefore the metallic rod, which represents the conductor, remaining as in the last case; the metallic ball which was connected with the communication between the interior cylinders has now a separate communication with the earth only.

RESULT.

WHEN the conductor was terminated by a point, and the cloud passed near it; some weak sparks passed from it to the point on the conductor. If the cloud passed at a greater distance, no sparks appeared, but the electricity on the tin-foiled cylinders was diminished as in the former cases.

When the conductor was terminated by a ball, weak sparks passed between the cloud and the ball, and the electricity of the tin-foiled cylinders decreased; but when the cloud passed at a distance above the point at which no sparks passed, there was no variation to be observed in the electricity of the charged air.

In this case no sparks passed in the interval between the balls where the rosin was placed.

To the three cases already exhibited, a fourth may be added, to shew that the lightning may have passed to the damaged part of the building without having struck the conductor.

Although a conductor may be extended to a considerable distance above the building, yet some part of the upper surface of the body of charged air may approach as near to some of the metallic substances belonging to the building as to the conductor; and which may be exhibited in this case in two parts.

CASE IV.—PART I.

THE metallic rod, which represents the conductor, has a perfect communication with the earth only; the glass tube filled with water, which connected it with one of the insulated balls, being removed. The other metallic rod, which is to represent any pointed metallic substance, is placed upon an insulated stand, and is connected by a metallic communication with the insulated ball to which the tube of water had been attached; the other insulated ball remaining connected with the communication between the interior cylinders as before.

The lower surface of the body of charged air is here supposed not to extend over the conductor, or over the other metallic body, but to extend over some substance within striking distance of it.

That the cloud might pass at nearly the same distance from the point on the metallic rod as from the conductor, the point on the metallic rod was fixed nearly at the same height as the upper extremity of the conductor.

RESULT.

WHETHER the conductor was terminated by a point or by a ball, and the cloud passed near to it as well as to the point upon the metallic rod, sometimes the conductor was struck, sometimes the point upon the metallic rod was struck, and which instantly set fire to the rosin between the balls.

Sometimes, when a spark passed between the cloud and conductor, and no spark upon the point of the other metallic rod; sparks would appear between the insulated balls, and of sufficient strength sometimes to light the rosin.

These

These sparks were the weakest when the conductor was terminated by a point, and much stronger when it was terminated by a ball.

This circumstance seemed to shew, that the passage of the electrical fluid is not confined to one circuit only.

CASE IV.—PART II.

THE lower surface of the body of charged air is now supposed to extend over the conductor; the other metallic substances remaining in the same state as before.

The conductor is now connected with the communication between the interior cylinders.

RESULT.

WHEN the cloud passed near to the conductor, and also within striking distance of the point upon the other metallic rod; no explosion or spark appeared in any of the intervals; but the body of air was silently discharged, as was shewn by the electrometer: and the same effect was produced, when, by lowering the point upon the conductor, the cloud passed nearer to the point on the metallic rod than to the conductor.

When the conductor was terminated by a ball, the ball was struck when the cloud passed near to it; and if the ball was situated as the point was, so that the cloud passed nearer to the point upon the metallic rod than to the ball on the conductor, the point on the metallic rod was struck, and the resin instantly lighted.

CONCLUSION.

FROM this experiment it appears to be manifest, that the advantages arising from metallic conductors, erected for the purposes of securing buildings from the effects of lightning, depend more upon the lower surface of the body of charged air extending over them, than upon their form or construction.

It is not easy to decide, from the result of the experiment, whether they should be terminated by *points* or by *balls*. Probably, with respect to the large operations of nature, there may be no difference. The ball used in this experiment is three inches diameter; the largest surface of charged air scarcely exceeded 70 square feet: but if some acres of charged air were brought into action, the ball of three inches would soon be reduced to a point. However, as the subject has occasioned much controversy, we may take the opportunity of investigating it from the data afforded by this experiment.

If we reason upon what is exhibited in the first and third Cases, it is obvious that pointed conductors are to be preferred to those terminated by balls. They have the property of acting at a much greater distance than balls, and have the power of destroying, silently, the effects of lightning, when balls can only accomplish it by means of an explosion, which must always be attended with some danger.

But if we reason upon the result in the second Case, we shall find that the power which points have of acting at a greater distance than balls, will make them more liable to produce the mischief shewn in that experiment.

Under these considerations, the conductor terminated by a ball might be thought preferable to one terminated by a point: but when we recollect that most of the metallic substances which belong to buildings are generally terminated in edges and points; that they have the same influence as the point upon the conductor; that their operation may be attended with more danger, and that they may extend their influence beyond that of a conductor terminated by a ball; we may therefore conclude, that the height to which a pointed conductor is generally raised above the other metallic substances belonging to a building, when compared with the vast distances at which lightning can act, will not increase, in any great degree, the danger to which a building may be exposed under the circumstances exhibited in the second case of this experiment: and therefore conductors that are terminated by points are more likely to produce the good effects expected from them than those which are terminated by balls.

These considerations upon the effects that may be produced by the joint operations of positive and negative electricity, have, thus far, been confined to what may happen within a space occupied by a building; but they may be extended to much greater spaces upon the surface of the earth. A conductor may be struck by lightning, and (if the lower surface of the body of charged air extends over no part of the building) may convey the lightning into the earth, without the least mischief being done to the building; but when the lightning arrives at the lower extremity of the conductor, there is no proof that by its union with the earth it becomes decomposed. That decomposition cannot happen until it becomes united with its opposite electricity, which may be attached to a body of earth or substances far removed from the conductor. The lightning will therefore continue its operations until it arrives at the place where the opposite electricity is present, and in its passage will occasion much mischief if it does not meet with good conducting substances.

The accidents which have been known to happen at a distance from the place which was seen to be struck by lightning, have been ascribed (by reasoning according to the theory of Dr. Franklin) to what is called a *returning stroke*; but it might, with an equal degree of probability at least, be ascribed to the object to which the accident happened being within the circuit which the lightning makes between the opposite surfaces of a body of charged air.

London, Nov. 29, 1797.

HENRY HALDANE.

In addition to what has been already written two figures are annexed (Plate XVIII). The first is a drawing to represent a building furnished with a conductor, which is supposed to be struck by lightning.

The second is a plan of a part of the electrical apparatus, arranged as described in the second Case of these Experiments.

References to Fig. I.

A A represents the upper surface of a body of charged air.

B B B the lower surface of the same body of charged air, which is attached to the earth and terrestrial bodies, and in an opposite state of electricity to that of the upper surface A. A.

This lower surface may extend to the places marked 1, 2, 3; that is, the limit of charged air may be represented by the dotted lines A 1, A 2, A 3, as described in the three Cases of the Experiment.

C the conductor.

C F The

C F, the passage which the lightning may take, according to the second Case of this Experiment, and damage the same building at F, if the limit of charged air be at A 2; or the building near to it, if the limit of charged air be at A 3.

References to Fig. II.

A A, the exterior cylinders.

B B, the interior cylinders.

The interval between them is shaded, and contains the air which is charged.

A A, the metallic rod which connects the exterior cylinders.

B B, the metallic rod which connects the interior cylinders.

C, the conductor.

D and E, the two insulated metallic balls.

CD, the glass tube of water which connects the conductor C with the ball D.

E R, a metallic rod, which connects the ball E with the communication B B, between the interior cylinders.

F, some inflammable substance, as rosin mixed with cotton, placed between the balls D E.

G, a metallic rod, to represent any metallic substance belonging to the building, to be placed or removed at pleasure. (It was used in the fourth Case, but was removed from the apparatus in the second Case.)

H H, circular pieces of cork covered with tinfoil, fixed to a metallic rod P H.

This rod P H is fixed at O, into a glass pillar of about ten inches in height; the lower end of the glass pillar is fixed into the centre of a wheel S, which turns upon a pivot that is fixed into the table T.

The edges of the wheel S being grooved, two cords are applied to the two grooves. These cords, passing through pulleys in the table and also in the stand of it, are conveyed to the hand of the person who is turning the cylinder of the electrical machine, by which means he can make the body H revolve at pleasure.

From the rod at O rises a metallic stem, with a swivel upon the top of it. To this swivel is fixed K L, which connects it with the exterior cylinder A.

Over the top of this stem below the swivel passes a cord, fixed at one end to the centre of H, and at the other to the balance weight P, which serves to keep P H in an horizontal position.

M N, a metallic rod which connects the exterior cylinders A A with the positive conductor of the electrical machine.

The metallic rod M N is about eight feet above the floor of the room, and the moveable rod P H traverses at about six feet above the floor. H. H.

II.

New Construction of the Air-Pump. By JAMES SADLER, Esq. *Chemist to the Admiralty.*

THE construction of the air-pump, with the improvements of Mr. Cuthbertson and the Rev. J. Prince, have been explained in a former communication *. In order that the effect of this instrument may be as perfect as its theory will allow, it is necessary that its valves

* Philosophical Journal, i. 119.

should infallibly open at the requisite periods of the operation, and that the space between the piston and the upper or the lower valve, when at its extreme positions, should be the least possible, or rather that there should be no vacuity remaining. It has been shown that in the air-pump of Prince the lower space is absolutely out of the question, because there is no lower valve; and the upper space may be, and probably is, much diminished by the effect of the oil. The air-pump of Cuthbertson, in which the construction of the valves is most admirable, and their action absolutely secured, seems to possess a still greater portion of this last advantage from the circulation of the oil, though not directly intended to answer this purpose. Mr. Sadler has directed his attention immediately to the good effects which a due application of that fluid is calculated to produce, and has constructed an air-pump, possessing the desirable requisites of simplicity, cheapness and power, of which Figure 1. Plate XIX. is a sketch.

A B represents the barrel, Q a solid piston or plunger fitted into the barrel, and leathered somewhat loosely. O is the piston-rod, capable of being raised and depressed in the usual way by a handle and toothed wheel which acts upon the straight rack-work. The barrel communicates, by means of the pipe C, with a chamber D, at the upper part of which is a valve K, opening upwards into a cistern L. P is the plate for the receiver, through which a communication-pipe N E passes to the interior part of the vessel, D. At the lower extremity E of this pipe there is a valve, connected with the lever F H, by a wire F I passing through a collar of leathers. G is the axis of the lever F H, upon which, by means of a weight at H, an action is produced which presses the valve E upwards into its place. H I is another wire or tail, by means of which the extremity H of the lever may be raised, and consequently the valve E opened.

From these particulars the effect of the machine may be explained. Let the piston be drawn out and the barrel filled with oil, which will of course rise to the same level in D. Let the piston then be put in and pressed downwards by turning the handle. The oil will be driven from the barrel into D, driving the air before it through the valve K so completely, that a portion of the oil itself will follow into the cistern L. Let the piston be again drawn upwards, and the oil will follow, because D is higher than the valve, and will consequently leave an empty space in this vessel D. As soon as the piston-rod has arrived at a certain height, it will act by means of a prominence upon the catch I, and open the valve E. A portion of the air in the receiver will therefore rush into D. Upon the return of the piston the valve E will in the first place shut, and the air in D will be extruded through K. This extrusion will be as complete as before, because at every stroke a portion of oil from the cistern L will pass through the pipe L M to the upper part of the barrel, and escape beside the piston while rising, on account of the loose manner in which it is leathered. From this contrivance it is provided that the internal part of the machine shall contain oil sufficient not only to displace the air, but even to pass through in a small quantity after it by means of the valve K; and consequently, by working the apparatus the exhaustion may be carried to any desired degree. Fig. 2. represents the same machine viewed in another direction.

In the present pump the valve K may be considered as representing the valve which closes the upper part of the barrel in Cuthbertson's pump, and the valve E as the lower fixed valve. We see therefore from this combination that the solid piston is made to act both ways, in
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the same manner as if a side valve had been made in Cuthbertson's barrel, opening outwards beneath the piston, and the piston itself had been made solid. For in such an arrangement the rising stroke would have drawn the air out of the receiver, and the descending stroke would have driven it out sideways. But Mr. Sadler has removed the objections which might be made to such a disposition, by making his piston in effect fluid, and therefore capable of adapting itself to every cavity or irregularity in the vessel D.

How far it might be found, in process of time, that the oil might become changed by the circulation, and less fit for the purpose; whether it might carry minute bubbles of air along with it, and whether mercury or any other fluid in vessels of suitable materials might be preferable, I know not. But thus much is certain, that the means of constructing an instrument of this kind, with a few slight variations to facilitate the execution, are within the power of many ingenious philosophical men who have neither the ability nor the skill to procure a good air-pump of the other constructions.

Fig. 3 represents a barrel in which the imperfection of a space between the piston and lower valve is obviated, and the action of this last valve secured. A is represents the barrel; O, the piston rod; C, the valve in the piston; I I, the leathers; L L, a hollow cylindrical piece of metal, which nearly fits the cavity M M, which surrounds the lower valve D and its tube. The piece L L does not touch the barrel, and there are two holes, K K, communicating with the central cavity in the piston, from the space between L and the barrel. This piston also is not leathered very tight, because it is intended that the pressure of the atmosphere shall force a portion of oil into the lower cavity of the barrel during the ascent of the piston. P is the tube which communicates with the receiver. N is a stopping-screw serving to close a hole through which any superfluous oil may be drawn. G E is a lever applied to open the lower valve D, when the piston has risen to a certain height, and by a proper communication acts upon the extremity G by the wire H. Let it be supposed that the piston is down, and that oil is poured into the barrel above it. While the piston is drawn up, a portion of the oil will insinuate itself past the leathering into the partial vacuum below. As soon as the piston has risen nearly to its limit, the valve D will be opened, and part of the air in the receiver will pass into the barrel through P. Upon the return of the piston the valve D will in the first place close; and when the piston shall arrive at its lower station, the oil in M M, being displaced by the piece L L, will rise through all the cavities of the piston, and drive out whatever portion of air may not have escaped through C during the descent. And since every rising stroke of the piston will be attended with the transition of a portion of the oil from the upper to the under part of the barrel, the space M M will, at the end of every descending stroke, be found to contain not only a sufficient quantity to fill the place which would else have been occupied by air, but also another portion, which will pass through C at the termination of the stroke, and complete the effect.

III.

Observations on Phosphorus. By Citizen BRUGNATELLI, Professor of Chemistry, &c. at Pavia.*

IN one of the public meetings of the Laboratory of the University, when the subject related to the peculiar properties of Kunckel's phosphorus, and required those experiments to be repeated upon which Gœtting has built his theory in opposition to that of the French chemists; I repeated the valuable experiments of Fourcroy and Vauquelin, which are described in the present volume †, which shew the error of the German chemists; and as I have observed some peculiar circumstances not mentioned by the Parisian chemists, I flatter myself that a detail of these experiments will not displease my readers.

EXPERIMENT I.—*The rapid Solution of Phosphorus in pure Oxygen Gas ‡; and its Combustion, which takes place by the Mixture of other Gases.*

I introduced a cylindrical piece of phosphorus supported on a tube of glass into a long jar, the upper part of which contained six cubic inches of oxygen gas obtained from nitre. The rest of the jar was full of water. It was formed of thin white glass, three inches in diameter; and the experiment was made in the most perfect darkness with the hydro-pneumatic apparatus. The thermometer of Reaumur stood at 12 degrees above 0, (59, of Fahrenheit). After the phosphorus had remained in this position for one minute, it was withdrawn, and a few bubbles of inflammable gas were introduced. This gas was obtained by dissolving iron in the sulphuric acid § diluted with water, which was constantly used in these experiments. As soon as the bubbles of this gas had entered the phosphorated oxygen gas, the whole mass of elastic fluid beneath the glass appeared phosphorescent. Azotic gas, obtained from animal fibre by means of nitric acids, produced the same effect.

In some instances the phosphorus introduced into the oxygen gas produced a slight phosphoric vapour, which immediately ceased; but the same phenomenon was produced by introducing inflammable gas into this gas. But when by accident the phosphorus introduced into the oxygen gas began to shine; the gas itself, which had thus been placed in contact with the phosphorus, no longer shone, even after the expiration of some time by the addition of the afore-mentioned gases, in whatever proportion they were added, unless the white vapour was condensed and the phosphorus extinguished.

* *Annali di Chimica e Historia Naturale*, tomexiii. ann. 1797, page 275. I do not possess this work, but have followed the translation of Citizen Van Mons in the 24th volume of the *Annales de Chemie*, whose accuracy and knowledge of the subject afford the most perfect assurance of his fidelity.

† *Ibid.* page 3.

‡ The author calls this gas *therm-oxygen*.

§ The author calls this acid *oxi-sulphuric*. He prefixes to the name of each acid the syllables *oxi*, such as *oxi-nitric*, *oxi-phosphoric*, &c.

M. Van Mons has preferred the nomenclature presented by the French chemists; and justly remarks, that, if every chemist were to make one, it would soon become impossible to distinguish the substances concerning which they might write.

EXP. II.—*Phosphorated Oxygen Gas becomes luminous by the Contact of the Oxygenated Muriatic Acid Gas.*

AFTER having left a cylinder of phosphorus for one minute in several cubic inches of pure oxygen gas, in which it did not shine at all, nor affect the transparency by white vapours when brought into the light; a bubble of oxygenated muriatic acid gas, obtained by distillation of the ordinary muriatic acid from the black oxide of manganese, was introduced. The mixture of the two gases produced a soft light through the whole mass of elastic fluid contained in the vessel.

EXP. III. *Phosphorated Oxygen is also luminous with Nitrous Gas.*

NITROUS gas is very greedy of oxygen, in order to convert itself into nitrous acid. The nitric acid suddenly takes this gas from the oxygen gas, while heat is developed, and the combination is shewn in red vapours. I was desirous of ascertaining what would happen when nitrous gas was introduced into phosphorated oxygen gas, according to the usual process. As I performed my experiment in the dark, I could not observe the moment in which the nitrous gas arrived at the upper part of the jar in contact with the phosphorated gas. A very perceptible azure light was seen, which was more considerable than had been afforded by the use of any other kind of gas. A second introduction of nitrous gas produced no further effect. The vessel being afterwards inspected by the light of a candle, the mixture of the two gases appeared so opaque, and the glass so loaded with white vapours, that it appeared as if filled with milk.

EXP. IV.—*Phosphorus is soluble in Hydrogene Gas.*

INFLAMMABLE gas dissolves phosphorus as speedily as oxygen gas in the first Experiment, and the whole volume of this gas is impregnated and becomes phosphorescent by the contact of oxygen gas, or the oxygenated muriatic acid gas. But, in order to observe this phenomenon in perfection, it is necessary to operate with pure inflammable gas, which has not long been in contact with water, because this would prevent its acting upon the phosphorus with the same energy; and as this gas absorbs a certain portion of pure air from the water, it would follow that the phosphorus would produce more or less of white vapour, and readily emit light. It is therefore necessary to operate over mercury.

EXP. V. VI. and VII.—*The Appearances of Phosphorus in the Oxygenated Muriatic Acid Gas.*

A PIECE of phosphorus was introduced into a cylinder of glass filled with oxygenated muriatic acid gas over water. It did not burn perceptibly in the dark; but in the light, white vapours were seen through the whole of the glass in such plenty as to render it opaque. A short time afterwards the phosphorus was fused and ran like wax by heat, or camphor by the nitric acid. The phosphorus in this state floated on the water, instead of falling to the bottom as before its fusion. The temperature of the atmosphere at the time of making this experiment, was about ten degrees above 0. I put a large cylindrical piece of phosphorus into a crystal decanter, placed on a table, and full of very pure oxygenated muriatic acid gas. The capacity of the bottle was eight cubic inches, and it was entirely full. The phosphorus took fire with flame, and threw out a great number of very brilliant sparks against the sides of the vessel, which it heated, and itself became entirely fused. The vessel was filled

filled with white vapours. Afterwards, in proportion as the vessel, which was not broken in this experiment, became cool, the phosphorus congealed, but continued to shine, though without emitting sparks. When the vessel was opened under mercury; the metal entered, and filled one third of its capacity. The gas which remained after the rapid combustion of the phosphorus was tried over mercury; first, by mixing oxygene gas, and afterwards the hydrogen and azotic gases: but no light or other phenomenon appeared, to deserve attention.

I covered the ball of a thermometer with two grains of phosphorus melted in hot water, and immediately conveyed this instrument, which marked 10 degrees above 0, into a small bottle full of oxygenated muriatic acid gas. I soon observed white vapours, by the assistance of a light; for the experiment was performed in the dark. The phosphorus became soft, flowed, and took fire; and the mercury in the thermometer was seen to rise from 10 to 50 degrees.

EXP. VIII.—*The Phenomena of Phosphorus in the Carbonic Acid Gas.*

AS the carbonic acid gas, at a low temperature, passes readily enough through water, without being absorbed, to be subjected to experiment even in the hydro-pneumatic apparatus; I disengaged this gas from the carbonate of lime by means of the diluted sulphuric acid. I collected the purest gas, and included six cubic inches in a large tube closed above, and having its lower end plunged in a vessel of water. A cylinder of phosphorus which was introduced in the dark did not immediately shine, but after about two minutes it began to emit a glimmering light, which increased to a greater degree of brilliancy than would have been exhibited in atmospheric air. The temperature was about ten degrees above zero.

Little satisfied with these trials, I repeated the same experiments over mercury, and obtained the following results. The phosphorus did not shine in the pure carbonic acid gas, and did not appear to me to be dissolved in the same manner as in some of the preceding gases; for the carbonic acid gas mixed with oxygene gas did not afford any light, though it had been long in contact with a large cylinder of phosphorus. Consequently the light which appeared in the carbonic acid gas over water depended on a portion of pure air which was disengaged from the water itself, and mixed with the carbonic acid gas, while this last combined with the water, and diminished the affinity of that fluid for the portion of oxygene it usually holds in solution.

EXP. IX.—*Some Phenomena of Phosphorus with Atmospheric Air.*

I PUT several pieces of phosphorus into two phials of crystal glass, of the capacity of about four cubic inches, half filled with pure water, and the other half containing atmospheric air. These phials were agitated from time to time: after three days, the temperature of the air being at 18 degrees above zero, the water in the bottles emitted a very perceptible smell of phosphorus, and was a little turbid. The phials being afterwards closed and gently agitated in obscurity, I observed that the included air, which was at first obscured, immediately shone by a flame which struck the inside of the phial. By the assistance of light it was seen that the transparency of the air was affected by white vapours. When the vapours had ceased, I agitated the phial again in the dark, and the same phenomenon appeared,

appeared, and was frequently renewed, by brilliant points from the surface of the water; Some of which produced the illumination through the air. I saw the same thing several times, as did likewise a number of spectators, who were much surprised at the effect.

I at first supposed that the water of the phial in which the phosphorus was placed might hold suspended certain small particles of the phosphorus, which were afterwards dissolved by the atmospheric air of the vessel; and that some part of this phosphorus, afterwards taking fire in consequence of the agitation of the water, might produce through the whole of the air, the flame I have here spoken of. But the ready combustion of phosphorus in atmospheric air at the temperature of 18 degrees contradicted this explanation; not to mention that it was difficult to conceive that the air should occasion the solution rather than the combustion of this substance. I could not even suppose that the atmospheric air of the vessel was decomposed by the phosphorus dissolved in the water, or by very small particles of this substance which might float upon the liquid; so that the azotic gas remaining alone might act on the phosphorus, partly dissolving it, and by this means constituting a phosphorated gas, which might shine by the agitation of the water, in consequence of the disengagement of a quantity of oxygene sufficient to occasion combustion. For I had often observed that, when the phial was uncorked with the greatest care, in order to avoid any agitation, there was no production of flame notwithstanding the access of atmospheric air; but the phosphorescence appeared immediately upon agitating the phial, whether the phosphorus had or had not a communication with the air of the atmosphere.

On one occasion, when I had six phials prepared in this manner, and observed them for some time in the dark, I saw that some of them afforded a flame spontaneously, and without agitation.

I attributed this flame which appeared in the air of the phials, containing water charged with particles of phosphorus, to a combustion and inflammation of the phosphorus, produced, at the instant of the agitation of the surface of the water, by the immediate solution of this combustible in the atmospheric air. I imagined that this phenomenon might be reproduced as long as there was any oxygene gas in the phial. It might, in fact, take place for a number of times successively, because a very small quantity of oxygene is required for the purpose. The agitation of the water appeared to me to be necessary to detach the covering of phosphoric acid with which the parcels of phosphorus might easily be covered, and which might hinder their combustion. more particularly when the phosphorus was very much divided; that, if this covering should be spontaneously detached, either by elevation of temperature or any other circumstance, the phosphorescence must appear in the end of the bottles even without agitation; and that the spontaneous flashes are produced in this manner.

I took a piece of phosphorus, which I caused to burn in the air of a phial of the same capacity as those employed in the foregoing experiment, at the bottom of which was a little water. The phosphorus, suspended by a silk thread in the upper part of the bottle, burned visibly (the temperature being 15 degrees above zero), and, as usual, emitted white vapours, which descended to the surface of the water. I left the phosphorus in the air of the bottle until it no longer shone, which effect happened in the course of the day.

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The bottle, when observed in the dark, exhibited no light; the water agitated in every direction produced no light; but it was seen to inflame immediately upon the contact of the atmosphere by taking out the cork; and when the light disappeared, it was capable of being produced again by agitating the same water, which immediately before did not exhibit any light by the strongest agitation.

In this experiment the flame likewise arose from the phosphoric particles on the surface of the water, with the vapours of the phosphorus which it condensed by burning. The total want of light in the included air before it was open arose from the total want of pure air, which is necessary for the production of this phenomenon.

EXP. X.

I DISSOLVED a small piece of phosphorus in pure oil of turpentine. The oil became slightly turbid, afterwards clear, assumed a yellowish colour, and emitted a slight smell of phosphorus. A crystal phial was half filled with this solution. I was desirous of observing whether agitation would afford a flame in the air, as in the former experiment; but I did not perceive the least light. I observed the same thing with the prepared oil of thyme, containing phosphorus. The temperature of the atmosphere was 15 degrees above zero.

EXP. XI.—*Observation on Phosphorated Alcohol.*

PHOSPHORUS dissolves in alcohol when it is very pure. In proportion as the solution is effected, the alcohol loses its proper smell, and assumes that of phosphorus, which is very disagreeable. The alcohol takes up very little of the phosphorus, and continues entirely limpid. I attempted, but constantly in vain, to observe the phenomenon of the flame in the air of the vessels, which I had partly filled with phosphorated alcohol. Its habitude was in every respect similar to that of phosphorated oil of turpentine at the ordinary temperature of the atmosphere.

EXP. XII. *Water causes Phosphorated Alcohol to shine very strongly.*

IF in perfect darkness a drop of phosphorated alcohol be poured into a crystal decanter at the bottom of which is a small quantity of pure water, a very singular phenomenon is observable. At the moment when the phosphorated alcohol comes into contact with the water, a brilliant light is disengaged, which passes with a serpentine motion, and rapid, crackling along the surface of the water. The air within the vessel then becomes entirely luminous, and remains for some time in this state. If the bottle be unclosed, and air blown into it; the white vapours disappear, the air becomes transparent, and the phosphorescence disappears: but if the mixture be again agitated, several luminous points are seen at its surface, the air of the bottle becomes again inflamed; and this may be renewed until the whole of the phosphorated alcohol has been consumed.

If a pen be dipped in water, and in this state it be plunged in phosphorated alcohol; at the moment of contact of the two fluids a sudden light spreads through the air, and very perceptible flames are seen, in a dark place, to issue from time to time out of the mouth of the phial.

EXP. XIII. *Water is not the only Substance which sets fire to Phosphorated Alcohol.*

I HAVE observed that the phenomenon of the inflammation of phosphorated alcohol not only takes place with cold and with boiling water, but also with concentrated sulphuric acid,

acid, with the solution of pot-ash, with the liquid nitrate of lime, and with the solution of various alkaline salts.

EXP. XIV. *Sulphuric Ether dissolves Phosphorus.*

I LEFT, during sixteen days, in a well closed crystal decanter, two cylinders of good phosphorus, with some ounces of sulphuric ether.

This liquid did not appear to have lost its usual smell, as was the case with alkohol. It was very transparent; and, upon being decanted into another bottle and agitated in obscurity, there were no signs of phosphorescence. I afterwards poured some drops into water which I had put in another bottle; but here also I observed no appearance of light, though the place of operation was perfectly dark.

The phosphorated ether was seen in the light floating upon the water, and very limpid. I added a small quantity of alkohol to this mixture, which immediately rendered it turbid like milk. This phenomenon may afford a sure means of ascertaining the purity of ether. The mixture agitated in every direction afforded no light; but light appeared with much vivacity and at several repetitions after the mixture had been a little heated. A great number of luminous points appeared on the surface of the water after it had been long agitated. I made this experiment in a bottle of a certain size, closing and unclosing it sometimes in order that the white vapours might issue out, in proportion as they were formed.

The following are the consequences which I have deduced from the foregoing experiments:

1. Phosphorus is very readily dissolved in pure oxygen gas at a mean temperature (*Exp. I.*) without emitting light; and the phosphorated oxygen gas shines when it is diffused in any other azotic or mephitic gas. (*Exp. II. and III.*) This phenomenon appears to me to depend on the agitation which the integral parts of the oxygen gas and phosphorus undergo when another gas is mixed with them, and from the diminution of affinity between the integral parts even of the oxygen gas, effected by the addition of the new gas. By this diminution of affinity, if phosphorus is placed in the sphere of chemical attraction with the oxygen, the oxygen gas becomes decomposed, and the phosphorus burns.

2. Phosphorus is likewise speedily dissolved in inflammable gas (*Exp. IV.*) and this phosphorated gas burns when it is brought into contact with any oxygen gas.

3. The oxygenated muriatic acid gas suddenly burns phosphorus. The oxygen gas which contains this acid is decomposed by the phosphorus, which rapidly combines with the oxygen (*Exp. V. VI. VII.*) to form phosphoric acid; and the concrete caloric, or other component principles of the oxygenated muriatic acid gas, being set at liberty, raises the temperature, and melts the phosphorus which is not yet oxygenated.

4. Phosphorus is not soluble in pure carbonic acid gas (*Exp. VIII.*); nevertheless, at the temperature of 12 degrees above 0 of the thermometer of Reaumur, it burns better in this gas mixed with a small portion of oxygen gas, than when in atmospheric air at the same temperature.

5. Water does not dissolve phosphorus, but it holds very small particles in a state of suspension, more especially those which are impregnated with a principle of combustion,

and have been fused by caloric (*Exp. V. VI. and VIII.*). The air of the atmosphere dissolves phosphorus at the moment it burns it, and the solution is sensibly phosphorescent when the experiment is made in a crystal glass bottle. This causes a flame more or less brilliant, in proportion to the greater or less quantity of phosphorus which is burned at the same time.

6. Phosphorus which is burned in a decanter with water at the bottom, not only decomposes the oxygen of the air in which it burns, but likewise that of the air which is found mixed with water (*Exp. X.*).

7. Oil of turpentine and oil of thyme, containing phosphorus, do not burn in contact with atmospheric air at the ordinary temperature (*Exp. XI.*).

8. Alcohol dissolves phosphorus, and loses its agreeable smell. Phosphorus shines in the atmospheric air of closed vessels, by pouring phosphorated alcohol upon some substance which shall combine with the liquid and separate the phosphorus; such are water, sulphuric acid, &c. (*Exp. XII. XIII. XIV.*)

Sulphuric ether likewise dissolves phosphorus, and this solution is not decomposed by water, upon which it floats; but when phosphorated ether is diluted with alcohol, or is changed into anodyne liquor, it then combines with water, and quits the phosphorus, which, being divided into very fine white particles, gives the mixture the colour of milk.

IV.

On the Advantage of inverting the Slider in many Operations on the Common Sliding Rule.
By the Rev. W. PEARSON, of Lincoln.

To Mr. NICHOLSON, Author of the "Journal of Natural Philosophy," &c.

SIR,

ON perusing the eighth number of your Journal, I was much pleased with the ingenious instruments mentioned in the VIIIth article, upon which the graduations of Gunter's Line of Numbers are so disposed as to be very considerably enlarged, whilst yet the whole range of a single line is contained in a small compass. If an instrument of either the parallel, circular, or spiral construction, of a convenient size, were manufactured for sale, with a proper degree of accuracy, I am fully persuaded that considerable advantage would be derived therefrom by purchasers; particularly by those persons whose profession is to make practical calculations. But as the public are in possession of a sliding rule, in the operations of which long usage has made every mechanic expert, it is to be apprehended that prejudice would in this, as in other instances, prove an obstacle to the general sale of such an instrument, were the introduction of one into general use attempted. This observation is, I believe, verified by the slow sale of the patent, and also of the new improved sliding rule, in comparison of that of the common one, amongst artificers, who constitute the most numerous class of purchasers. I have been informed by a reputable tradesman, who retails sliding rules of the different constructions, that for every patent, or new improved sliding rule that is purchased, more than twenty of Cogshall's, or the common sliding rules, are sold.

fold. By the same prejudice, probably, any *new method* of working by the *old rule*, though more expeditious and equally accurate, would be rejected in practice if such method were offered to the public. But, be this as it may, I think the consideration that the probability is but small of any particular improvement being generally adopted in practice, is not a sufficient reason why that improvement should be withheld from the public eye, whilst it is considered as such. There are always individuals whose minds are unfettered by prejudice*, and to whom any hint tending to produce an improvement always proves acceptable.

The logarithmic line of numbers, known by the name of Gunter's line, is so well understood by every one who is acquainted with the properties of logarithms, as to render any observation on the construction of it unnecessary. The use of two such lines, usually denominated A and B, working together by means of a direct slider, supercedes the application of compasses, and is taught in most practical books of arithmetic.

There is however another, and I think more commodious method of applying the slider, than the one generally practised (except in working direct proportions), which I have not seen explained, nor practised by any person; and which, therefore, I shall beg the favour of you to communicate to the public through the medium of your interesting Journal, provided you deem it of sufficient importance. The method I have to communicate lays no claim to superior accuracy when compared to the common method, the powers of the instrument being the same in both; but, besides novelty, I trust it will be found to have facility to recommend it.

The method is simply this: Invert the slider B on any common sliding rule, whereby the numerical figures will ascend on it, and on the fixed line A, in contrary directions: now, as the distance from unity to any multiplier, on Gunter's line, will invariably extend from any multiplicand to their product, it follows, that if any particular number on the inverted slider B be placed opposite to any other given number on A, the product of those numbers will stand on the slider B, against unity on A; for, in any position of the inverted slider, the distance from unity to the multiplier on A, instead of being carried forward on B, as when the slider is in a direct position, is brought back thereby to unity again; so that unity (or *ten* on single lines where the slider is too short for the operation) is invariably the index for the product of any two coincident numbers throughout the lines.

In division, by the same process, if the dividend on B be put to the index, or unity on A, the division and quotient will coincide on the two opposite lines; so that when one is given, and sought for on either line, the other is seen on its opposite line at the same time.

* This prejudice is the effect of habit, and can seldom be eradicated from the minds of such individuals as consider the ready occurrence of a proposition as a test of its truth. To establish a new philosophical theory has, in every instance, required time sufficient to educate an entire generation of men. The rejection of the Aristotelian philosophy, the adoption of experimental research, the substitution of the doctrine of gravitation instead of that of vortices, and the rejection of phlogiston by our contemporaries, are sufficiently illustrative of this assertion. New practices are still more difficult to be introduced. The new grammar, the new rudiments of science, or the new instrument, however superior to the old in simplicity, facility, and truth, must be less valuable to the ordinary teacher or artisan, whose memory is familiarized with the precepts of the latter, and whose only ambition is to earn his subsistence with the least possible exertion. N.

The next operation which offers itself here is reciprocal proportion, which can be effected by no other method than by inverting the slider, but which is rendered as easy by this application, as direct proportion is in the common way; for if any antecedent number on B inverted be set to its consequent on A, any other antecedent on B, in the same position, will stand against its consequent on A, so as that the terms may be in a reciprocal ratio. In squaring any number, it will appear from what has been already said, that if the number to be squared be placed on B, inverted against the same on A, the square will stand on B, against unity on A. Therefore, to extract the square root of any number, let that number on B stand against unity on A; and then wherever the coincident numbers are both of the same value, that point indicates the root. If two dividing lines of the same value do not exactly coincide, the coincident point will be at the middle of the space contained between those two which are nearest a coincidence; and as there is only one such point, there can be no mistake in readily ascertaining it. The finding of a mean proportional between any two numbers is extremely easy at one operation; for if one of the numbers on B inverted be set to the other on A, the coincident point of two similar numbers shews either of those to be the mean, or square root of their product, according to the preceding process. Thus have we a short and easy method of multiplying, dividing, working reciprocal proportion, squaring and extracting the square root, at one position of the inverted slider, whereby the eye is directed to only one point of view for the result, after the slider is fixed: whereas, by the common method of extracting the square root by A and B direct, the slider requires to be moved backwards and forwards by adjustment, the eye moving alternately to two points, till similar numbers stand, one on B against unity on A, and the other on A against the square number on B; which square number, in the case of finding a mean proportional, must be found by a previous operation. Hence, for more convenience in the extraction of roots, and measuring of solids, an additional line called D, has been added to the rule, which renders it more complex, and consequently seldom understood by an artificer. Upon examining the patent rule with a graduated piece of brass, to be attached to, or detached from the slide at pleasure; and also Mr. Horton's new improved rule with two sliders; I perceive that the method of multiplying length, breadth, and depth into one another at one position, in which their excellence consists, is nothing more than a combination of the two methods on the common rule; for the product of the two first numbers, effected by means of an inverted and a direct line, forms the multiplier to be used with the third number, by a third direct line, at the same position of the sliders. Hence, a gauger's rule will answer the same purpose, if an additional slider be held in a proper situation inverted. No reason, however, has been given for the effect of this process in any book that I have yet seen, nor of the application of an inverted slider to any other species of calculation*.

* In reply to the obliging postscript of the author of this paper, I have only to remark, that the approbation of the intelligent cultivators of science is the most estimable reward of my endeavours to diffuse knowledge; and that inclination, as well as duty, will induce me to pay every attention to his future communications. N.

V.

Abstract of a Memoir entitled "Enquiries concerning the Nature of Prussian Blue"
By Mr. PROUST*.

IF iron were susceptible, as chemists imagine, of uniting with oxygen in all the intermediate proportions between $\frac{1}{100}$ and $\frac{4}{100}$, which appear to be the two extreme terms of its union with that principle, ought it not to afford, with any given acid, as many different combinations as it is supposed capable of producing oxides? Why, for example, does not this metal, which affords, with the sulphuric acid, a salt constant in its properties when oxidized no farther than $\frac{1}{100}$, exhibit different combinations, equally constant in their respective properties, when it contains $\frac{3}{100}$, $\frac{3}{100}$, or $\frac{4}{100}$ of oxygen?

A great number of facts prove, on the contrary, that iron does not rest indifferently at all the different degrees of oxidation between the two terms abovementioned; and, notwithstanding the different degrees of oxygenation through which iron is supposed to pass when its sulphate is exposed to the air, we are acquainted with no more than two sulphates of this metal.

The first is the green, or crystallizable sulphate, in which Lavoisier has proved that the iron contains $\frac{1}{100}$ of oxygen. This salt, when pure, is insoluble in spirit of wine; its solution in water is of a very pale sea green colour. It is not altered by the gallic acid, affords no blue with alkaline prussiates, &c.

The second kind of sulphate, no less constant in its properties, is that red deliquescent incrySTALLIZABLE combination which is soluble in alcohol, and is known by the name of mother water of vitriol; but is not really such, unless when it produces no alteration in the oxygenated muriatic acid; that is to say, when its oxide contains $\frac{3}{100}$ of oxygen.

This sulphate is easily obtained by treating it with nitric acid until its solution no longer disengages nitrous gas, by the addition of a new quantity of acid.

This sulphate, besides, possesses exclusively the property of forming a black precipitate with the gallic acid, and of affording Prussian blue with the alkaline prussiates, as will hereafter appear.

Between these two sulphates, which Mr. Proust calls the green sulphate and the red sulphate, there is no intermediate point. If the green sulphates, by exposure to the contact of air, assume a colour which appears to belong to neither of these species, it may be decidedly shown that they are simply a mixture of the two, by separation by means of alcohol. Each salt will then exhibit all its distinctive properties. The green sulphate will constantly produce a green precipitate with the caustic alkalis; which precipitate will soon become black if preserved under water without the contact of the air, because its particles continually approach each other, and render the colour more and more intense.

The red sulphate, on the contrary, will afford, with the same alkalis, a yellow red pre-

* This abstract is translated from the Annales de Chimie XXIII. 85. The editors do not say where the original is to be found.

cupitate, which is an oxide, incapable of attracting any thing from the air of the atmosphere, nor from the oxygenated muriatic acid.

This oxide, according to the experiments of Mr. Proust, contains $\frac{4}{11}$ of oxygene, and is capable of forming the basis of a series of combinations, which bear the same relation to those which afford the black oxide, as the red sulphate of iron bears to the green sulphate.

So that in whatever degree of alteration an ordinary sulphate may be examined, it will be found to consist merely of these two salts, mixed in different proportions.

Whence it follows, that a sulphuric or muriatic solution of this metallic substance is nothing but a mixture of the two salts, one of which has for its basis the oxide of iron, containing $\frac{2}{3}$ of oxygene, and the other the same oxide, containing $\frac{4}{11}$ of oxygene. And as little attention has hitherto been paid to the distinctive properties of these salts; effects have frequently been attributed to the one, which belong exclusively to the other.

We must therefore distinguish two sulphates, two muriates, two arseniates, two prussiates, &c.: the latter salts more particularly constitute the object of Mr. Proust's Memoir.

The White Prussiate.

IN order to obtain this salt, a solution of the perfectly pure green sulphate must be taken and mixed with a well saturated solution of the prussiate of pot-ash. This last solution, if perfect, affords crystals of a beautiful orange-yellow in tetrahedral pyramids, truncated near their base.

The green sulphate of iron is obtained and preserved in a state of purity, by keeping it in a well closed vessel quite full, in which a small piece of iron or tin is put. The same purpose is answered by reducing the red oxide, which may exist in the solution, to the state of black oxide, by a mixture of water charged with sulphurated hydrogen gas. This sulphate, after purification, ought not to change by the addition of the gallic acid. In this state, its colour is an extremely weak shade of sea-green. The green colour does not appear, unless the solution be in contact with the atmospheric air.

After having poured the solution of the prussiate upon the sulphate of iron, the bottle must be immediately closed. A plentiful white precipitate immediately falls down, which soon afterwards assumes a green tint, occasioned by the air contained in the bottle, and likewise by another cause. But if the vessel remain closed this shade does not become deeper, and light alone does not produce any change.

It is advisable to pour an excess of the prussiate upon the sulphate, in order to complete the decomposition. After several hours repose, this pale prussiate is covered with a yellow liquor, which is a mixture of the prussiate and the sulphate of pot-ash. This liquor retains in solution a small quantity of white prussiate, which acquires a blue colour by absorption of oxygene, when it is placed in contact with the atmospheric air. By this union, it becomes insoluble, and falls upon that which is at the bottom. The last-mentioned precipitate receiving in its turn the impression of the atmosphere, soon becomes blue at its surface. The intensity of colour successively increases, and proceeds downwards, until at length the white prussiate becomes converted into Prussian blue.

The action of the oxygene of the atmosphere, in these circumstances, is still more confirmed by the phenomena which are seen when the white prussiate is exposed to the action

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of the air on a filter. It becomes blue by absorbing oxygene; and the colour does not acquire its greatest intensity until the oxide of iron has obtained $\frac{7.5}{100}$ of that principle.

Of all the saline combinations of iron, there is none of which the basis becomes saturated with oxygene so rapidly as the prussiate. The carbonate of iron is not to be compared with it.

The sulphuric and muriatic acids poured on the white prussiate of iron occasion no change. The oxygenated muriatic acid, on the contrary, enlivens the prussiate in an instant, and loses its odour. The nitric acid likewise converts it to blue, but more slowly, because it does not abandon its oxygene with the same facility as the oxygenated muriatic acid.

The solution of sulphurated hydrogen, which does not alter the green sulphate, as has already been remarked, has likewise no action on the white prussiate. It merely reduces to white the small quantity of prussiate which had become blue by the access of air; or, in other words, it deprives it of that portion of oxygene which, it may already be seen, constitutes the difference between the white and the blue prussiates.

It may perhaps be an anticipation of the facts, to distinguish that prussiate by the name of white, which in strictness is such only at the moment of its formation, and afterwards becomes covered with a greenish white; but it is very probable that it acquires this shade merely from the air contained in the vessel, and in the liquids made use of. There exists likewise another cause in the small portion of red oxide, from which the alkaline and even the calcareous prussiates derive their yellow colour. This oxide becomes converted into blue prussiate, and mixes with the white prussiate at the moment wherein the prussic acid, being disengaged from the alkalis, is at liberty to unite with the different oxides it meets with. These oxides are, as we have seen, a great quantity of the black oxide mixed with the red oxide of the alkaline prussiate. It must therefore necessarily follow, that a small portion of the blue prussiate will be mixed with a great quantity of the white prussiate; whence this last acquires its greenish tinge.

From the whole of these facts, it is evident that the oxide, which constitutes the base of the white prussiate, possesses the same degree of oxidation as when it formed the basis of the green sulphate. This oxide in effect passes from the sulphate to the white prussiate, without the interference of any cause which can either increase or diminish the proportion of oxygene. Hence it follows, that since the alkalis separate the oxide of the green sulphate of a grass-green colour, they ought also to separate it from the white prussiate under the same tinge.

This in fact happens at the instant when the caustic alkali, or ammoniac, is poured on the white prussiate. But in order to judge better of the shades, it is advisable to use these solutions diluted with water. The green oxide is not completely deprived of prussic acid till after the repeated application of alkalis. This may be shewn by taking up the green oxide, by an acid which does not dissolve the white undecomposed prussiate.

The Blue Prussiate.

AFTER what has been said, it is easy to foretel the habitudes of the nitrate, and all the solutions of which the oxide is at its maximum of oxidation, when alkaline prussiates are applied. No interval is perceived between the precipitation and the most lively blue. The colour

colour is perfect at the instant of precipitation, and exposure to the atmosphere adds nothing to its intensity.

Prussian blue is, in a word, that prussiate, whose basis contains $\frac{4}{5}$ of oxygen. It is the same with respect to the white prussiate as the red sulphate is to the green. These two prussiates do not differ with regard to their acid. Their distinctive characters arise from the different oxidation of their bases.

The blue prussiate is not altered by acids. The oxygenated muriatic acid changes it, by rendering it green, at the same time that itself undergoes alteration, as Berthollet has discovered. But the action of this destructive acid is exercised on the prussic acid, and not on the oxide, which can receive no greater dose of oxygen than it has already acquired from the nitric acid, the air, &c.

The acids which are used to brighten such Prussian blues as are imperfect, are of no other use than to dissolve the great quantity of carbonate of iron, precipitated by the pot-ash which is not saturated with prussic acid, and super-abounds in ill-prepared lixiviums. If white prussiate were to exist in such precipitates, it would be in no respect changed by the acid, and would acquire a blue colour only by absorbing from the atmospheric air the quantity of oxygen necessary for that purpose.

In order to shew that oxygen is the principle which in the blue prussiate of iron affords the distinctive colour, nothing more is necessary than to remark the colour of the oxide when precipitated by alkalis. In the green sulphate the precipitate was black; after the precipitation from Prussian blue it is red. No other principle but oxygen could have occasioned this difference in these oxides.

Mr. Proust having spoken indifferently of the yellow and the red oxide of iron, as being completely saturated with oxygen, observes, that he uses these expressions without distinction, because a number of facts have convinced him that there is no difference between them. Every red oxide, says he, when dissolved in any acid whatever, is precipitated of a yellow colour by alkalis, whether pure or saturated with carbonic acid. This last acid occasions no difference in the precipitate, because it has no tendency to unite with iron at that degree of oxidation. The red oxides when dried are brown, obscure and often black, according to the degree of dryness or density they may have acquired. But if they be pounded in a mortar, the characteristic colour soon appears. It is affirmed, that these oxides have the power of decomposing ammoniac; but our author kept them for several years in ammoniac, without observing that they were in any respect changed. He had no better success with the oxide of manganese at the ordinary temperature of the air.

The solution of sulphurated hydrogen gas, when kept in a vessel with the blue prussiate, is decomposed. It seizes from the oxide that portion of oxygen which constitutes the difference between the blue and the white prussiates; and the prussiate thus rendered white exhibits the same phenomena with alkalis, as if it had been obtained immediately from the green sulphate. The white prussiate kept under hepatic water undergoes no alteration; in which respect it resembles the green sulphate. Either of these readily yield to the hydrogen dissolved in this water all the oxygen they possess, exceeding 27 per cent.

The same theory explains, why the red sulphate and the nitrate of iron decompose sulphurated hydrogen. The oxide of iron burns the hydrogen, the sulphur is deposited, and the supernatant liquor, instead of affording a red precipitate by alkalis, exhibits a green.

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This observation points out a method of restoring the copperas of the shops to the state of green sulphate. When brown depositions are formed, it contains copper.

The solution of sulphurated hydrogen is not the only practicable means to reduce the blue prussiate to the state of white prussiate. The requisite abstraction of oxygen is made by keeping Prussian blue with water and plates of iron or tin in a well closed vessel. By this treatment it acquires all the properties of the white prussiate.

The distribution of oxygen between a metal and its oxide is not an uncommon fact in chemistry. By keeping a red sulphate or muriate with iron, they are enabled to recover their first state. Mercury kept in a solution of corrosive sublimate becomes changed, as does likewise the metallic salt itself, into the mild muriate of calomel. Mercury undergoes the same change in a solution of the red muriate of iron, but is not altered in a solution of the green muriate of the same metal. In the red sulphate, the mercury is converted into that kind of sulphate which does not become yellow by the affusion of water, that is to say, in which the oxide is at its minimum. Many other facts of the same kind might be mentioned.

It has been asserted in this paper, that the oxygenated muriatic acid does not act upon the oxide of Prussian blue. The following affords a proof of the truth of this assertion: All the known red oxides, whether natural or artificial, colcothar, the iron ore of Elba*, undergo no alteration in this acid; but the native brown oxides, which for the most part are mixtures of the black and red oxides, are affected by such treatment.

It is found by means of the oxygenated muriatic acid, that the oxides of the nitrate, the acetate and the muriate of lead are not at their maximum of oxidation. All these salts, when kept beneath this acid, are decomposed. A brown or puce-coloured oxide is soon deposited, and even crystallized round the sides of the vessels: and the nitric acid no longer acts upon the new oxide. In process of time, however, that acid assumes a fine rose colour, bubbles of air rise from the mixture at the bottom, and at length the nitrate is produced, as soon as the oxide, continually solicited to union by the nitric acid, has parted with the dose of oxygen which opposed that union.

The muriatic acid does not dissolve super-oxygenated lead without producing at the same time abundance of the oxygenated acid; but in order to procure this oxide in greater quantity, it is only necessary to treat the minium of commerce with a weak nitric acid, which separates 13 or 14 per cent. of brown oxide, discovered, as is well known, by Scheele. The red lead of Siberia is nothing else, as Mr. Macquart has shewn, but a natural super-oxidation of this metal. It would be interesting to know, whether by carrying the calcination beyond the point which affords minium, it might not be possible to render the oxide brown. Such a process might perhaps supply the want of manganese, for preparing the oxygenated muriatic acid.

Mr. Proust defers to a future communication an account of the nature of lead, oxidized in a less degree than constitutes the basis of the nitrate of this metal.

From the foregoing facts Mr. Proust draws the following conclusions;

The oxide which alkalis separate from Prussian blue is red, though it originally was black in the green sulphate which afforded this blue.

* The ore of Elba often contains sulphate of iron. It may be extracted by treatment with nitric acid, and subsequent precipitation with ammoniac or caustic pot-ash.

The white prussiate is a salt which is affected by atmospheric air in no other manner than the green sulphates, muriates and carbonates, or, in a word, most of the saline combinations which contain iron oxidized to the minimum. There is no other difference with respect to their super-oxidation, but the greater or less time these metallic salts demand; for he observed that the muriates and phosphates of which the oxide is at the minimum are not perceptibly altered by exposure to the air.

The prussiate of iron is not the only combination of this metal which owes its blue colour to atmospheric oxygen. That which is called native Prussian blue is merely the phosphate of iron oxidized to a certain point. Mr. Proust intends to shew artificial phosphates which are grey, blue, and white, according to the degree of oxidation. We are at present well acquainted, adds the author, with the cause of those dull greenish tinges which frequently appear in the newly-made prussiates. It often happens that they are not brightened well by acids, and do not acquire their peculiar lively colour but by exposure to the air. This fact is well known to manufacturers, who accordingly take care not to waste their acid in attempting to render such blues perfect.

It has been observed, that the pure green sulphate does not afford a black with the gallic acid. This is very true of it at the first moment. But the contact of air soon colours the mixture at its surface. A few drops of the oxygenated muriatic acid immediately produce the same effects throughout the fluid. We see therefore that iron does not form ink with the gallic acid, but in proportion as it is oxidized. This black colour may likewise be destroyed by including the black mixture in a bottle with a certain quantity of hepatic water.

From these facts it also very evidently appears why it is the established practice to expose stuffs to the air after the black dye; and why ink newly made and pale becomes black very speedily after it is spread upon paper, &c. For in all these mixtures the sulphate of commerce is used, which contains little of the red sulphate, and much of the green. When the gallic acid is poured into solutions of the sulphate and muriate, containing the red calx of iron, ink is instantly produced. The basis of ink and of every black dye is therefore merely the gallate of iron, of which the iron is oxidized to the maximum. Lastly, we cannot but observe in all these facts that chemists have hitherto erred with respect to the property of the ordinary sulphate of iron to become black with the acid of galls, to afford a blue with alkaline prussiate, &c. These properties belong exclusively to the combinations of which the oxide contains $\frac{1}{10}$ of oxygenic, and not $\frac{7}{10}$ only. I shall conclude, says Mr. Proust, by deducing from these experiments the principle I have established at the commencement of this memoir; namely, that iron, like many other metals, is subjected to the law of nature, which presides at every true combination; that is to say, that it unites with two constant proportions of oxygen. In this respect it does not differ from tin, mercury, lead, and, in a word, almost every known combustible. He intends shortly to explain the nature of that kind of oxide which results from the union of oxygen with carbone, in a less proportion than is required to constitute the carbonic acid.

VI.

*An Account of some Experiments to determine the Force of Fired Gunpowder. By BENJAMIN Count of RUMFORD, F. R. S. M. R. I. A.**

AFTER observing that no human invention, except perhaps the art of printing, appears to have produced such important changes in civil society as the invention of gunpowder; that notwithstanding the extensive uses to which this wonderful agent is applied, it seems not hitherto to have been examined with the attention it merits, and that probably this want of investigation may have arisen from the danger attending the experiments; our author proceeds to relate his own experiments and observations.

Several eminent philosophers and mathematicians have, from time to time, directed their attention to this subject. The modern improvements in chemistry have greatly elucidated the cause and circumstances attending the explosion of gunpowder. But the great desideratum, namely, the real measure of the initial expansive force of this agent, has not yet been determined. Robins, from his experiments, concluded that the elastic force of the fluid generated in the combustion of gunpowder, is one thousand times greater than the mean pressure of the atmosphere; but the celebrated Daniel Bernouilli determines its force to be not less than ten thousand times the same mean pressure. Count Rumford, being struck with the great difference in these results, has occasionally, for many years, endeavoured to ascertain the truth by experiment. In a paper printed in the year 1781, in the Transactions, he gave an account of an experiment, No. 92, by which it appeared that, calculating even upon Mr. Robins's own principle, the force of gunpowder must be at least 1308 times greater than the mean pressure of the atmosphere; and from that, and many other experiments, he became convinced that it was necessary to abandon the methods of that philosopher, and make others with a very different apparatus.

His first attempts were to fire gunpowder in a confined space, thinking that when he had accomplished this he should find means of measuring its elastic force without difficulty. A very strong short gun-barrel was prepared, and attempts made to fire gunpowder in the same, by means of a small vent, provided in one instance with a valve, and in another experiment lined with gold to prevent corrosion. These attempts were unsuccessful; the force of the explosion proving sufficiently great to enlarge the hole, and so rapid as not to allow time for closure of the valve. It became necessary, therefore, to endeavour to fire the gunpowder by means of heat conveyed through the mass of the metal itself. This was successfully performed in a barrel of the best forged iron, 3,45 inches long, the diameter of its bore $\frac{7}{16}$ ths of an inch, and its ends closed up by two screws, each one inch in length, which were firmly and immovably fixed in their places by folder. The vacuity between them in the barrel was consequently 1,45 inch in length, and constituted the chamber of the piece, whose capacity was nearly $\frac{7}{8}$ ths of a cubic inch. The thickness of the metal was equal to the bore of the piece. An hole $0,37$ of an inch in diameter was bored through both sides of the barrel, through the centre of the chamber, and at right angles to its axis. Two tubes of iron, $0,37$ of an inch in diameter, the diameter of whose bore was $\frac{1}{16}$ th of an inch, were firmly fixed in these holes with folder, in such a manner, that, while their internal openings

* Abridged from the Philosophical Transactions for 1797, p. 222.

were exactly opposite to each other, and on opposite sides of the chamber, the axes of their bores were in the same right line. The shortest of these tubes, which projected $1\frac{3}{4}$ inch beyond the external surface of the barrel, was closed at its projecting end; or rather it was not bored quite through its whole length, $\frac{1}{8}$ th of an inch of solid metal being left at its end, which was rounded off in the form of a blunt point. The longer tube, which projected 2.7 inches beyond the surface of the barrel on the other side, and which served for introducing the powder into the chamber, was open; but it could occasionally be closed by a strong screw furnished with a collar of oiled leather which was provided for that purpose. The method of making use of this instrument was as follows: The barrel being laid down, or held in a horizontal position with the long tube upwards, the charge, which was of the very best fine-grained glazed powder, was poured through this tube into the chamber. In doing this, care must be taken that the cavity of the short tube be completely filled with powder; and this can best be done by pouring in only a small quantity of powder at first, and then, by striking the barrel with a hammer, cause the powder to descend into the short tube. When, by introducing a priming-wire through the long tube, it is found that the short tube is full, it ought to be gently pressed together, or rammed down by means of the priming-wire, in order to prevent its falling back into the chamber, upon moving the barrel out of the horizontal position. The short tube being properly filled, the rest of the charge may be introduced into the chamber, and the end of the long tube closed up by its screw.

More effectually to prevent the elastic fluid generated in the combustion of the charge from finding a passage to escape by this opening after the charge was introduced into the chamber, the cavity of the long tube was filled up with cold tallow, and the screw that closed up its end (which was $\frac{1}{2}$ inch long, and a little more than $\frac{1}{8}$ th of an inch in diameter) was pressed down against its leather collar with the utmost force. The manner of setting fire to the charge was as follows: A block of wrought iron, about $1\frac{1}{2}$ inch square, with a hole in it capable of receiving nearly the whole of that part of the short tube which projects beyond the barrel, being heated red hot, the end of the short tube was introduced into this hole, where it was suffered to remain till the heat, having penetrated the tube, set fire to the powder it contained, and the inflammation was from thence communicated to the powder in the chamber.

The result of this experiment fully answered the expectation of the author. The generated elastic fluid was so completely confined that no part could make its escape. The report of the explosion was so very feeble as scarcely to be heard. It certainly could not have been heard at the distance of 20 paces. It resembled the noise occasioned by breaking a very small glass tube. The quantity of powder made use of in this experiment was not more than $1\frac{1}{8}$ th of what the chamber was capable of containing.

The next attempt was to measure the force of the elastic vapour thus confined. A hole was bored in the axis of one of the screws, or breech pins, which closed up the end of the barrel just described, and a piston of hardened steel was fitted into this hole, which was $\frac{1}{8}$ th of an inch in diameter. The end of the piston, which projected beyond the end of the barrel, was then caused to act upon a heavy weight, suspended as a pendulum to a long iron rod. It was hoped that a deduction of the pressure of the elastic vapour might be made from the length of the arc described by this pendulum; but the experiment was not found to answer, though various alterations and improvements were made in the apparatus before
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the method was abandoned. It was found almost impossible to prevent the escape of the elastic fluid by the sides of the piston; and the results of apparently similar experiments were exceedingly different, and so uncertain, that the Count was often at a loss to conjecture the reasons of these extraordinary variations.

In order to elucidate these in a discursive way, the Count proceeds to remark, that Mr. Robins's two assumed positions, namely, that the operation of gun-powder is performed by the rarefaction of a permanently elastic fluid, and that the whole of any charge is set on fire before the ball is sensibly moved from its place, are not to be admitted. On the contrary, from his own experiments, the Count is disposed to refer the prodigious expansion of fired gun-powder to the action of water in the state of steam at a very elevated temperature; and he shews by various observations, several of which must be familiar to every one who is habituated to use this agent, that the accension of gun-powder is so far from being performed in an extremely minute portion of time, that it is in every instance gradual and progressive. But having found it impossible to measure the elastic force of fired gun-powder with any degree of precision by the methods already mentioned, he totally changed his plan of operations, and, instead of endeavouring to determine its force by causing the generated elastic fluid to act upon a moveable body through a determinate space, he contrived an apparatus in which this fluid was made to act by a determinate surface against a weight, which being increased at pleasure should at last be such as would be just able to confine it, and in that case would just counterbalance, and consequently measure its elastic force.

A solid block of very hard stone, four feet four inches square, was placed upon a bed of solid masonry, which descended six feet below the surface of the earth. Upon this block of stone, which served as a base to the whole machinery, was placed the barrel in which the explosions were made. It was made of hammered iron 2,78 inches long, and 2,82 inches in diameter at its lower extremity, which was flat in order to rest upon its supporter. Its bore was one quarter of an inch in diameter, 2,13 inches long, measured from the upper surface of the barrel, and it ended in a very narrow opening below, not more than 0,07 of an inch in diameter, and 1,715 inch long, which formed the vent or passage by which the fire is communicated to the charge. This passage, however, was not open below, but terminated in a projection from the centre of the bottom of the barrel about 0,45 of an inch in diameter, and 1,3 inch long, which formed the vent tube closed below. When this barrel is placed upon its stand, which is of gun metal, the vent tube passes, through a hole, into a cavity in the stand which has a lateral opening. Into this lateral opening a ball of red hot iron is introduced, having a proper cavity within for the reception of the vent tube, which it speedily ignites, and sets fire to the powder through the solid substance of the tube itself. The opening of the bore of the barrel, which is placed in the vertical position when in use, is closed by a solid hemisphere of hardened steel, whose diameter was 1,16 inch, its plain side being downwards. It is confined laterally by three upright cylindrical pins, which allow it to rise only in the vertical direction. Upon this hemisphere is placed the weight made use of for confining the elastic fluid generated from the powder in its combustion. This weight, which in some of the experiments was a heavy twenty-four pounder placed vertically upon its cascabel, being fixed to certain timbers, was capable of sliding up and down

in a vertical frame, and could be raised and lowered in the intervals between the experiments by a strong lever.

The end of the barrel was covered with gold, in order to prevent as much as possible its being corroded by the elastic vapour, which, when the weight is not heavy enough to confine it, escapes between the end of the barrel and the flat surface of the hemisphere; but even this precaution was not found to be sufficient to defend the apparatus from injury. The sharp edge of the barrel, at the mouth of the bore, was worn away almost immediately; and the flat surface of the hemisphere, notwithstanding it was of hardened steel, and very highly polished, was sensibly corroded. The corrosion of the mouth of the bore, by which the dimensions of the surface, upon which the generated elastic fluid acted, were rendered very uncertain, would alone have been sufficient to have rendered all the Count's attempts to determine the force of fired gun-powder abortive; had he not found means to remedy the evil. The method he pursued for this purpose was as follows: Having provided some pieces of very good, compact sole-leather, he caused them to be beaten upon an anvil with a heavy hammer, to render them still more compact; and then, by means of a machine made for that purpose, cylindrical stoppers of the same diameter precisely as the bore of the barrel, and 0.13 of an inch in length, (that is to say, the thickness of the leather) were formed of it; and one of these stoppers, which had previously been greased with tallow, being put into the mouth of the piece after the powder had been introduced, and being forced into the bore till its upper end coincided with the end of the barrel, upon the explosion taking place, this stopper (being pressed on the one side by the generated elastic fluid, and on the other by the hemisphere loaded with the whole weight employed to confine the powder) so completely closed the bore, that when the force of the powder was not sufficient to raise the weight to such a height that the stopper was actually blown out of the piece, not a particle of the elastic fluid could make its escape. And in those cases in which the weight was actually raised, and the generated elastic fluid made its escape, as it did not corrode the barrel in any other part but just at the very extremity of the bore; the experiment by which the weight was ascertained, which was just able to counter-balance the pressure of the generated elastic fluid, was in no wise vitiated either by the increased diameter of the bore at its extremity, or by any corrosion of the hemisphere itself; for as long as the bore retained its form and its dimensions in that part to which the efforts of the elastic fluid were confined, that is, in that part of the bore immediately in contact with the lower part of the stopper, the experiment could not be affected by any imperfection of the bore either above or below.

The powder made use of in these experiments was of the best quality, being that kind called *poudre de chasse* by the French, and very fine grained, and it was all taken from the same parcel. Care was taken to dry it very thoroughly, and the air of the room in which it was weighed out for use was very dry. The weights employed for weighing the powder were German apothecary grains, 104.8 of which make 100 grains troy. The weights employed to confine the elastic vapour generated in the combustion of the powder, are reduced from Bavarian pounds, in which they were originally expressed, to pounds avoirdupois. The measures of length were all taken in English feet and inches. The experiments were all made in the open air, in the court-yard of the arsenal at Munich, and they were all made in

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fair weather, and between the hours of nine and twelve in the forenoon, and two and five in the afternoon; but the barrel was always charged, and the extremity of the bore closed by its leather stopper in the room where the powder was weighed. In placing the barrel upon the block of stone, great care was taken to put it exactly under the centre of gravity of the weight employed to confine the generated elastic vapour. Upon applying the red-hot ball to the vent tube, and fixing it in its place by its lever, which supported it, the explosion very soon followed.

When the force of the generated elastic vapour was sufficient to raise the weight, the explosion was attended by a very sharp and surprisingly loud report; but when the weight was not raised, as also when it was only a little moved, but not sufficiently to permit the leather stopper to be driven quite out of the bore, and the elastic fluid to make its escape, the report was scarcely audible at the distance of a few paces, and did not at all resemble the report which commonly attends the explosion of gunpowder. It was more like the noise which attends the breaking of a small glass tube, than any thing else to which it could be compared. In many of the experiments in which the elastic vapour was confined, this feeble report attending the explosion of the powder was immediately followed by another noise totally different from it, which appeared to be occasioned by the falling back of the weight upon the end of the barrel after it had been a little raised, but not sufficiently to permit the leather stopper to be driven quite out of the bore. In some of these experiments a very small part only of the generated elastic fluid made its escape; in these cases the report was of a peculiar kind; and though perfectly audible at some considerable distance, yet not at all resembling the report of a musket. It was rather a very strong sudden hissing, than a clear, distinct, and sharp report.

Though it could be determined with the utmost certainty by the report of the explosion, whether any part of the generated elastic fluid had made its escape; yet for still greater precaution, a light collar of very clean cotton wool was placed round the edge of the steel hemisphere, where it rested upon the end of the barrel, which could not fail to indicate, by the black colour it acquired, the escape of the elastic fluid, whenever it was strong enough to raise the weight by which it was confined, sufficiently to force its way out of the barrel.

Though the end of the barrel at the mouth of the bore was covered with a circular plate of gold, in order the better to defend the mouth of the bore against the effects of the corrosive vapour; yet, this plate being damaged in the course of the experiments (a piece of it being blown away), the remainder of it was removed, and it was never after thought necessary to replace it by another. When this plate of gold was taken away, the length of the barrel was of course diminished as much as the thickness of this plate amounted to, which was about $\frac{1}{80}$ th part of an inch; but in order that even this small diminution of the length of the barrel might have no effect on the results of the experiments, its bore was deepened $\frac{1}{80}$ th of an inch when this plate was removed, so that the capacity of the bore remained the same as before.

After making use of a great variety of expedients, the best and most convenient method of closing the end of the bore, and defending the flat surface of the steel hemisphere from the corroding vapours, was found to be this: First, to cover the end of the bore with a circular plate of thin oiled leather; then to lay upon this a very thin circular plate of
hammered:

hammered brass, and upon this brass plate the flat surface of the hemisphere. When the elastic fluid made its escape, a part of the leather was constantly found to have been torn away, but never in more places than one; that is to say, always on one side only.

What was very remarkable, in all those experiments in which the generated elastic vapour was completely confined, was the small degree of expansive force which this vapour appeared to possess after it had been suffered to remain a few minutes, or even only a few seconds, confined in the barrel; for upon raising the weight by means of its lever, and suffering this vapour to escape, instead of escaping with a loud report, it rushed out with a hissing noise, hardly so loud or so sharp as the report of a common air-gun; and its effects against the leathern stopper, by which it assisted in raising the weight, were so very feeble as not to be sensible. Upon examining the barrel, however, this diminution of the force of the generated elastic fluid was easily explained; for what the Count thinks was undoubtedly in the moment of the explosion in the form of an elastic fluid, was now found transformed into a solid body as hard as a stone. It may easily be imagined how much this unexpected appearance excited his curiosity; but, intent on the prosecution of the main design of these experiments, the ascertaining the force of fired gun-powder, he was determined not to permit himself to be enticed away from it by any extraordinary or unexpected appearances or accidental discoveries, however alluring they might be; and faithful to this resolution, he postponed the examination of this curious phenomenon to a future period, and since that time he has not found leisure to engage in it. He thinks it right, however, to mention such cursory observations as he was able in the midst of his other pursuits to make upon this subject.

This matter was very hard, and so firmly attached to the inside of the barrel, and particularly to the inside of the upper part of the vent tube, that it was always necessary, in order to remove it, to make use of a drill, and frequently to apply a considerable degree of force. It was of a black colour, or rather of a dirty grey, which changed to black upon being exposed to the air, had a pungent acrid alkaline taste, and smelt like liver of sulphur. It attracted moisture from the air with great avidity. Being moistened with water, and spirit of nitre being poured upon it, a strong effervescence ensued, attended with a very offensive and penetrating smell. Nearly the whole quantity of matter of which the powder was composed seemed to have been transformed into this substance*; for the quantity of elastic fluid which escaped upon removing the weight was very inconsiderable. But this substance was no longer gun-powder; it was not even inflammable.

What change had it undergone? demands our author,—What could it have lost? It is very certain the barrel was considerably heated in these experiments. Was this occasioned by the caloric, disengaged from the powder in its combustion, making its escape through the iron? And is this a proof of the existence of caloric considered as a fluid *sui generis*; and that it actually enters into the composition of inflammable bodies, or of pure air, and is necessary to their combustion? He dares not take upon him to decide upon such important questions. He once thought that the heat acquired by a piece of ordnance in being fired arose from the vibration or friction of its parts, occasioned by the violent blow it received in

* It is much to be regretted that no experiment was made of the weight of this substance afforded by a given weight of gunpowder. N.

the explosion of the powder, but acknowledges fairly that it does not seem to be possible to account in a satisfactory manner for the very considerable degree of heat which the barrel acquired in these experiments, merely on that supposition.

That this hard substance found in the barrel, after an experiment in which the generated elastic vapour had been completely confined, was actually in a fluid or elastic state in the moment of the explosion, is evident, as he thinks, from hence: that in all those cases in which the weight was raised, and the stopper blown out of the bore, nothing was found remaining in the barrel. It was very remarkable, that this hard substance was not found distributed about in all parts of the barrel indifferently, but there was always found to be more of it near the middle of the length of the bore than at either of its extremities, and the upper part of the vent tube in particular was always found quite filled with it. It should seem therefore, says he, that it attached itself to those parts of the barrel which were soonest cooled; and hence the reason, most probably, why none of it was ever found in the lower part of the vent-tube, where it was kept hot by the red-hot ball by which the powder was set on fire.

He found by a particular experiment, that the gunpowder made use of, when it was well shaken together, occupied rather less space in any given measure than the same weight of water; consequently when gunpowder is fired in a confined space which it fills, the density of the generated elastic fluid must be at least equal to the density of water. The real specific gravity of the solid grains of gunpowder, determined by weighing them in air and water, is to the specific gravity of water as 1,868 to 1000. But if a measure whose capacity is one cubic foot hold 1000 ounces of water, the same measure will hold just 1077 ounces of fine-grained gunpowder, such as was used in these experiments, that is to say, when it is well shaken together. When it was moderately shaken, together its weight was exactly equal to that of an equal volume, or rather measure, of water. But it is evident that the weight of any given measure of gunpowder must depend much upon the forms and sizes of its grains. He adds one observation more relative to the particular appearances which attended the experiments, in which the elastic vapour, generated in the combustion of gunpowder, was confined, and that is with regard to a curious effect produced upon the inferior flat surface of the leathern stopper where it was in contact with the generated elastic vapour. Upon removing the stopper, its lower flat surface appeared entirely covered with an extremely white powder, resembling very light white ashes, but which almost instantaneously changed to the most perfect black colour upon being exposed to the air.

The sudden change of colour in this substance, upon its being exposed to the air, led him to suspect that the solid matter found in the barrel was not originally black, but that it became black merely in consequence of its being exposed to the air. The dirty grey colour it appeared to have, immediately on being drilled out of the cavity of the bore where it had fixed itself, seems to confirm this suspicion. An experiment made with a very strong glass barrel would not only decide this question, but would most probably render the experiment peculiarly beautiful and interesting on other accounts.

The Count thinks a barrel of glass might be made strong enough for the experiment, if it could withstand the sudden heat, and on the whole seems disposed to think the trial worth making.

All the parts of the operation being ready, it was in the autumn of the year 1792 that the first experiment was made.

The barrel being charged with 10 grains of powder, its contents when quite full amounting to about 28 grains, and the end of the barrel being covered by a circular piece of oiled leather, and the flat side of the hemisphere being laid down upon this leather, and a heavy cannon, a twenty-four pounder, weighing 8081 pounds avoirdupois, being placed upon its cascabel in a vertical position upon this hemisphere, in order to confine by its weight the generated elastic fluid, the heated iron ball was applied to the end of the vent-tube; and after waiting but a very few moments in anxious expectation of the event, Count Rumford had the satisfaction of observing that the experiment had succeeded. The report of the explosion was extremely feeble, and so little resembling the usual report of the explosion of gunpowder, that the by-standers could not be persuaded that it was any thing more than a cracking of the barrel, occasioned merely by its being heated by the red-hot ball: yet, as the Count had been taught by the result of former experiments not to expect any other report, and as he found, by putting his hand upon the barrel, that it began to be sensibly warm, he was soon convinced that the powder must have taken fire; and after waiting four or five minutes, upon causing the weight which rested upon the hemisphere to be raised, the confined elastic vapour rushed out of the barrel. Upon removing the barrel and examining it, its bore was found to be choked up by the solid substance already described, and from which it was with some difficulty that it was freed, and rendered fit for another experiment. The extreme feebleness of the report of the explosion, and the small degree of force with which the generated elastic fluid rushed out of the barrel upon removing the weight which had confined it, had inspired the assistants with no very favourable idea of the importance of these experiments. It was seen indeed from the beginning by their looks, that they thought the precautions to confine so inconsiderable a quantity of gunpowder as the barrel could contain, perfectly ridiculous; but the result of the following experiment taught them more respect for an agent, of whose real force they had conceived so very inadequate an idea.

In this second experiment, instead of 10 grains of powder, the former charge, the barrel was now quite filled with powder, and the steel hemisphere, with its oiled leather under it, was pressed down upon the end of the barrel by the same weight as was employed for that purpose in the first experiment, namely, a cannon weighing 8081 pounds. The barrel (which, though similar to it in all respects, was not the same that has already been described) was made of the best hammered iron, and was of uncommon strength. Its length was $2\frac{3}{4}$ inches; and though its diameter was also $2\frac{3}{4}$ inches, the diameter of its bore was no more than $\frac{1}{4}$ of an inch, or less than the diameter of a common goose quill. The length of its bore was 2.15 inches. Its diameter being $2\frac{3}{4}$ inches, and the diameter of its bore only $\frac{1}{4}$ of an inch, the thickness of the metal was $1\frac{1}{2}$ inch; or it was 5 times as thick as the diameter of its bore. The charge of powder was extremely small, amounting to but little more than $\frac{1}{10}$ th of a cubic inch; not so much as would be required to load a small pocket pistol, and not one-tenth part of the quantity frequently made use of for the charge of a common musket. This inconsiderable quantity of gunpowder, when it was set on fire by the application of the red-hot ball to the vent-tube, exploded with such inconceivable force as to burst the barrel asunder in which it was confined, notwithstanding its enormous strength, and with such a loud report as to alarm the whole neighbourhood. The spectators turned pale with affright and astonishment, and it was some time before they could recover themselves. The barrel was not

not only completely burst asunder, but the two halves of it were thrown upon the ground in different directions; one of them fell close by the Count's feet, as he was standing near the machinery to observe more accurately the result of the experiment.

From some former experiments with small iron wire, Count Rumford deduces, that a cylinder of that metal, whose transverse section is one inch, would be able to sustain 63466 pounds without being broken. But for greater accuracy, he caused several small pieces to be cut out of the solid half of the barrel which was broken. From four experiments with pieces whose diameters were respectively in thousandth parts of an inch, fifty, sixty, sixty-six and seventy-six parts, which were broken by a direct pull, it was found that the medium weight required to break one inch of this iron was in pounds avoirdupois 63173. The variations in the result were such as to render the second figure (expressing thousands) uncertain. Our author computes the resistance of the barrel from the area of the surface of fracture, namely, $6\frac{1}{2}$ inches, by using this as a simple multiplier, which gives 410624 $\frac{1}{2}$ pounds. And this force being considered as applied to a surface of half an inch, which is the area of a longitudinal section of the bore of the barrel, and reduced into atmospheres, by allowing 15 pounds avoirdupois for the medium pressure of the air upon a square inch, gives 54750 atmospheres for the measure of the force exerted for overcoming such a resistance. This force, enormous as it may appear, is supposed by the Count to be short of the real initial force of the elastic fluid generated in the combustion of gunpowder, because he thinks it probable that the barrel was in fact burst before the generated elastic fluid had exerted all its force. On this head of probabilities I would venture to make a remark, that the iron in the barrel may perhaps be considered as not exactly in the situation of the pieces which our author broke in his engine by a direct pull. For in these last it may reasonably be supposed, that the whole pressure of cohesion was equally acted on till the moment of fracture; whereas in the barrel the fracture may, from the spring of the metal and other circumstances, be supposed to have proceeded from the inner to the outer surface by a progression or tearing asunder, which, however swift, must have rendered the division more easy. But at all events, whatever may be the force of this remark, it can in no respect invalidate such conclusions as were drawn from the actual lifting of great weights by raising the hemispherical cover of the barrel.

A set of experiments were instituted with an apparatus of the kind here described, for the purpose first of determining the expansive force of the elastic vapour generated in the combustion of gunpowder in its various states of condensation, and the ratio of its elasticity to its density; and secondly, of measuring by one decisive experiment the utmost force of this fluid in its most dense state; that is to say, when the powder completely fills the space in which it is fired, and in which the generated fluid is confined. A numerous series of experiments are tabulated in the Count's Memoir.

The dimensions of the barrel made use of in those experiments were as follow:

Diameter of the bore at its muzzle = 0.25 of an inch.

Joint capacities of the bore and of its vent-tube exclusive of the space occupied by the leathern stopper = 0.08974 of a cubic inch.

Quantity of powder contained by the barrel and its vent-tube when both were quite full, exclusive of the space occupied by the leathern stopper, 25,641 German apothecaries grains, = 24 $\frac{1}{2}$ grains troy.

The Table, in the original, contains a great number of experiments, in which the weight or piece of ordnance was either not raised, or was thrown up with a loud report. Those experiments, in which the weight just moved without a report, are obviously such as indicate the elastic force to have been equal to the pressure. I have accordingly selected these only in the following table as suitable to form the basis of computations. The day, hour, and minute when each experiment was made, are likewise to be found in the original, though it did not seem necessary to insert them in this abridgment. The expression of the weight on atmospheres is grounded on the assumption, that the mean pressure of the atmosphere is equal to fifteen pounds avoirdupois upon a square inch.

TABLE I.—Experiments on the Force of Fired Gunpowder.

State of the Atmosphere.		The Charge of Powder.		Weight employed to confine the elastic fluid.		State of the Atmosphere.		The Charge of Powder.		Weight employed to confine the elastic fluid.	
Thermometer. F.	Barometer.	Grain.	Parts.	In lbs. avoirdupois.	In atmospheres.	Thermometer. F.	Barometer.	Grain.	Parts.	In lbs. avoirdupois.	In atmospheres.
Deg.	Eng. Inch.			lbs.		Deg.	Eng. Inch.			lbs.	
37	28,56	6	234	504,8	685,6	32	28,2	10	390	1387,5	1884,3
57	28,37	1	39	57,4	77,86	32	28,2	11	429	1634	2219
34	28,1	2	78	134,2	182,3	36	28,34	12	460	1895,1	2573,7
48	28,31	3	117	212,24	288,2	42	28,3	13	507	2422	3288,3
50	28,36	4	156	281,57	382,4	43	28,31	14	546	2951	4008
48	28,35	5	195	413,27	561,2	43	28,31	15	585	3477	4722,5
59	28,34	7	273	597,66	811,7	70	28,2	16	624	5220	7090
50	28,32	8	312	857,64	1164,8	68	28,3	18	702	8081	10977
50	28,32	9	351	1142,3	1551,3						

In the last experiment, with near eleven thousand atmospheres, the weight was raised with a very sharp report, louder than that of a well-loaded musket; and in the experiment immediately following with the same charge, but with the addition of six hundred and nineteen pounds to the weight, the vent-tube of the barrel burst. This experiment was the 85th of the Table, probably the 85th time of heating the tube.

(To be concluded in the next Number.)

VII.

Observations and Experiments on Steel, resembling that of Damascus; with an easy Test for determining the uniform Quality of Steel before it is employed in Works of Delicacy or Expence.

IN the infancy of society the hardest bodies, such as stones, and certain kinds of wood, were selected and used for cutting instruments, and still are applied to that purpose in several parts of the world. These materials were succeeded by copper, hardened by a mixture of tin, of which numerous weapons yet remain in the cabinets of the curious. And lastly, steel,

steel, whether obtained directly from the ore, or by cementation of malleable iron, has deservedly taken place of every other article, on account of the united qualities of tenacity and hardness *. When the sword was the chief weapon of war, it must have been an object of great interest and demand to give to its blade a durable keen edge, and a degree of firmness or strength, which, without rendering it unwieldy, should ensure the warrior against exposure to the fatal accident of its breaking in the act of combat. The fabres of Damascus have been famous for ages, and still bear a great price in the East; but we have no decided account of the manner in which this steel is manufactured or made up. Some years ago I was favoured with the possession of a true blade of this kind for a few days, which, if my recollection be accurate, had cost the possessor twelve guineas at Constantinople. I know the sum was not less than this. As I was not permitted to make any experiments upon it, I could only ground my process upon reasoning from its external appearance and obvious qualities.

It had a dull grey or bluish appearance, was scarcely harder than common steel from the forge, was not easily bended, and when bended had no spring to recover its figure. Its back was smooth, as were also two narrow sloped surfaces which formed its edge under an angle of about 40 degrees; but its flat sides were every where covered with minute waving lines in masses in all directions, not crossing each other, and, for the most part, running in the direction of its length. The lines were in general as fine as harpsichord wire, not extremely well defined nor continued; and their distinction from each other was effected by no perceptible indentation of the surface, but rather by the succession of parts differing in the degree of polish or brightness. No one, upon inspection of this surface, would for a moment have imagined or allowed that it could have been done by engraving or etching, as the false blades are damasked. I was informed that if any part of this blade were made smooth by grinding or whetting, the wavy appearance, called the water, could be again produced by means of lemon juice; and that its excellencies were, that it could be depended upon not to break, and that it would cut deeper into a soft substance, such as a pack of wool, or into flesh, than any other kind of blade.

From these circumstances, as well as from the price, I was induced to think that the blade was composed of steel and iron, and that the process of forging was such as greatly to enhance the cost, by the labour and management it might require. For if we suppose the pieces to be united together at the welding heat, and then forged or drawn out, it is certain that no small degree of skill and care would be required to render all the parts found, and at the same time preserve the steel and iron in possession of their characteristic properties. Too great a heat would probably render the whole mass more uniform than is consistent with the subsequent production of the water or wavy appearance. In my attempt to imitate this steel, I endeavoured to substitute a mechanical contrivance in the place of this supposed careful forging.

I caused a cylindrical hole of about one inch in diameter to be bored through a piece of cast iron, the lower part of which could be so placed upon an anvil as to close one end of the hole. A forged iron plug was made nearly to fit the cylindrical hole, but considerably longer. Equal weights of German steel and Swedish iron, both in filings, were then well

* Philosophical Journal, I. 381.

mixed with oil, and wrapped in a paper, which had before been rolled upon the plug, and consequently fitted the cylinder. The ends of the paper were neatly folded; and the whole mafs being then put into the cast-iron cylinder placed upon the anvil, a few blows were given by driving the plug into the hole with a heavy hammer. By this means the mafs of filings, when thrust out of the cylinder, was compact and manageable. It was then placed in a charcoal fire, and urged to a welding heat by the double bellows. Thence it was taken with the tongs; again hastily put into the cylinder, and hammered by means of the plug and the heavy hammer. When it was taken out, the whole was found to be consolidated; but upon forging it into a plate, a considerable portion flew off in a crumbly form. The plate, however, was filed up, smoothed, and examined.

Its colour presented nothing remarkable. When weak nitrous acid was poured upon it, it became mottled in consequence of the numerous small black spots which appeared upon the particles of steel, while those of iron remained clean. On the nitrous acid being washed off, the surface appeared wavy like the Damascus steel, but scarcely at all fibrous; doubtless because the solid had not been drawn out by forging. An attempt was made to harden it by ignition and cooling in water; but it still remained soft enough to be cut with the graving tool, the point of which did not indicate any difference in that respect between the parts of iron and of steel, though it is very probable such a difference did really exist.

I infer, therefore, that the Damascus steel is in fact a mechanical mixture of steel and iron; that it is incapable of any considerable degree of hardness, and consequently is in no danger of breaking from its brittleness; that its tenacity is ensured not only from the admixture of iron, but likewise from the facility with which its soundness may be ascertained throughout, by the same process which exhibits the water or fibrous appearance: and, lastly, that the edge of a weapon formed of this material must be rough, on account of the different resistance which the two substances afford to the grindstone, in consequence of which it will operate as a saw, and more readily cut through yielding substances than such cutting tools as are formed of a more uniform substance.

This experimental enquiry directed my attention to a method of ascertaining the uniformity of texture in iron or steel, which perhaps may have been noticed by others, but is certainly unknown in most manufactories, though I have found it of great utility. If a weak acid, for example the nitrous, which I have usually taken in a very diluted state, be applied to the face of iron or steel previously cleaned with the file, or with emery paper, the parts which contain the greatest portion of carburet of iron (or plumbago) immediately shew themselves by their dark colour. It very frequently happens that articles of considerable value, intended to be fabricated in iron or steel, are not known to be defective until much expence has been laid out in manufacturing them. A piece of iron, which has a vein of steel running through it, as is too often the case, will require at least three times the labour and care to turn it in the lathe, which would have been demanded by a piece of greater uniformity. Steel which abounds with spots, or veins, or specks called pins, may be fashioned completely, and will not shew its defects, until the final operation when the attempt is made to polish it. Other articles, such as measuring screws, blades of sheers, fine circular cutters, &c. either bend in the hardening, from the difference of expansion, or resist the tool when wrought in the tempered state, or exhibit other incurable defects when they come to be tried; which the test, by nitrous acid would have indicated before

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any expence had been incurred. In these, and in numberless other instances, it would have been incomparably more advantageous to have rejected the material upon the first trial, rather than have proceeded to the very expensive process of manufacturing the article, and then finding it of no value. By this simple expedient I have seen bars of steel as full of veins and irregularities as wood, and have been enabled to select the best and most uniform pieces for works of the greatest delicacy; whereas, before I thought of this mode of trial, I have very often had the mortification to fail in the last stage of experimental processes, upon which much cost and labour had been bestowed.

VIII.

On the Irritability of the Pollen of Plants. With an Account of a Composition for closing Wide-mouthed Vessels.

To Mr. NICHOLSON, Editor of the Philosophical Journal.

SIR,

A FEW summers ago a friend of mine chanced to make a curious discovery relative to the irritability of the pollen or fecundating dust of plants; which, as I know not that it has yet met the eye of the public, you are at liberty, if thought sufficiently interesting, to communicate through the channel of your Journal.

Whilst making observations with one of Adams's compound microscopes on the figure of the particles of pollen collected from different plants, he applied a drop of water to a small group (which was placed on the glass plate of the stage), in order to increase the bulk; and, by that means, more accurately determine the figure of the particles. Happening, at the same time, to have a small phial of spirits of wine at hand, he next tried a drop of that liquid to fresh pollen, when he was agreeably surprised with seeing it produce a quick gyration, as well as a darting of the particles backward and forward in the drop: this motion continued for a few minutes, and then gradually subsided; the particles unravelling into a continuous filmy thread, and at other times appearing to burst and emit a multitude of particles * infinitely smaller, and which conglomerated together, leaving the capsular vessel empty, or only filled with the spirit.

The speedy evaporation of the spirit appearing at times to prevent the completion of the phenomenon, he afterwards tried, and with better success, a little common brandy. The motions, in this case, were continued longer, and the appearances were more complete. It will appear that these do not proceed from the mere evaporation of the liquid, because those particles which have been once saturated, though from some cause they may not have burst or unravelled, are incapable of excitation, at least in any similar degree, by the application of more spirit.

Different kinds of pollen were made use of for this experiment; but that of the "*Cactus flagelliformis*" was mostly employed, as affording the most striking appearance, on account of the magnitude of its particles.

* At least it presented this appearance at times to my eye; which, I doubt not, has been the occasion of its obtaining from some botanists the appellation of the spermatic aura of pollen.

I avoid entering into any theoretical observations on the aforesaid phenomenon, until more fully illustrated by experiment, and content myself with merely announcing it to the curious investigators in the paths of philosophical botany.

I take this opportunity of communicating another method, or at least another composition for cementing wide-mouthed vessels, in addition to those mentioned in your Journal for September. It is a mixture of spermaceti and caoutchouc: the former to be melted in a ladle, and the latter added in small bits, which will be gradually but effectually dissolved, and the compound forms a cement perfectly air-tight when poured on fluid and suffered to cool. It is also, I believe, very little, if at all, attacked by acids, and on that account might be of considerable use in philosophical laboratories. The quantity of caoutchouc may be varied according to the intended purpose, or any hardening substance, as mastic, added if required. If this latter communication should prove of any use to you personally, or to your chemical friends, it will amply gratify

Your obliged reader,

Dec. 1st, 1797.

AMICUS.

IX.

Experimental Researches to ascertain the Nature of the Process by which the Eye adapts itself to produce distinct Vision.

[Continued from page 313.]

IN the former part of this article, respecting the adjustment of the eye, a considerable number of original experiments and remarks were related from communications to the Royal Society. These have for their objects the determination, whether the crystalline humour by its supposed muscularity, the external muscles by their greater or less pressure upon the orbit, or the cornea by a variation in its curvature, be each the sole or chief agent in causing the pencils of light from visible objects to arrive in all cases at those foci which distinct vision requires. I shall now proceed to give the substance of the other Memoirs published by the Royal Society upon the same subject.

In the volume for 1795, page 263, I find a paper of Observations on the Eyes of Birds, by Mr. Pearce Smith. This gentleman relates, that, in the year 1792, he observed, while dissecting the eyes of birds, an irregular appearance of the sclerotica in that part which surrounds the cornea, and which in them is generally flat. On a more minute examination, it appeared to be scales lying over each other, and which appeared capable of motion on each other. On investigating this structure, the scales were found to be of bony hardness, at least much more so than any other part of the sclerotica. On the inside of the sclerotic coat there was no appearance of these scales. Tendinous fibres were detected spreading over the scales, terminating in the four recti muscles of the eye, so that upon the contraction of those muscles the scales would be moved.

By reflecting on the probable uses of this conformation, Mr. Smith deduced, that the internal capacity of the eye will be greatest when the recti muscles are not in action; and the several circles of scales being suffered to repose upon each other in succession, are upon

the whole, nearer the anterior extremity of the optic axis, than at any other time; that when by voluntary effort those muscles are made to act, the several circles of scales will be drawn back, and diminish the periphery of the eye at their respective positions in this last situation; and that consequently the internal capacity of the organ being rendered less, the most elastic part, namely, the cornea, will yield to the pressure of the included humour, and become more convex. This he concludes must render small objects, near the animal, very distinct.

On the relaxation of the muscles, the original flatness of the cornea will be restored by the elasticity (as he affirms) of the sclerotica; and he proceeds to make remarks on the advantage the animal system derives from elasticity being opposed to muscular force, giving nearly the same instances as were offered with regard to the elasticity of the cornea a few months before by Mr. Home *. He does not explain in direct terms the peculiar excellence this imbricated system of scales may be imagined to possess, but thinks it particularly necessary to birds, which, without it, would, as he says, be continually exposed to dash themselves against the trees of a thick forest. He conceives also, that the extraordinary alteration in the focus of the eye of an eagle in almost an instant of time when it darts from the upper air and seizes an object on the ground, and the pursuit and seizure of a gnat or small fly by a swallow, must require the aid of this apparatus, which in them is very distinct. But with regard to these particulars, or final purposes, somewhat more of investigation seems to be wanting. For it is certain, that no bird will dare to fly rapidly through a thick wood, but either pass leisurely from branch to branch, or soar above the tops of the trees when speed is the object of their aim. And however considerably the bony circles may be shewn to add to the range of adjustment for near objects, it does not appear that any greater suddenness of change, in the eagle and other birds of the same species, is wanted, than is within the power of animals not possessing that structure; because the adjustment from remote to near objects is effected in the human, and probably every other eye, in a time incomparably shorter than that of an eagle's descent upon his prey. Neither do swallows pursue gnats or flies, but scud through the air with at least twelve times the velocity of those small creatures, which they probably catch by opening their mouths without the least use of sight.

The mention of swallows, and the probability of winged animals striking themselves against trees in their flight, brings to my recollection some particulars which do not seem altogether foreign to the present subject. In a certain state of the atmosphere, it is usual for swallows to fly rapidly along near the face of a row of houses, at the distance of three or four inches from the wall. Now I take it for granted, that they cannot possibly distinguish the houses during their course, on account of the rapid angular motion; and I doubt very much whether they see objects before them with any precision—not to mention the probability that the eye itself would be immediately dried if the nictitating membrane were not almost constantly upon it. With these reflections, on an occasion of this kind, it did not seem surprising that many of these birds came so near my face, while observing them from a window, that they scarcely seemed to have noticed me. But the most remarkable circumstance was, that they did in fact turn aside so as to avoid striking me; though in some instances so near that I

* Philoſ. Trans. 1795, p. 22; or this Journal, p. 313.

both heard and felt the action of the air to which they gave motion. On more attentive consideration, however, it appeared to me, that this motion of the air is the very instrument which by its re-action may warn them of the presence and position of an obstacle the instant they approach it; which they may habitually and speedily avoid. For, as the intelligent blind are said to know, by the motion of air and its echo, the size of rooms, the width of streets, and other local attributes, which are commonly ascertained by the sense they are deprived of; and as boats move more sluggishly in shallow or contracted channels; and air itself cannot be forced with rapidity through long tubes;—in all which circumstances the re-action must be very considerable; so, on the other hand, it can scarcely be doubted, that the object of such re-actions, if capable of perception, would be sufficiently aware of their presence or absence. I suppose the re-action of the air dashed against my face by the swallow, and the noise I heard, were as perceptible and as impressive to the bird as to myself; and from this reasoning I am less disposed to wonder at the result of the cruel experiment performed in Italy a few years ago upon bats. As I have seen the notice only in a voluminous periodical publication to which I cannot now turn, I must simply relate, that bats have been thought to possess a sixth sense, because, when the eyes of one of those animals were dissected out, it flew round a room with the same speed and precision as before, avoiding the walls and obstacles as readily as if still possessed of sight. If my conjectures have any foundation, these creatures did never use the eye to warn them of impediments to their flight, but had constantly attended to the re-action of the fluid in which they moved; which would be very different in the vicinity of an obstacle compared with that in open space. From the re-action of a direct obstacle, they would turn to the side where the pressure was least, or the space most open; and if the obstacle were oblique, it may easily be imagined that the requisite deviation from their course would be equally obvious.

To return to Mr. Smith's Memoir. He was led from the examination of the eyes of birds to those of quadrupeds, and found by tearing and dissecting that the recti muscles terminate in the cornea; in which, and in his inferences, he entirely agrees with Mr. Home in the paper before-mentioned*.

The Croonian Lecture, read before the Royal Society in November 1795, was written by Everard Home, Esq. and contains a prosecution of the enquiry respecting vision, of which an abridgement has already been given.

The explanation of the adjustment of the eye by an assumed change in the radius of the cornea being different from the theories before formed on that subject, it was thought right to put it to the test of every experiment which might appear likely to refute or confirm the observations already made. The reader has seen that a perceptible variation in the figure of this part of the organ of sight, was ascertained by observing its outline by means of a microscope. Another method suggested itself; namely, that if the convexity of the cornea were increased to a certain degree, it could be measured by applying an achromatic microscope, with a divided eye-glass micrometer, to view the image in the virtual focus of its surface. The first step in this experimental process was to ascertain, by experiments upon convex mirrors, what difference of curvature could be decidedly observed by an apparatus of this kind. Two convex mirrors, one of $\frac{1}{10}$ ths of an inch focus, the other $\frac{1}{15}$ ths, had their

* Philof. Transf. or this Journal as last quoted.

flat surfaces made rough and blacked, to prevent an image being seen from both surfaces. One of these was stuck upon a piece of wood, directly opposite a window, at 12 feet distance from it. A board, four feet long, and six inches broad, was placed perpendicularly against the sash of the window, and its image reflected from the mirror upon the object glass of an achromatic microscope, with a divided eye-glass micrometer.

The two images were separated by means of the divided eye-glass till their surface of contact, which appears like a black line, was rendered as small as possible. When this effect was produced on the images from the mirror of $\frac{4}{5}$ ths of an inch focus, that mirror was removed and the other put in its place. The contact of the two images, which before appeared like a line, had now acquired considerable breadth, corresponding exactly to the difference between the convexities of the mirrors.

When the same experiment was repeated upon the eye, there was a perceptible, though extremely small, change in the micrometer at first when the eye was fresh. This was not however seen afterwards, and the eye very soon became so much fatigued, that it was necessary to desist. It was found that every time the eye adapted itself to different distances it became necessary to alter the distance of the object-glass of the microscope from the cornea.

This experiment was repeated on four different days with the same result; namely, a change in the micrometer at first, which in the subsequent trials could not be detected. Two suppositions offered themselves as likely to account for this effect, namely, a motion of the head forwards, or an action of the muscles of the head itself; but, as the author remarks, this effect ought in the first case to have been more frequent, the greater the fatigue of the eye; and the latter circumstance would have produced a contrary result. It appeared, therefore, that the effect on the micrometer did really arise from a change in the cornea, though too small to be detected with certainty in this way.

With two other mirrors, of which the focal distances were $\frac{4}{10}$ ths and $\frac{4.66}{100}$ ths of an inch, the difference between the size of the images was just visible in the micrometer; but it did not appear probable that the same difference would have been visible had the mirror not been perfectly at rest. From the unsteadiness of the eye, it might therefore be reasonably supposed that a change of this magnitude might take place in the cornea without being distinctly seen.

To give an idea of the short time that a part can remain nicely adjusted by muscular action, the author points out an experiment which any one may make upon himself. Let him take a glass spirit level, and rest one end of it on a table, supporting the other with his hand, and endeavour to keep the air bubble in the middle. If the hand is very steady, the bubble may be kept nearly in its place, but not exactly so; for it will undulate in correspondence with the action of the muscles, making up for want of steadiness, by short motions, in contrary directions.

From these experiments, the change in the curvature of the cornea could not be more than $\frac{1}{15}$ th part of an inch, as any greater quantity would probably have been distinctly seen in the micrometer. This, however, is still more than was ascertained by the former experiments, which made it to exceed $\frac{1}{200}$ th part of an inch.

This change in the cornea, at first view of the subject, appeared sufficient to account

for the adjustment of the eye, and may probably be sufficient when the lens is removed, but not when the eye is entire. The author therefore proceeded to examine what alteration the figure of the human eye might be susceptible of when air was thrown into its cavity, through the optic nerve, so as to distend its coats. From the experiments it was found that the axis of vision was lengthened a small degree in the eye of a boy six years old, forty-five minutes after death, while the transverse diameter and axis from the optic nerve were shortened. A similar though less effect was observed in the eye of a man 25 years old, one hour after death; but no alteration took place, by the like treatment, in the eye of a man 50 years old, 20 minutes after death. The object of these experiments does not clearly appear, as there is no suspicion of an action of this kind in the living subject; but they appear to shew that the cornea is the part most variable from elasticity; and when the pressure is made laterally, and from without, the elongation must be still greater, the action of the straight muscles being the most advantageous that could be imagined for this purpose.

This lateral pressure, Mr. H. observes, will not only elongate the eye, and increase the convexity of the cornea, but will produce an effect upon the crystalline lens and ciliary processes, pushing them forward in proportion as the cornea is stretched. For, as these processes form a complete septum between the vitreous and aqueous humours, the cavity of the aqueous humour will be always of the same size, and the cornea and lens at the same distance from each other; in the accurate production of which effects, he supposes it likely that the ciliary processes may operate by muscular action; an opinion which other facts in the course of the lecture tend to confirm.

The result of this enquiry, which was not carried on, as he remarks, in support of any particular theory, but with the sole view of discovering the truth, appears to be, that the adjustment of the eye is produced by three different changes in that organ; an increase of curvature in the cornea, an elongation of the axis of vision, and a motion of the crystalline lens. These changes, in a great measure, depend upon the contraction of the four straight muscles of the eye. Mr. Ramsden, from computations grounded on the principles of optics and general state of the facts, estimates, that the increase of curvature of the cornea may be capable of producing one-third of the effect, and that the change of place of the lens, and elongation of the axis of vision, sufficiently account for the other two-thirds of the quantity of adjustment necessary to make up the whole.

After this explanation of the mode by which the axis of vision can be elongated, and the convexity of the cornea increased in the human eye, for the purpose of its adjustment, Mr. Home was desirous of applying these observations to the eyes of other animals.

Quadrupeds in general must have their eyes fitted to see very near objects, as many of them collect their food with their mouths, in which action the objects are brought very close to the eye. Birds are under the same circumstances in a still greater degree with respect to their food; but from their mode of life, they also require the power of seeing objects at a great distance from the eye. Fishes, from the nature of the medium in which they live, must have some other mode of adjusting the eye than that of a change in the cornea, because that substance is possessed of the same refractive power as the surrounding fluid.

Quadrupeds have three modes of procuring their food; one by their fore-paws only, which they use like hands, as in the monkey tribe; the second by their fore-paws and mouth,

mouth, as the lion and cat tribe; the third by the mouth only, as all ruminating animals. These three different modes require the food being brought from different distances from the eye, and it is curious that the muscles of the eye are different in all the three tribes.

In the monkey tribe the muscles of the eye are exactly the same as in the human. In the lion tribe they are double in number, and the four intermediate muscles are lost in the sclerotic coat at a greater distance from the cornea than the others. In the ruminating tribe there are four muscles, as in the human eye; but there is also a muscle furrounding the eye-ball attached to the bottom of the orbit, round the hole through which the optic nerve passes, and lost upon the sclerotic coat immediately before the broadest diameter of the globe of the eye. The upper portion of this muscle is rather the longest, the insertion being nearly in a circular line at right angles at the axis of vision, but not to the axis of the eye from the entrance of the optic nerve.

In quadrupeds in general, the ball of the eye is broader in proportion to its depth than in the human subject. In the bull the proportion is $1\frac{1}{2}$ inch to $1\frac{1}{16}$. The cornea is larger and more prominent; its real thickness not easy to be ascertained, because, like that of the human eye, it readily imbibes moisture after death. When dried, it is thinner than the sclerotic coat in the same state. In ruminating animals, the cornea, though circular, has an oval appearance, arising from an opaque portion which is covered by a membrane. The ciliary processes, as in the human eye, are connected with the choroid coat; but they are larger, and are united at their origin with the iris.

This structure of the eye in quadrupeds, as far as it differs from that of the human eye, appears calculated to increase the power of adjusting it to near objects; and from the mode of life which these animals pursue, such additional powers appear necessary to enable them to procure their food with ease.

That birds, procuring their food by their beak, must require an adjustment to see very near objects, and that a degree of precision and facility, with regard to remote objects, is no less necessary from their situation during flight, are obvious circumstances. Mr. Home mentions some instances, in which birds are supposed to have seen distant objects with peculiar distinctness. It is related, that vultures and other birds very speedily repair to the place where a dead animal or other prey is exposed to them; and though there is nothing, perhaps, in these facts which suppose an acuteness of sight greater than that of other animals, yet it cannot be doubted, upon the whole, that these animals see with very great precision at vast distances.

The eyes of birds are larger in proportion than those of other animals, and broader in proportion to their depth, with a more prominent cornea than in the quadruped. The cornea in the goose is not united to the sclerotic coat by the terminating of one abruptly in the other; but the two edges are bevelled off and laid over each other for nearly $\frac{1}{16}$ th of an inch. This circumstance, as also the bony rim or apparatus furrounding the basis of the cornea, which is peculiar to this class of animals, and described by Mr. Smith, was well known to Haller. Mr. Home describes it more particularly, and states its use in the focal adjustment to be nearly the same as that described by Mr. Smith. The ciliary processes are larger and longer in birds than in other animals whose eyes are of the same size; and the marsupium or membranous process peculiar to the class of birds, is shewn by him to be very similar to that of the ciliary processes; but stronger in all its parts, and, like them, connected

neated with the crystalline lens at the one extremity, and at the other with the bottom of the eye. By experiments on this membrane he ascertained, that it is capable of muscular contraction: for, when the marsupium and crystalline lens of a goose's eye were exposed immediately after death, and the lens was pushed forwards so as to elongate that membrane from $\frac{7}{10}$ ths to $\frac{1}{2}$ ths of an inch, it repeatedly recovered its original dimension when the pressure was taken off. But when the parts had been left until all remains of life were gone, and the tension was then made as before to $\frac{1}{2}$ ths of an inch, it contracted only to $\frac{3}{4}$ ths; whence it was concluded, that the difference between the contractions was the effect of muscular action. It was observable, however, that in so minute a quantity it was easy to be deceived. Another experiment was therefore instituted upon the well-known fact, that in the act of dying, the muscles are found to contract to their utmost where there is no resistance to prevent such action; and that this is also found to take place in the greatest degree when the animal is killed by any violence committed upon the brain or spinal marrow. The crystalline lens of a turkey's eye was extracted, and immediately afterwards the turkey was killed by wounding the spinal marrow. The two eyes were taken out and put into spirits. In the one the marsupium had nothing to prevent its contracting to the utmost; while in the other the lens being in its natural situation could not allow of any unusual contraction. Some days afterwards the two eyes were examined: in the perfect eye the marsupium measured $\frac{1}{2}$ ths of an inch, and its different folds were semi-transparent; in the imperfect eye the marsupium measured $\frac{3}{4}$ ths of an inch, and the folds were much more opaque. Here then was a difference of $\frac{1}{4}$ th of an inch in the length of the two marsupiums: which could arise from no other cause than the one having contracted so much more than the other, which contraction must be considered to be muscular.

On a review of the peculiarities in the eyes of birds the author infers, that they tend to facilitate the lengthening of the axis of vision, and increasing the convexity of the cornea. The bony rim to which the muscles are attached, confines their effect to the broadest part of the eye; and as their action throws the cornea forward, the anterior rim of the bony edge yields to adapt itself to that change. The ciliary processes are long, and admit of the lens being moved forward during the adjustment for very near objects, which is performed with more facility than in other animals, while their action serves to bring it back to its place. The marsupium serves also to draw it backwards, and, by sustaining part of the pressure from behind, renders the cornea flatter, while the anterior edge of the bony rim is adapted to it in this state. It may be said that no such great change is necessary for vision with parallel rays; but Mr. Home remarks, that where vision is to be very distinct, a certain nicety of adjustment becomes necessary, and the action of the marsupium is probably intended for that purpose.

The subject of vision in birds is concluded by an observation, that one of the most beautiful illustrations of the combination of muscular and elastic substances is seen in the motion of the nictitating membrane. This membrane is elastic, and is connected by means of a tendon with two muscles situated upon the posterior part of the eye-ball. The action of these muscles brings the membrane over the cornea; and the instant they cease to contract, the elasticity of the membrane draws it back again.

The eyes of fishes have several peculiarities, and in many respects their structure differs from that which is observed in the quadruped and bird.

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The muscles of the eye, that correspond to the straight muscles in the quadruped, are four in number: they are however differently placed; they do not surround the eye-ball, but two of them are on that side of the orbit next to the nose of the fish, the other two on the opposite side; their attachment to the eye is close to the edge of the cornea; they do not pass round the eye-ball towards the posterior part, as in other animals, but are connected with the bones of the head at some distance from the eye on each side, so that they cannot at all compress the eye laterally; they can only pull it backward by the combined effect of their actions.

The bottom of the orbit on which the eye-ball rests is solid and adapted to it, there being no fat interposed between them as in other animals; and where the eye is removed to a great distance from the skull, and that cannot be the case, there is a strong cartilage projecting from the skull to the bottom of the eye, and that end of it next to the eye is concave, and fitted to the portion of the eye-ball directly opposite the cornea, just above the entrance of the optic nerve. This is considered as a fixed point upon which the eye moves; but it will also, from the situation of the muscles, allow the eye to be forced back upon it, and the whole eye to be flattened.

The shape of the eye differs considerably in different fishes; but in all of them the transverse diameter is the longest. In the haddock the proportion is $\frac{1}{10}$ ths to $\frac{9}{10}$ ths of an inch, and in some fishes it differs much more.

The size of the eye does not correspond with that of the fish; the salmon's eye being smaller than the haddock's.

The sclerotic coat is in some fishes membranous*, in some partly bone†, in others entirely so‡; but in general the posterior part is membranous, although the lateral parts are bone§.

The cornea is in general flat, not always circular in its shape, is very thin, made up of laminae, and does not lose its transparency in spirits, appearing like talc||. In others it is more convex, as in fish of prey; this appears to adapt it to the spherical crystalline lens, which in them lies directly behind it¶. The tunica conjunctiva forms the anterior layer of the cornea**, and in some fishes is quite detached.

(The Substance of Mr. Home's Concluding Lecture upon the Eye will be given in a future Number.)

NEW PUBLICATIONS.

Philosophical Transactions of the Royal Society of London, for the Year 1797, Part II.

Quarto, 541 pages, exclusive of the title, contents, list of presents, and index, with nine plates. Sold by Elmsly, London.

THIS part contains the following papers: 1. On the Action of Nitre upon Gold and Platina, by Smithson Tennant, Esq. F.R.S. 2. Experiments to determine the Force of Fired Gun-powder, by Benjamin Count of Rumford, F.R.S. M.R.I.A. (see Philos. Journal, i. p. 459). 3. A third Catalogue of the comparative Brightness of the Stars; with an in-

* Haddock. † Sword-fish. ‡ Devil-fish. § Mackerel. || Sword-fish. ¶ Pike. ** Haddock.

introductory Account of the Index to Mr. Flamsteed's Observations of the fixed Stars contained in the Second Volume of the *Historia Caelestis*; to which are added several useful Results derived from that Index, by William Herschell, LL. D. F. R. S. 4. An Account of the Means employed to obtain an overflowing Well, in a Letter to the Right Hon. Sir Joseph Banks, K. B. P. R. S. from Mr. Benjamin Vulliamy. 5. Observations of the changeable Brightness of the Satellites of Jupiter, and of the Variation in their apparent Magnitude; with a Determination of the Time of their rotatory Motions on their Axis. To which is added a Measure of the Diameter of the Second Satellite, and an Estimate of the comparative Size of all the four, by William Herschell, LL. D. F. R. S. 6. Farther Experiments and Observations on the Affections and Properties of Light, by Henry Brougham jun. Esq. 7. On Gouty and Urinary Concretions, by William Hyde Wollaston, M. D. F. R. S. 8. Experiments on Carbonated Hydrogenous Gas, with a View to determine whether Carbon be a Simple or a Compound Substance, by Mr. William Henry. 9. Observations and Experiments on the Colour of Blood, by William Charles Wells, M. D. F. R. S. 10. An Account of the Trigonometrical Survey carried on in the Years 1795 and 1796, by Order of the Marquis Cornwallis, Master General of the Ordnance, by Colonel Edward Williams, Captain William Mudge, and Mr. Isaac Dalby.

A Lecture introductory to a Course of Popular Instruction on the Constitution and Management of the Human Body, by Thomas Beddoes, M. D. octavo, 72 pages, price 1s. 6d. Printed for Cottle in Bristol, and sold in London by Johnson, 1797.

The author of this pamphlet was accidentally informed, by a practitioner in surgery, in the course of the year just expired, that he was desirous of giving a course of anatomical lectures in Bristol. To furnish individuals with so much knowledge of themselves as should enable them to guard against habitual sickness, and a variety of serious disorders, had long been an object of Dr. Beddoes's contemplation. He, therefore, proposed that the course should be modelled according to this idea. He remarked that a distinct exhibition of the larger lines of anatomy and physiology would be also the mode of instruction best adapted to young students in medicine; much observation of lectures having convinced him that extreme minuteness is only perplexing to beginners. This, joined to some other considerations, prevailed. Messrs. Bowles and Smith undertook the course, the intention of which is, to exhibit the structure of the body in a manner neither superficial nor tedious; to explain the functions of the parts as far as they have hitherto been investigated; to illustrate, by specimens, the principal deviations of these parts from their healthy conformation, and to intersperse such reflections as may be useful in physical education, and the whole conduct of life. Dr. Beddoes purposes to contribute his utmost assistance to the design, in whatever way that assistance shall, upon reflection, appear most likely to be effectual. This introductory lecture constitutes part of his exertions in a plan so truly calculated to promote individual happiness, and public welfare. He is desirous that its publication may produce similar undertakings elsewhere, and justly supposes that a communication of the fact, that these lectures are attended by an audience more than twice as numerous as the friends of that design expected, will tend to promote this purpose.

After this concise statement of an undertaking to which every friend of mankind must wish success, I must decline analysing the lecture, and content myself with observing, that the subject, which itself is highly interesting to all descriptions of men, is treated in a very perspicuous and impressive manner.



On Pumps by James Walker Esq.



Fig. 1

Fig. 2

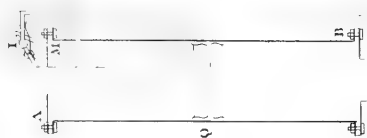


Fig. 3



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ARTICLE I.

*An Attempt to accommodate the Disputes among the Chemists concerning Phlogiston. In a Letter from Dr. MITCHILL of New York to JOSEPH PRIESTLEY, LL.D. F. R. S. &c. &c. Dated November. 20, 1797 *.*

ON reviewing the state of philosophical controversy as carried on both in Europe and America between the Phlogistians and their opponents; it has of late appeared to me, that much of the difficulty which attends the subject arises, as in abundance of other cases, from the want of a precise language and of a right understanding of each other's meaning. This was so evident to me in the present case, that I informed my audience of it in one of my public lectures in Columbia College, and added my belief, that due attention to terms, their application and use, would have great influence in bringing the dispute to a termination.

Having subjected water heated to the temperature of steam in an colipile, and directed the steam issuing from it to the surface of red-hot charcoal; the coal brightened, and a greater flame was observed near the spot against which the steam was made to play. Here was an occurrence opposing the common observation of mankind, that water will always extinguish fire by reason of its own incombustibility. Water kept at or below a certain temperature will extinguish fire, and so will oil; but if water be raised to a heat sufficiently high, it will also burn or undergo decomposition like oil. As far as I could judge from the phenomena before me, water in proper circumstances underwent a true combustion, and was inflammable for the same reason that oil was, because it contained a something that would burn; and this something seemed to be exactly similar to that which made oil capable of exhibiting flame. It struck me instantly, that the inflammability of

* Communicated by the Author. N.

this vapour proceeding from burning fat, from heated alcohol, from camphor, ether, coal, and a multitude of other substances, gave evidence of their possessing a principle enabling them to burn with flame after the same manner that water did. If there was this similarity or indeed identity of the inflammable radical among them, there appeared to be no more propriety in calling that radical *hydrogene* than in terming it *olegene*, *alcohologene*, *etherogene*, *radigene**, &c. To give the radical substance enabling oil, alcohol, ether and coal to burn with flame a name derived from water, because it enabled water to burn with blaze too, appeared to me partial, illogical and wrong, inasmuch as it constantly and unnecessarily brought water and its properties to mind whenever any thing was thought of that contained *hydrogene*; and by this unhappy association, besides the difficulty which attends the subject in point of fact, vastly greater difficulty was made to surround it by reason of the ill chosen and badly assorted terms employed in talking about it.

I had entertained no doubt, for two years, that *hydrogene* was an improper word for a nomenclature of science, and deserved to be struck out of the list: but as I was engaged in reforming another article of that arrangement, I chose not then to meddle with it; and I am glad I did not; since the prolonged disputes between the parties afford more weighty causes for an alteration of terms at this day than existed at any former time.

The circumstance common to all the processes I have mentioned, is "burning with flame or blaze," which, wherever it occurs, seems to indicate the presence of what has been called *hydrogene*. According to my present conception of the matter, this principle or substance common to so many bodies, and enabling them to undergo inflammation, may in strict propriety be called *phlogiston*. I always thought *phlogiston* a well conceived word, and have objected to it, not on account of the impropriety of the term as such, but because of the vague and unsatisfactory way in which it was used. If a definite signification can be affixed to it, I think the adoption of it will be still a great acquisition to philosophical language, and have a tendency to settle at least half the controversy which divides the chemists.

I propose, then, to expunge *hydrogene* and substitute *phlogiston* in its place. *Phlogiston* will thus be the radical term, and strictly mean the thing in combustible bodies which forms blaze or ignited vapour. The union of this with mere caloric will make *phlogistons* or inflammable air, the air which burns with blaze. The combination of *phlogiston* with oxygen will constitute water or the *oxyd of phlogiston*, one of the products of inflammation, and, like fixed air and other compounds formed during the same process, incombustible in common temperatures and circumstances afterwards. And the cause of this slowness to burn, of water and the other compounds which combustion furnishes, is owing to the large dose of oxygen with which they are charged, giving them little or no appetite for more. If this base be united to a yet larger quantity of oxygen, it will form the acid of *phlogiston*, or water soured by excess of oxygen, as *perhaps* (though I do not believe it) in what is termed the pyro-lignic and pyro-mucic acids, and perhaps in some other cases: but the readiness with which *phlogiston* parts with its surplusage of oxygen, turns back to water, and preserves itself in that oxydated form (as proved by the operation of sharp-pointed filaments under water in effecting the separation) shews that nature, in enabling the princi-

* So in the original: doubtless by oversight. N.

ple of inflammability to combine with oxygene, disqualifies the latter in most cases from becoming an acid with the former; unless it should be found (and in this I have no faith) that the formation of the native acids of vegetables is a process of this kind. Should this latter conjecture turn out to be the fact, there would be instances enough of *phlogistic* and *phlogistic acids*.

The nomenclature will then stand thus:

1.	2.	3.	4.
Phlogiston for Hydrogene.	Phlogisticous gas for Hydrogenous or inflammable air.	Oxyd of phlogiston for Water or oxyd of hydrogene.	Gaseous oxyd of phlogiston for Aqueous gas, or watery steam.
5.	6.	7.	
Phlogisticous and phlogistic acids. Probably no such things; the hydrogene or phlogiston not being capable of combining with oxygene beyond the de- gree of oxyd.	Phlogistates and phlo- gistites. Probably no such combination.	Phlogistures and phlogisturets; of sulphur, coal, phosphorus, iron and zinc, instead of The common sulphur, &c. of the laboratories and shops, &c. &c.	

On the decomposition of fat and oil by fire, it is known that a large quantity of water is formed, and this probably by an union of the base of vital air with phlogiston or hydrogene. The like obtains in the inflammation of alcohol, camphor, ether and coal, part of the phlogiston or hydrogene of which apparently turns to water by junction with oxygene. And the principle which in the first instance readily exhibits the blazing appearance is changed by and during that operation to a something much more difficult to inflame by any after-process.

If hydrogene or phlogiston is the material which inflames in the substances already mentioned, there is presumptive evidence, upon the face of the subject, of its existence also in the common sulphur, phosphorus, zinc, and iron of the laboratories. I do not mean to say it is a *necessarily* constituent part of either of those bodies; for I believe they may exist without it, or at least they may be conceived to exist abstractedly from it. They therefore stand very well in the nomenclature as simple substances. But if these substances, such as we commonly get them after exposure to the common atmosphere in ordinary temperatures, are taken for simple or pure elementary bodies, the persons who consider them so fall into a great mistake. In their usual forms they are all incorporated with hydrogene or phlogiston, and from it derive their capacity to burn with flame. This will be the more clearly seen by considering them more particularly one by one.

1. Of Sulphur. The Phlogistians say sulphur is composed of phlogiston united to vitriolic acid; consequently if any thing takes away that ingredient from the acid, this will turn to brimstone. The antiphlogistians affirm sulphur to be a simple body, uncombined chemically with any thing; and that it becomes sulphuric acid by junction with oxygene. Now both parties have reasoned in a manner that does not by any means satisfy me. They have viewed the combustion of sulphur in the abstract, rather than taken it up as it is. The fact is, that the acid formed in the combustion of sulphur is not the solid crystallized mat-

ter of the glacial oil of vitriol, but a solution of these crystals in water. The existence of vitriolic acid in a fluid form implies necessarily the co-existence of water. The formation of the water in the inflammation of sulphur appears to have been passed over by both parties, though the interpretation of this part of the process seems to me to furnish the means of reconciliation. Thus, while the pure sulphur combines with one portion of oxygen to make the acid, the hydrogen or phlogiston unites with another parcel of it to form the water in which the acid dissolves. Common brimstone then is not a simple substance, but is a *phlogisture of sulphur*. And this is confirmed by the fact that, where combustion is restrained, the sulphur may be resolved into hepatic gas; the phlogiston turning with caloric into inflammable air and dissolving some of the sulphur. The formation of common hepatic gas seems to evince the same thing; for while the potash seizes the sulphur, the hydrogen or phlogiston is set loose, turns with caloric to hepatic gas, and snatches, as it departs, a portion of the sulphur from the alkali. Thus it appears that the two systems are reconcilable with each other. When the Old Chemists talk of phlogiston, they should define it to be that thing which burns with flame, and when united to oxygen forms water. When the new ones make experiments on sulphur, they should remember that the common material called by that name is not the abstract, pure, uncombined elementary thing they intend in their nomenclature.

2. In like manner the phenomena attending the inflammation of phosphorus seem to have been as negligently interpreted. Phlogiston added to phosphoric acid was said by some to constitute phosphorus; while oxygen added to phosphorus made phosphoric acid in the opinion of others. But these were a kind of chemical theorems, true only in the abstract experiments. We find that phosphorus burns *with flame*, and *water* is exhibited during the process. All that needs be said about it is, that in common circumstances phosphorus, though capable of existing *per se*, has a very strong attraction for hydrogen or phlogiston; and in ordinary cases attaches more or less of it to itself. During its inflammation part of the oxygen, as in the case of the sulphur, combines with the phlogiston into water; and another part of it joins the oxygen to constitute the acid. In estimating the *whole* of the process, the candid partisans of both sides will allow that the substance under consideration parts with its phlogiston and borrows oxygen, and thus water and the acid dissolved in it are formed.

Where is the harm of owning that common phosphorus contains a portion of hydrogen united with it? It does not invalidate the modern theory: but it shews that the objections of the ancient doctrine were not frivolous, as they have by some been deemed to be; but on the contrary very substantial, and not capable of reconciliation upon any other plan, than I know, than the one herein suggested.

3. Zinc may be abstractedly considered as a simple body, and with propriety placed as such in the catalogue. Commonly, however, it is presented to us in close connection with hydrogen, for which its attraction is so strong that they commonly appear in the form of a *phlogisture of zinc*. When that composition is employed for experiments, it is very easy to conceive how, when such zinc is exposed to a sufficient heat in an open fire, the phlogiston dislodged, and immediately becoming phlogistic gas or inflammable air, shall take upon itself the form of flame and constitute water; while the oxygen combines with the metal into a white oxide, the flowers of zinc. So if the same compound be dissolved in sulphuric acid,

acid, the phlogiston displaced will turn to inflammable air, while the acid and the zinc form white vitriol. In this way some of the phlogistous or inflammable gas may be accounted for, as extricated from the metallic preparation: and at the same time I see no objection to deriving the rest of the great quantity afforded by this process, from the decomposition of part of the water or oxyd of phlogiston. To accommodate matters then, the advocates of the Lavoisierian theory should concede that zinc, in common circumstances, is associated with hydrogen or phlogiston. And the disciples of Stahl should on their side allow that zinc cannot be considered as a pure metal, while alloyed or blended with phlogiston or any other foreign ingredient. The material they have all worked upon is not the uncombined metal, but a phlogisture of zinc.

4. Some sorts of iron treated by heat alone afford phlogistous or inflammable air. The same metal may be made to burn with flame, and, when treated with sulphuric acid, affords much phlogistous air. What then is the thing commonly called iron? Is it a pure and unmixed substance? Or is it a compound of elementary iron with hydrogen or phlogiston? The facts enumerated lead conclusively to the latter opinion. The Phlogistians are right then, when they say common iron is a compound: and they are right when they say the inflammable air obtained from it is nearly pure phlogiston. And the Antiphlogistians are justifiable in placing in their enumeration of simple bodies, such a thing as elementary iron is or may be imagined to be; and in ascribing the production of hydrogenous or phlogistic gas to a decomposition of part of the water. The compound called iron then gives out something, and takes in something, in all the common processes. And the modern chemists should correct the mistake they appear generally to have fallen into, of taking it for granted, that was a simple substance which in fact is a chemical composition of iron with hydrogen. And thus fiery cinder, which evidently differs from hæmatites or any pure oxyd of iron, may be a triple compound of iron, hydrogen or phlogiston, and oxygen; which just about corresponds with your idea that it consists of iron and water.

It will not follow from all this, that because phlogiston or hydrogen so generally exists in combination with zinc and iron, it must be an ingredient in all metals. For gold, arsenic, silver, platina, mercury, copper, tin, lead, bismuth, cobalt, antimony, and manganese are capable of existing without it, and accordingly do not commonly burn with flame, nor afford inflammable air by solution in acids; though if ever they exhibit in any of their states blaze by burning, or phlogistous air with acids, this will only evince the existence of hydrogen in them in such cases. Both parties may thus allow that some metals contain phlogiston, and some do not. Nor will charcoal, as has sometimes happened, be confounded with phlogiston according to this view of the matter; though hydrogen is often blended with it. If coal at any time affords inflammable air, this is no evidence of the conversion of that substance into phlogistous gas, but merely a proof that the coal when submitted to experiment was combined with the basis of inflammable air, which it could part with and still remain coal, though in that case incapable of burning with flame; but, in a sufficient exposure to heat and oxygenous air, taking on without blaze the form of carbonic acid gas. It may be conceded then on both sides, that, though phlogiston or hydrogen may exist with coal, nevertheless coal can exist without phlogiston.

I know not in what manner these considerations may impress your mind. I have
 3
 flattered

flattered myself they have a tendency to accommodate and reconcile the gentlemen of both sides to each other, and to the truth. But in this if I have deceived myself, I have only to appeal to your known experience and candour. It does however seem to me capable of getting through with much of the discussion not yet solved between Mr. Kirwan and the French philosophers; and between yourself and Messrs. Adet and Maclean; and I suspect the celebrated experiments of Mrs. Fulham are in no wise repugnant to this mode of interpretation.

Permit me to express my satisfaction, before I conclude, that you have not given up the points before the philosophical world, without due examination. I no more like unanimous decisions in haste upon a question of science than of legislation. The sure way to introduce errors both into laws and into philosophical performances, is to assent to new bills and projects upon the credit and authority of the proposer, without compelling them to undergo the amendments of debate. And I cannot help considering your refusal to agree to *all* that the New Nomenclature Men have asserted and recommended, as a fortunate event in philosophy; as I am sure, with respect to myself, the opposition you have made has caused me to consider the subjects in dispute with greatly more attention than I otherwise should have done. With much respect I beg leave to assure you that I am, &c.

SAM. L. MITCHILL.

ANNOTATIONS upon the preceding LETTER.

Steam in an eolipile.] It is well known that ignited charcoal in the gun-barrel decomposes water, and that the products are hydrogen and carbonic acid gas. In this case the carbone is burned, and the hydrogen unburned. Are we to suppose that the water was first decomposed in Dr. Mitchell's experiment, and then recombined by a second combustion of the hydrogen? or was the effect any thing more than an increased energy of combustion by the greater afflux of atmospheric air mechanically impelled by the steam? Dr. Lewis * found that the steam from the eolipile always extinguished his fires, unless the vapour was made to pass through a portion of the atmosphere. Whence I infer that the quantities of heat carried off and rendered latent in giving elasticity to the hydrogen and carbonic acid are so great as speedily to diminish the temperature, and terminate the combustion of the charcoal; unless, as in the experiment of the gun-barrel, there be a sufficient supply of heat from without.

By this unhappy association.] The argument of association of ideas seems to militate strongly against the reception of the word phlogiston in a new sense.

Burning with flame or blaze.] Though the author's arguments in favour of hydrogen, as the exclusive cause of flame, may be hypothetically applied to all the instances in which this appearance is seen; yet the contrary position, namely, that bodies, whether mechanically divided, such as the powder of resin or coal, or chemically, as zinc in the elastic state during sublimation, may be set on fire and exhibit the appearance of an ignited transparent fluid, such as flame, seems at least equally probable. It cannot therefore be stated as matter of fact, that where flame is there must necessarily be hydrogen.

* *Philosoph. Commerce of Arts*, p. 21.

In the decomposition of fat and oil by fire, &c.] The difference between the leading assumption in Professor Mitchell's letter, and the theory formerly maintained by Mr. Kirwan, and stated by him in the Introduction to his Work upon Phlogiston, appears to be, that the former does not suppose his phlogiston, or principle which affords flame, to be essential to the metallic state, but merely, as I conceive it, accidental in that class of combustible substances. He follows Mr. Kirwan, by first directing his attention to the universality of hydrogen in organized bodies, and then endeavouring to shew that it exists in sulphur, phosphorus, zinc, and iron, in their ordinary combustible state. It has not yet been shewn that aqueous sulphuric acid can be produced by the combination of sulphur and oxygen, without the presence of water ready formed. The same observation more strongly applies to phosphorus, which Lavoisier converted into the solid acid by the burning glass in a vessel of oxygen gas over mercury. In the observations on zinc and on iron, we look in vain for the experimentum crucis, which must be exhibited before the supposed fact that metals contain hydrogen can be admitted.

I have ventured to make these remarks on the Professor's ingenious communication, without aiming at the establishment of positions contrary to those he has offered. The questions he treats must be decided by plain facts, if such can be found. Till this be done, they remain among the mass of unknown propositions, concerning which, we ought neither to affirm nor deny.

II.

Experiments on the Composition and Proportion of Carbon in Bitumens and Mineral Coal.
By RICHARD KIRWAN, Esq. F. R. S. L. & E. M. R. I. A. &c.

AN exact knowledge of the component parts of the different species of mineral coal, and also of bitumens (substances which most of them contain) forms an object of some importance, not only to the naturalist whose views are merely speculative, but to the practical œconomist, who wishes to extract from each species all the advantages it is capable of yielding, and to be enabled to compare the various kinds afforded by different countries, in order to obtain and employ that which shall on the comparison appear to him best suited to his intentions.

In effect, coals are not only applicable to the more usual purposes of combustion—an use, simple as it may appear, attended, according to their various species, with a considerable difference of calefactive power both in intensity and duration, but also to the production of varnishes, much more advantageously applicable in many instances than those extracted from the Vegetable Kingdom, as Lord Dundonald has discovered, and abundantly proved*, and also of that charred residuum called coal, the only one that can be resorted to in many cases, and, in most, superior to vegetable charcoal.

* Upon the most minute enquiry why coal-varnish is not more commonly employed in paying the bottoms of ships, I have been informed the principal reason is, that it succeeds too well; the ships not requiring such frequent repair. K.

Coals and bitumens are, however, substances that resist the usual modes of analysis; they elude the action of aqueous, acid, alkaline, or spirituous menstrooms; and distillation, the only mode hitherto used, confounds and varies their natural contents.

Reflecting on these obstacles to an exact discrimination of bitumens and coals, and of the various kinds of these last, it occurred to me, that partly by combustion and partly by their efficacy in decomposing nitre, the secret of their internal composition might possibly be unveiled.

1. Combustion. I have observed that all the species of solid bitumen properly so called, when laid on a red hot iron, burn with a large bright flame, smoke and foot, leaving none or scarce any coaly residuum, and only a little ashes:

That the softer bitumens, as maltha, burn in the same manner, leaving no coal, but only a little ashes, and requiring no increase of heat for their entire consumption:

That asphalt burns with flame and foot, but melts and swells, and requires for its entire consumption an increase of heat, leaving scarce any coal, and but little ashes.

It is moreover well known that liquid bitumens contain inflammable air and carbon: that they absorb atmospheric air when long exposed to it and light: that, in consequence of this absorption, they are thickened, blackened, and condensed, first into mineral tar, then into mineral pitch or maltha, and lastly into asphalt: that almost all species of mineral coal yield more or less of both species of bitumen in distillation, leaving a shining coaly residuum; but that the proportion is variable in every species according to the degree of heat applied; that the residuum always obstinately retains a proportion of bitumen, and that consequently distillation, in addition to its other imperfections, is an insufficient medium whereby to discern the proportion of carbon and bitumen, and consequently to discriminate the various sorts of mineral coal from each other.

2. Decomposition of nitre. It has long ago been remarked by the justly celebrated Macquer*, that nitre detonates with no oily inflammable matter, until such matter is reduced to a coal, and then only in proportion to the carbonaceous matter it contains. An observation the truth of which will fully appear in the subsequent experiments.

Hence it occurred to me that, since in the act of detonation nitre is always totally or partially decomposed, and since, where carbonaceous compounds are employed, this decomposition arises solely from the mere carbonaceous parts, and, every thing else being equal, is proportioned to the quantity of mere carbon they contain; and since most species of coals are compounds of mere carbon and bitumen, as appears by the products of their distillation; it should follow, that, by the decomposition of nitre, the quantity of mere carbon in a given quantity of every species of coal may be discovered; and this being known, that of bitumen may be inferred; and, the other unessential ingredients being detected by incineration, the whole contents of coaly substances might be ascertained.

The composition of bitumens also, as far as relates to their proportion of carbon and oil, may be evidenced in the same manner; and here it is to be observed, that the bitumens I here consider are those that are found in a dry or solid state, and that these contain a larger proportion of carbon than the liquid bitumens; for, though these last also contain carbon, it being an essential component part of all oils, yet this portion does not extricate or educe

* *Diction. Chym.* second edition, 481.

any air from nitre, nor consequently contribute to its decomposition, as the subsequent experiments sufficiently evince; but is consumed partly by the pure air spontaneously emitted by nitre during its ignition, and partly by the ambient atmospheric air.

Nay, when mineral coal is employed in the decomposition of nitre, the share which the mere carbonaceous part of the bitumen contained in it contributes to the decomposition will be found so small that it merits no consideration in the general account.

The first step towards carrying this analytic plan into execution must therefore be, to determine the quantity of pure carbon necessary to decompose a given quantity of pure nitre. But here many practical difficulties occur, which shall presently be mentioned. The most perfect method of obviating them was that employed by the ever memorable Lavoisier: he mixed the purest nitre with charcoal also purged of the inflammable as well as other airs and water which it usually absorbs, in the proportion which, after several trials, he found requisite for the entire decomposition of that salt, rammed them into a copper tube, fired them, and continued the inflammation under water; by which means the charcoal was acted on solely by the air educed from the nitre to the entire exclusion of the external air, and this air was educed solely by the ignited charcoal to the entire exclusion of external heat; advantages that cannot be procured by the usual mode of effecting this decomposition. Thus he found the proportion of charcoal necessary for the entire decomposition of nitre, to be as 1 to 7,57, or, in other words, that 13,21 parts charcoal decompose 100 of nitre*: and yet even in this experiment I find a small inaccuracy, as he did not take the water employed in mixing the nitre and charcoal into the account; and hence, and for some other reasons, the detail of which would lead me too far, I think the proportion should be as 1 to 7,868 nearly, or that 12,709 charcoal decompose 100 of nitre; but the difference is of little importance.

This mode of experimenting, however, is inapplicable on the present occasion; the different species of mineral coal being not so readily inflammable as to carry on the combustion in this manner. Hence I contented myself with the common manner only, using such precautions as to render its results tolerably uniform, and repeating each experiment several times.

I examined the purity of the nitre I employed by nitrated silver, and found by the quantity of salited silver produced, that 480 grains of the nitre contained 3,5 grains of common salt (135 grains of muriated silver, indicating 100 of common salt): hence the constant quantity of nitre I used was 483,5 grains, except in the experiments on bitumens, as I had not enough of them to expend in so large a quantity of nitre.

The nitre was heated barely to redness, before any coal was projected on it, in a wind furnace and a very large crucible; upon this uniform degree of heat much of the uniformity of different experiments on the same species of coal depends.

In my first experiments the coals were reduced to a very fine powder, and then projected on the ignited nitre; but I observed that by this method much more of each species of coal was requisite to alkalyze the standard quantity of nitre, than when it was reduced to a coarse powder about the size of a pin's head, or somewhat larger; and the reason is, that by the force of the explosion much of the finer powder is carried off, without having been in

* 11 Mem. Scav. Etrang. 616.

contact with the nitre. Hence in the experiments of Mr. Hielm on the quantities of charcoal of different woods requisite to alkalyze 100 parts of nitre, we find these quantities to bear for the most part some analogy to their specific gravities, being generally smaller when the specific gravity of the charcoal is lighter; thus*:

Grains requisite to alkalyze 100 grains of nitre.					
	Specific gravity.		First experiment.		Second ditto.
Oak-coal	— 0.332	—	35	—	30
Birch-coal	— 0.542	—	22	—	22
Pine-coal	— 0.280	—	29	—	20
Fir-coal	— 0.441	—	33	—	25
Coak	— 0.744	—	19		

Another circumstance of great importance towards procuring just and uniform results is, that the projections of coal should succeed each other without delay as soon as the flame ceases; for, as ignited nitre gives out pure air spontaneously, and so much the more as it is more heated, the acid will be decomposed, and the nitre alkalyzed, by a quantity of coal so much the smaller as the intervals of projection are longer. From inattention, perhaps, to this and the last-mentioned particular, as well as from various conditions of common charcoal, which seldom contains less than $\frac{1}{12}$, and often $\frac{1}{6}$ of its weight of moisture and absorbed air, proceeded the various results of different chemists, with respect to the proportion of it necessary to alkalyze nitre.

It is almost superfluous to add, that the charcoal should be projected in very small portions. I seldom projected more than one or two grains at a time: each operation lasted from 20 to 25 minutes nearly.

There is always some portion of nitre undecomposed, being protected by the surrounding alkali. This error is unavoidable, but very small. Even the position of the crucible in the furnace is not indifferent; for, if it be near the flue, more coal must be employed, which I attribute to the torrent of air, which, in that case, affects and carries away more than when the crucible is nearer to the anterior part of the furnace.

It may, perhaps, be suspected that this and some other incidental errors may be avoided by previously mixing the nitre and coal, and projecting the mixture in small portions into a red-hot crucible; but not to mention that this method supposes the due proportion of these two substances to be known, which cannot be till after the experiment, and that also every atom of these substances is in perfect contact with the other substance, else they cannot act on each other—independently, I say, of these unfounded suppositions, this mode of experimenting is still more fallacious than the former, as during these projections a considerable proportion of the nitre is scattered and dispersed, and may be seen adhering to the sides of the crucible. This loss being repeated at every projection, becomes at last intolerable.

I now proceed to relate the experiments themselves, conducted in the manner I have mentioned. The different species of coal and bitumen, whose composition I have thus examined, were Kilkenny coal, Maltha, Asphalt, Lancashire cannel, Slaty, Scotch cannel, Whitehaven, Wigan, Swanfey, and Leitrim; selecting of each sort the purest specimens, free from pyrites and visible stony matter.

* Schwed. Abhandl. 1787, 188.

Kilkenny Coal.

ITS colour is black, and, when fresh broken, frequently violet.

Lustre 4, Metallic. Transparency 0*.

Fracture foliated, the course of the lamellæ variously and confusedly directed. Fragments rather sharp, and often discovering between the distinct concretions whitish laminations.

Hardness 7. Specific gravity 1,526.

Does not burn until wholly ignited, and then slowly consumes without caking or emitting flame or smoke. 266 grains of it exposed to a heat of 27° Wedgwood in a crucible for five hours, did not lose their lustre until almost $\frac{2}{3}$ of them had disappeared, and at last left reddish ashes amounting to 7,13 grains, nearly 2,7 per cent. Projecting this coal in fine powder on 480 grains of pure ignited nitre, I found the salt required 65 grains of the coal to alkalinize it, but only 50 grains when in coarse powder; and in a third experiment, when the crucible was farther from the flue of the surface, only 49 grains; so that I look upon 50 grains as being in round numbers nearest to the truth. That is, in the proportion of one part of Kilkenny coal to 9,6 of nitre: or, 100 parts of nitre require for their decomposition 10,416 of Kilkenny coal.

This proportion of coal is much smaller than that of charcoal in Mr. Lavoisier's experiment, which we have seen to be as 1 to 7,57, or as 13,21 to 100, which I attribute to the advantageous mode in which his experiment was instituted, as already explained; whereas in mine, and the usual way, the decomposition of nitre is promoted by the external heat applied, as well as by the coal, and consequently less of the coal is employed.

From the experiments of Scheele one might be led to infer, that the proportions of charcoal and nitre necessary to the alkalization of this latter, approach still nearer to each other

* The figures made use of by this author to denote degrees of lustre, transparency, hardness, and sharpness of fragments, are explained in the following Table, which I have constructed from his section upon the distinctive characters of minerals.

Numbers or Degrees.	Lustre.	Transparency.	Fragments.
0	None.	None.	Perfectly blunt.
1	Exceedingly weak; or a few shining particles.	Only at the edges.	Less blunt.
2	Glossy; not more than of silk.	Transmits light, but not sufficient to discern objects.	Sharp.
3	Shining; as of crystals or of metals not much polished.	Transmits light enough to shew objects imperfectly through the mass.	More sharp.
4	Brightest; as of diamonds or polished metals.	Objects clearly distinguished through the mass.	Most sharp; as of glass.

On account of the higher range of numbers, degrees of hardness could not conveniently be included in the above Table. No. 3, denotes the hardness of chalk; 4, a superior hardness, but yet what yields to the nail; 5, that which will not yield to the nail, but easily and without grittiness to the knife; 6, that which yields more difficultly to the knife; 7, that which scarcely yields to the knife; 8, that which cannot be scraped by a knife, but does not give fire with steel; 9, that which gives a few feeble sparks with steel, as basalt; 10, that which gives plentiful lively sparks as flint. N.

than in Lavoisier's statement, and consequently much nearer than in mine: for, in his Essay on Plumbago, he tells us that five parts nitre are sufficient to consume one of charcoal, and consequently it should seem that one part charcoal should decompose no more than five of nitre. The consequence however is not just, for undoubtedly five parts nitre would consume one of charcoal, but it does not thence follow that they would not consume still more. On the other hand, he found that ten parts nitre were necessary for the consumption of one part of plumbago; whence it follows, that one part of plumbago decomposes ten of nitre, otherwise nine parts nitre would suffice to consume it, and the tenth would have been unnecessary, as it acts only as it is decomposed. Now this proportion approaches very nearly to my results; namely, one of charcoal to 9,6 of nitre.

Hence, and since Kilkenny coal in the preceding experiments shewed no sign of its containing any thing bituminous, I take it for granted that it consists almost entirely of pure carbon; and since 50 grains of it alkalize 480 grains of pure ignited nitre, that in all the subsequent experiments on other species of coals or bitumens free from sulphur and iron, the decomposition of this standard quantity of nitre will indicate, in the quantity of coal necessary for that decomposition, the presence of 50 parts of mere carbon.

Before I proceed to the recital of other experiments, I must mention another circumstance that occurs in making them, which is, that, after the inflammation ceases, a hissing noise is perceived for a long time, and is increased on adding fresh quantities of coal, even when the nitre is seemingly decomposed. This seemed to me to arise from the decomposition of the nitrous air, or mephitized nitrous acid, of which a portion is always retained by the alkali; and consequently I paid no attention to it, but always ceased adding coal when the inflammation totally ceased.

Maltha.

ITS colour is dark-brown, or black.

Lustre o. Transparency o. Fracture uneven, tough. Specific gravity 2,070.

It feels somewhat greasy, yields to compression, has a heavy smell, acquires a polish when scraped, does not adhere to the tongue, or stain the fingers; its flame high and bright, leaving no coal, but only a little ashes.]

Having but a small quantity of this substance, I on this occasion used only 240 grains of nitre: when it was heated to redness, I threw on it one grain of vegetable pitch; it immediately inflamed, but floated quietly on the surface of the nitre, and decrepitated like common salt from the moisture it contained: the flame was partly white from the action of the air spontaneously emitted by the nitre, and partly yellowish from the action of the ambient atmospheric air, but steady, and unattended with those turbulent gushes that attend the decomposition of nitre by carbonaceous substances.

I then gradually projected on it 55 grains of maltha, which was all I had: this burned just as the pitch, but attended with a blacker smoke; yet the nitre was so far from being alkaliized, that, to produce this effect, I was obliged to throw on it 29 grains of cannel coal.

Now 33,5 grains of cannel coal, if it alone had been used, would suffice to alkalize 40 grains of nitre: it will presently be seen, therefore, the 55 grains of maltha and the one grain of pitch contained no more carbon than $33,5 - 29 = 4,5$ grains. Therefore 100 grains of maltha contain no more than 8 grains of carbon. And as these 8 grains of carbon provoked no turbulent eruption of air from the nitre, it is plain they did not contribute

to its decomposition, but were taken up by the air it spontaneously emitted, and partly by the ambient atmospheric air.

Asphalt.

ITS colour is greyish black. Lustre 2.3. greasy. Transparency 0.

Fracture perfectly conchoidal. Hardness from 7 to 8, very brittle. Specific gravity from 1.07 to 1.165 by my trials. It feels smooth, but not greasy; has no smell, except while pounding; does not stain the fingers; when heated it melts, swells, and at last inflames; but it requires, for inflammation, a higher heat than maltha does, and leaves no coal, and scarce any ashes.

Of this bitumen I found 161 grains requisite to alkalize the standard quantity of nitre: it visibly educed air from the nitre; for there were eruptions from time to time. I suppose when the more oily part was consumed, and the carbonaceous laid bare, much of the flame was also yellowish. Hence 161 grains of asphalt contain only 50 of mere carbon, that is, nearly 31 per cent.

Mr. Thory, burning it in a low heat, found it to leave about $\frac{1}{3}$ of its weight of coal after melting, swelling and inflaming as usual*: however, his asphalt was not perfectly pure, as he obtained sulphur from it.

Cannel Coal.

ITS colour is black. Lustre 2. Common when fresh broken, often barely 1. Transparency 0.

Cross fracture conchoidal. Fragments rather sharp. Hardness from 7 to 8. Specific gravity by my trials 1.232, by Dr. Watson's 1.273; does not stain the fingers, easily kindles without melting, and burns with a large bright flame, but of short duration, leaving a large coally residuum; does not cake. 240 grains of it, heated until all the coally part was consumed, left 75 grains of reddish brown ashes, mostly argillaceous, that is, 31.2 per cent.

66.5 grains of it were sufficient to alkalize the standard quantity of nitre. It burned with a large bright flame, except the last portion, which was yellowish, the pure air of the nitre being then exhausted. Hence 66.5 grains contained 50 of pure carbon, and 2.08 of ashes; then deducting 52.08 from 66.5, we find the quantity of bitumen equal 14.42. Then 100 parts of it contain 75.2 of carbon, 21.68 bitumen of the sort called maltha, and 3.1 of ashes.

I take this bitumen to be maltha, from its quick inflammability, and the short duration and brightness of its flame, both which properties indicate the most inflammable of the bitumens, and whose flame is least durable from its refusal to cake (caking being a property arising from the fusion of asphalt), and the difficult combustibility of the carbonaceous substance that remains after the cessation of its flame—qualities that counter-indicate asphalt.

Slaty Cannel Coal.

THAT which I employed was from Ayrshire in Scotland, the only one of this sort imported to Dublin.

Its colour is black.

* 6 Crell's Chym. Jour. 62.

Its lustre 2. Common. Transparency 0. Its fracture partly flat, partly imperfectly conchoidal; fragments sharp.

Its hardness from 5 to 8. Specific gravity 1.426 by my trials.

It burns like the compact cannel, but ceases sooner to flame; does not cake—leaves a stony residuum. 240 grains of it, treated as before mentioned, leave 50 of reddish grey ashes, equal 20.83 per cent. From the smell that issues from it during ignition, I am led to think it contains some portion of sulphur.

To alkalyze 480 grains of nitre, 105 grains of this coal were employed. It burned like the former with a large white continued flame, except the last portions. Hence this quantity contained 50 grains of mere carbon; and since it also contained 20.83 of ashes, the remainder, namely 34.15, must have been bitumen. Then 100 parts of it contain 47.62 of carbon, 32.52 of bitumen, and about 20 of ashes. Some deduction however from these quantities of carbon and bitumen may be made by reason of the small proportion of sulphur contained in it. This bitumen I take to be maltha, and not asphalt, for the reasons I mentioned in treating of compact cannel.

It is from a coal of this sort that Lord Dundonald extracts his tar, as maltha easily distills; but it is probably of a better kind, as this stony kind exists mostly in Ayrshire.

By his Lordship's mode of distillation, however, much seems to be lost during the internal combustion. I should think the Prince of Nassau Saarbruck's method in this respect more advantageous. Mr. Sage tells us, that, by distillation, he obtained from cannel coal $\frac{1}{2}$ of its weight of tar*; but Mr. Faujas, who uses Lord Dundonald's method, obtains from the coal of Decire, which seems to be of this kind, only 4 per cent. of tar†. Faujas also observed, that this tar is gradually converted into asphalt by long exposure to the air, which confirms the difference I have established between the two bitumens.

Whitehaven Coal.

ITS colour is black.

Its lustre 3. Greasy. Transparency 0.

Its fracture plane foliated. Its fragments 2, often discovering quadrangular or cubic distinct concretions, sometimes intersected with brownish red flakes.

Its hardness 6, very brittle. Specific gravity 1.257 by my trials. Stains the fingers, particularly when moist.

It burns at first with a clear flame, and for a long time; but at last cakes. 240 grains of it, after five hours strong heat, left only 4 grains of reddish ashes, or about 1.7 per cent.

The standard quantity of nitre was alkalyzed by 88 grains of this coal. Hence 100 grains of it contained nearly 57 of mere carbon, 41.3 of a mixture of maltha and asphalt; and 1.7 of ashes. That it contains both maltha and asphalt is evident from its flame and caking. The proportion I cannot exactly ascertain, but most probably the asphalt predominates.

Wigan Coal.

ITS colour is black.

Its lustre 3. Greasy. Transparency 0.

Its fracture plane foliated. The lamellæ, some uniformly, some promiscuously directed. In the grofs often flat; forms separate concretions, often with bright yellowish illinitions.

* Roz. Journ. p. 387.

† Roz. Journ. p. 188.

Its hardness 6. Specific gravity 1,268 by my trials. It burns with a bright flame and quicker than the foregoing, and is less apt to cake. 328 grains of it, exposed as the former to a strong heat, left 5,13 grains of ashes, that is, 1,57 per cent. 81 grains of it decomposed 480 grains of nitre. Hence 100 grains of it contain 61,73 of carbon, 36,7 of a mixture of maltha and asphalt, and 1,57 of ashes.

It seems to contain a larger proportion of maltha, with respect to its quantity of asphalt, than Whitehaven coal does.

Swansey Coal.

ITS colour is black.

Its lustre 2. Transparency 0.

Its fracture foliated; but some lamellæ being at right angles with the other, give it a fibrous or striated appearance. Fragments 2.

Its hardness 5, very brittle. Specific gravity 1,357 by my trials.

It burns more slowly than the former, and cakes.

240 grains of it, treated as the former kinds, left 8 grains of yellowish red ashes; that is, equal 3,33 per cent.

Of this coal 68 grains were requisite to decompose 480 grains of nitre. Then 100 grains of it contain 73,53 of carbon, 23,14 of a mixture of maltha and asphalt, and 3,33 of ashes. The asphalt seems to predominate.

Leitrim Coal.

ITS colour is black.

Its lustre when fresh broken 3. Transparency 0.

Its fracture foliated. Its fragments 2.

Its hardness 6, very brittle. Specific gravity 1,351 by my trials. It slightly cakes.

240 grains of it left, after three hours exposure to heat, 12,5 grains of reddish grey ashes, that is, equal 5,2 per cent.

The decomposition of the standard quantity of nitre required 70 grains of this coal. Hence 100 grains of it contain 71,43 of carbon, 23,37 of a mixture of maltha and asphalt, and 5,2 of ashes.

Newcastle Coal.

I HAD none of this kind of coal; but, according to Dr. Watson's experiment, it left on distillation a coally residuum amounting to 58 per cent. and hence contained about 40 of a mixture of asphalt and maltha, in which the former appears to predominate. Hence it much resembles the Whitehaven coal; but it evidently contains sulphur also, which that of Whitehaven seldom does.

A synoptical View of the Contents of Bitumens and different Sorts of Mineral Coal.

100 parts	Carbon.	Bitumen.	Ashes.	Specific gravity.
Maltha	8	—	—	2,070 *
Asphalt	31	68	—	1,117
Kilkenny	97,3	—	3,7	1,526
Compact cannel	75,2	21,68 maltha	3,1	1,232
Slaty cannel	47,62	32,52 maltha	20	1,426
Whitehaven	57	41,3 mixt	1,7	1,257
Wigan	61,73	36,7 mixt	1,57	1,268
Swansey	73,53	23,14 mixt	3,33	1,357
Leitrim	71,43	23,37 mixt	5,20	1,351
Newcastle	58	40 mixt	—	1,271

To these results I shall add a few more taken from a Treatise on Pit Coal, lately published by Signior Fabroni. The Italian coals were examined by himself; the French and German by other chemists—All by distillation.

100 parts	Carbon.	Bitumen.	Ashes.	Specific gravity.
Coals of Halles	86	12	—	—
of Tudertino	25	75	—	—
of Cortolla	45	43	12	1,403
of Macinaia	60	37	3	1,411
Stony of ditto	12,5	37,5	50	1,666
of Mocaio	32	35	33	1,403

These coals contain very little asphalt, but chiefly maltha.

Most coals afford a volatile alkali by distillation:—this seems to me to be rather a product of the operation arising from the union of hydrogen and mephitic air, and thus the alkaline basis of the ammoniacs found on volcanos seems to have been formed. Coals also afford an acid, commonly the marine, or if pyritous, also the vitriolic, more rarely the fuccinous.

According to Mr. Jars, 100 parts of the best English coal give when charred 63 of coals †; but Hielm found the residuum of the best English coals distilled, to amount to 73 per cent. and Dr. Watson found the residuum of Newcastle coal to amount only to 58 per cent. These results necessarily differ according to the degree of heat applied, the duration of the combustion, and the variable admission of air. It is plain the bitumen is never totally expelled, at least not until most of the carbon is consumed; but much more of it is expelled by combustion than by distillation. Watson, p. 27 and 28 ‡.

* Probably 1,07.

† † Jars, 329.

‡ This paper is taken from the Second Volume of Elements of Mineralogy, lately published.

III.

Observations on the best Methods of producing Artificial Cold. By Mr. RICHARD WALKER *.

HAVING already investigated the means of producing artificial cold, and at the conclusion of my last paper (on the Congelation of Quicksilver) dismissed that part of the subject, the best method of making use of those means naturally becomes a desideratum; to that therefore I have lately given my attention, and flatter myself that the following observations may be considered as an useful Appendix to my former papers. The freezing point of quicksilver being now as determined a point on the scale of a thermometer, viz. 39° , as the freezing point of water; and as this metal, exhibited in its solid state, affords an interesting as well as curious phenomenon, I shall apply what I have to say principally to that object.

Frequent occasions having occurred to me of observing the superiority of snow in experiments of this kind to salts, even in their fittest state, that is, fresh crystallised, and reduced to very fine powder, I resolved upon adopting a kind of artificial snow.

The first method which naturally presented itself was by condensing steam into hoar frost. This answered the purpose, as might be expected, exceedingly well; but the difficulty and expence of materials in collecting a sufficient quantity determined me to relinquish this mode for another, by which I can easily and expeditiously procure ice in the fittest form for experiments of this kind; the method I mean is, by first freezing water in a tube, and afterwards grinding it into very fine powder. Thus possessed of the power of making ice, and afterwards reducing it to a kind of snow, the congelation of quicksilver becomes a very easy and certain process; for, by the use of a very simple apparatus, (Plate XX. fig. 1.) quicksilver may be frozen perfectly solid in a few minutes, wherever the temperature of the air does not exceed 85° : thus, one ounce of nitrous acid is to be poured into the tube *b* of the vessel, observing not to wet the side of the tube above with it; a circular piece of writing-paper, of a proper size, is to be placed over the acid, resting upon the shoulder of the tube, and the paper brushed over with some melted white wax. Thus prepared, the vessel is to be inverted, and filled with a mixture of diluted nitrous acid, phosphorated soda, and nitrous ammoniac, in proper proportions for this temperature *, and tied over securely, first with waxed paper, and upon that a wet bladder.

The vessel being then turned upright, and placed in a shallow vessel, viz. a saucer or plate, an ounce and a half of rain or distilled water is to be poured into the tube, which is to be covered with a stopper or cork, and, as soon as frozen solid, ground to very fine powder, an assistant holding it firmly and steadily the while; observing occasionally to work the instrument in different directions up and down, that no lumps may be formed. When the whole of the ice is thus reduced to powder, and the lumps, if any, broken, the frigorific mixture is to be let out quickly, by cutting or untying the string, and removing the bladder, &c. which confines it; a communication made by forcing a rod of glass or wood through the partition; and the whole mixed expeditiously together.

* Philosophical Transactions, 1795.

† I have, by a very accurate preparation of this mixture, sunk a thermometer from 85 degrees (temperature of the vessel and materials) to $+2$ degrees.

In this climate a mixture much less expensive will be sufficient; viz. that composed of diluted nitrous acid, Glauber's salt, sal-ammoniac and nitre; a mixture of this kind sinking a thermometer in the warmest weather to near 0° . At the temperature of 70 degrees, or a little higher, the quantity of diluted nitrous acid may be about one-fourth less than is mentioned in the table for 50 degrees.

These methods are the most expeditious, and attended with the least trouble; but as ice may be used with equal certainty, and with much less expence, I shall give a particular detail of an experiment made with the use of it, first mentioning a preparatory experiment, to which I was immediately led by the recollection that Sir Charles Blagden, in his paper "on the Point of Congelation;" (Phil. Trans. vol. lxxviii.) had found that common sal-ammoniac and common salt mixed with snow produced a cold of -12 degrees, whereas the latter used alone with snow produces only -5 degrees. I used a mixed powder of equal parts of common sal-ammoniac and nitre with the common salt, by which the thermometer sunk to -18 degrees; and when I used nitrous ammoniac with common salt, to -25 degrees. This cold I could not increase by the addition of any other salts, nor could I equal it by any other combination of salts. Those I tried were, Glauber's salt, salt of tartar, soda, and sal catharticus amarus. By several trials, I found the best proportions to be, snow or pounded ice twelve parts, common salt five parts, and of nitrous ammoniac, or a powder of equal parts, sal-ammoniac and nitre mixed, five parts; or one-third of common salt, when I used that alone with snow or pounded ice.

My apparatus then (Dec. 28. last) consisted of two vessels (fig. 3 and 4.); an instrument (fig. 6.) to grind or rather scrape the ice to powder; a kind of spatula (I use a marrow spoon) to stir the powder occasionally; a thermometer (fig. 8.) and a small thermometer glass, with the bulb three-fourths full of quicksilver (fig. 7.) I filled the vessel fig. 3, holding when inverted two pints, *stratum super stratum*, with pounded ice, common salt, and a powder consisting of equal parts sal-ammoniac and nitre mixed together; by first putting in six ounces of pounded ice, then two ounces and a half of common salt, and, after stirring these well together, two ounces and a half of the mixed salts, mixing the whole well together: this was repeated in the same manner until the vessel was quite full. It was then tied over securely with a wet bladder, turned upright, and one ounce and a half of rain water poured into the tube through a funnel, the tube covered with a cork, and the vessel left undisturbed till the water was frozen perfectly solid. The instrument for grinding it was then put in to acquire cold, whilst the vessel fig. 4, holding a pint, was filled in the same manner, with the same proportions of materials; a bladder tied over it, set upright, and one ounce of fuming nitrous acid poured in to be cooled. The ice was then ground to powder; and when finished, the nitrous acid being found to have acquired a sufficient degree of cold, viz. -13 degrees, the frigorific mixture of ice and salts was let out of the vessel which contained the nitrous acid, and the powdered ice (still surrounded by its frigorific mixture) added to the acid as quick as possible; when the thermometer sunk to near -50 degrees, and the mixture soon froze the quicksilver in the glass bulb. In this experiment 18 minutes were required to freeze the water perfectly solid, and 15 to reduce the ice by moderate labour to very fine powder. The experiment was over in 55 minutes, and the temperature of the preparatory cooling mixture then found to be -10 degrees.

I had

I had a spirit thermometer by me; but a mercurial thermometer being much more sensible, and consequently descending much quicker, I prefer it in experiments made merely to freeze quicksilver—knowing from experience how the congelation is going on, from the irregular descent of the mercury when a few degrees below its freezing point, and from having usually found that the quicksilver in the thermometer glass begins to freeze as soon as the mercurial thermometer reaches — 40°.

Whenever I have occasion to use ice in summer for this purpose, I usually pound together first some ice and salt in a stone mortar, about two parts of the former to one of the latter; throw this away, and wipe the pestle and mortar perfectly dry: the mortar being thus cooled, the ice may afterwards be pounded small without melting.

And as a mixture made of snow or ice in powder and salts does not give out its greatest cold till it is become partially liquid by the action of the ice and salts on each other, it is necessary that the whole be stirred well together, till it is become of an uniformly moist pulpy consistence; especially since, in becoming liquid, the mixture shrinks so much, that, if this be not attended to, the vessel will not be near full, and consequently the upper part of the tube not surrounded, as it ought to be, by the frigorific mixture. The dissolution of the ice and salts may, if required, be hastened, by adding occasionally a little water; but then the cold produced will be less intense, and not so durable.

That particular form of the vessel in which the ice is made and reduced to powder is chosen, because it subjects the powdered ice in the tube to the constant action of the freezing mixture, without which it would be less fit, particularly in warm weather, for the intended use; and because in it the ice is not liable to be impregnated with the salts of the mixture, by which it would be utterly spoiled; and that for cooling the nitrous acid, and making the second mixture in, because it is steady, and is besides insulated, as it were, from the external warm air, and surrounded in its stead by an atmosphere much colder.

It is scarcely necessary to add, that when snow which has never thawed can be procured, it may be cooled in this apparatus by a mixture of snow (instead of the pounded ice) and the salts, and the trouble of reducing the ice into powder saved.

I prefer the red fuming nitrous acid, because, as I have observed in a former paper, it requires no dilution. Being under the necessity at one time of using the pale nitrous acid, I found it required to be diluted with one-fifth its weight of water. The best and only way of trying or reducing any acid to the proper strength, is by adding snow, as Mr. Cavendish directs, or the powdered ice to it, until the thermometer ceases to rise; then cool the acid to the same temperature of the snow again; add more snow, which will make the thermometer rise again, though less; cool it again, and repeat this until the addition of snow or powdered ice will not make the thermometer rise: to be very accurate, it should be reduced in this manner to the proper strength, at the temperature, whatever it be, at which the nitrous acid and snow, or powdered ice, are to be mixed together when cooled.

In the course of my experiments I have endeavoured to ascertain the comparative powers of ice to produce cold with nitrous acid in the different forms I have had occasion to use it. The result is, that fresh snow sunk a thermometer to — 32 degrees, ground ice to — 34 degrees, and the most rare frozen vapour to below — 35 degrees; the vessel and materials each time being, + 30 degrees.

The vessels for these mixtures, particularly that in which the quicksilver is to be frozen, should be thin, and made of the best conductors of heat; first, because thin vessels rob the mixture of less cold at mixing, that is, if two mixtures of the same kind are made, one in a thin, the other in a thick vessel, the former will be coldest; secondly, because the air is a sufficiently bad conductor; and thirdly, for the very obvious reason that the cold is transmitted through them quicker.

For these reasons, and from the difficulty I have found in procuring vessels of glass, which are undoubtedly fittest for experiments of this kind, I have used tin, which is readily had in any form, and, if coated with wax, is sufficiently secured against the action of the acids.

I give the inside such a coating by pouring melted white wax into the vessel, previously clean and dry, and turning it about by hand, so as to leave no point of the metal uncovered for the acid to act on, pouring the surplus away.

In the experiments above described, I used a single vessel for cooling the nitrous acid; a cupping-glass (represented by the dotted line at *b* fig. 4.) being cemented into the tin, and thereby forming that part in which the nitrous acid was first cooled, and the mixture afterwards made in which the quicksilver was frozen; but from the trouble and impediments arising from letting out the mixture, and clearing the bottom from the lumps of ice, &c. adhering to it, I was led to the addition of the other part (fig. 5.) by which all these difficulties are got rid of: and it is besides a much more comfortable and neat way of conducting it; the upper part, which contains the nitrous acid, being lifted off, and placed on the table immediately before the powdered ice is added.

The whole of this apparatus may be of tin; that part only (when the cooling mixtures are made without using any corrosive acid) in which the acid mixture is to be made being previously coated in the manner above mentioned; or a thin glass tumbler, of a proper size, may be cemented in.

I have occasionally used a thin glass tumbler for the mixture in which the quicksilver is to be frozen, immersing it, with the acid, in a frigorific mixture, till the acid is sufficiently cooled; then adding the ground ice to it, previously removing the tumbler out of the frigorific mixture, as in the experiment above mentioned: this simplifies the apparatus, but is less convenient on many accounts.

The scale of this apparatus may be diminished or increased at the will of the operator; for there is no doubt that a small quantity of quicksilver may be frozen at any time with one-fourth of this quantity, with an apparatus of this kind, by any one conversant in such experiments.

I have frequently frozen quicksilver by mixing together at 0° , three drams of ground ice with two drams of nitrous acid.

Whenever the intention is, as in these experiments, to cool the materials to nearly the same temperature with the frigorific mixture in which they are immersed, the proportion of the frigorific mixture to the intended mixture (or materials to be cooled) should not be less than twelve to one; a greater disproportion is still better.

By attending to the directions particularly mentioned in the experiment made on December 28, a thermometer may be always dispensed with; the proportions of the materials

terials to be cooled being exactly adjusted, and, when they are to be mixed, precisely determined by the time employed in grinding the ice to powder. The proportions of snow or pounded ice and salt or salts may be guessed sufficiently near without weighing, unless in very nice experiments.

Imagining that a recapitulation of the different mixtures described in my former paper for producing artificial cold, brought into one view, might not be unuseful, I have subjoined a table of the salts, their powers of producing cold with the different liquids, and the proportions of each, according to a careful repetition of each; the temperature being 50 degrees.

S A L T S.			Liquor.	Temperature of Cold produced.	
* Sal-ammoniac 5, nitre 5	—	—	Water 16	—	+ 10°
Sal-ammoniac 5, nitre 5, Glauber's salt 8	—	—	— 16	—	+ 4
* Nitrous ammoniac 1	—	—	— 1	—	+ 4
Nitrous ammoniac 1, sal soda 1	—	—	— 1	—	7
Glauber's salt 3	—	—	D. nitr. acid 2	—	3
Glauber's salt 6, sal-ammoniac 4, nitre 2	—	—	— 4	—	10
Glauber's salt 6, nitrous ammoniac 5	—	—	— 4	—	14
Phosphorated soda 9	—	—	— 4	—	12
Phosphorated soda 9, nitrous ammoniac 6	—	—	— 4	—	21
Glauber's salt 8	—	—	Marine acid 5	—	0
Glauber's salt 5	—	—	D. vitr. acid 4	—	+ 3

N. B. I have chosen the temperature of 50 degrees, because the materials may at any time, by immersion in water drawn from a spring, be cooled nearly to that temperature, and the experiment for freezing with any of these mixtures commence there.

At a higher temperature than 50 the quantity of the salts must be increased, and the effect will be proportionably greater: at a lower temperature diminished, when the effect will be proportionably less.

It must be observed, that, to produce the greatest effect by any frigorific mixture, the salts should be fresh crystallized †, not damp, and newly reduced to very fine powder; the vessel in which they are made, very thin, and just large enough to contain the mixture; and the materials mixed intimately together as quickly as possible, the proper proportions at any temperature (those in the table being adjusted for the temperature of 50° only) having been

* The salts from each of these may be recovered by evaporating the mixture to dryness, and used again repeatedly.

N. B. The figures after each salt, and after the liquor, signify the proportion of parts by Troy weight to be used; the trouble of weighing the water may be saved by observing, that a full ounce of it by wine measure, corresponds exactly with one ounce of it by Troy weight: likewise it must be noticed, when more kinds of salts than one are used, to add them to the liquor one after the other in the order they stand in the table, beginning on the left hand, and stirring the mixture well between each addition. D. nitr. acid is red fuming nitrous acid two parts, and rain or distilled water one part by weight, well agitated together and become cool. D. vitr. acid is strong vitriolic acid, and rain or distilled water equal parts by weight, thoroughly mixed (very cautiously) and cooled.

† Soda, phosphorated soda, and Glauber's salt, are best crystallized afresh, because their effect, especially the two last, in the acids, depends upon the quantity of water they contain in a solid state.

previously

previously tried, by adding the powdered salts gradually to the liquid till the thermometer ceased to sink; observing to produce the full effect of one salt before a second is added, and likewise of the second before a third is added. Neither soda, phosphorated soda, nor Glauber's salt, should be mixed with nitrous ammoniac, or the powder composed of sal ammoniac and nitre, unless at a low temperature, that is, below 0; but pounded and kept apart.

In the experiments alluded to in the table, the precaution of fresh-crystallizing the salts was not observed, because I chose to give the ordinary effects only. I therefore then used salts in their common state, taking care, however, to choose such as had not in the least effloresced.

Since it is always useful, and generally absolutely necessary, to know how much room in a vessel the several materials take up separately; and when mixed, it will be right to observe, that snow or ice in powder, at near 0°, occupy in measure nearly two thirds more than their weight; that is, one ounce weight of water will, when in the form of snow, or ice ground to powder, nearly fill a vessel which holds three ounces wine measure; powdered salts, nearly double their weight; strong nitrous acid, about three-fourths its weight; and a mixture made of salts and diluted nitrous acid; measures rather less than two-thirds of the weight of the ingredients. Without a previous knowledge of this, it is impossible to adjust the size of the vessels to the mixtures which are to be made; because, in most nice experiments of this kind, the height to which a vessel will be filled is indispensably necessary to be known beforehand.

The long continuance of the late frost having afforded me opportunities of repeating these experiments in various ways, I shall mention briefly the result of such as appear to me to be material.

I have found, that ice may be ground so fine as to be equal to frozen vapour; and the harder it is frozen, the finer it is ground, but with more labour.

That quicksilver may be frozen by cooling the nitrous acid only, saving the trouble and inconvenience of cooling the snow likewise; either by adding snow at + 32 degrees, to nitrous acid at — 29 degrees; or snow at + 25 degrees, to nitrous acid at — 20 degrees; or snow at + 20 degrees, to nitrous acid at — 12 degrees. Most winters offer an opportunity of doing it in this way; the nitrous acid may be cooled in a mixture of snow and nitrous acid.

That it may likewise be frozen by mixing expeditiously together snow and nitrous acid, when the temperature of each is + 7 degrees.

Or by mixing ground ice and nitrous acid at + 10 degrees.

Hence it follows, that the cold of this climate offers occasionally opportunities of freezing quicksilver, without previously cooling by art the materials to be mixed; for I have once seen the thermometer at + 6 degrees—and others, I believe, have seen it lower.

I expected an opportunity would have offered this winter, but the lowest point I saw my thermometer at this season was only + 10 degrees. At this temperature I mixed nitrous acid (cooled out-of-doors to the temperature of the air) and snow on January 23d last; but the cold produced was not quite sufficient to freeze the quicksilver, although very near it, as indicated by a thermometer. From what I have observed since these latter experiments were made, I think it may be reasonably expected that powdered ice and nitrous acid at + 14 degrees, or snow at + 10 degrees, will succeed, if mixed expeditiously.

Strong

Strong spirit of vitriol, whose specific gravity is 1,348, required to be diluted with half its weight of water, and produced with snow at the temperature of $+ 30$ degrees, about eight degrees less than with nitrous acid, sinking the thermometer to $- 24$ degrees; four parts of the diluted vitriolic acid required at that temperature fix parts of snow.

It perhaps will be remarked, that I have taken no notice before of the vitriolic acid: The reason is, because, the freezing point of quicksilver being 39, it may be frozen tolerably hard by a mixture of nitrous acid with snow or ground ice, though the utmost degree of cold this acid can produce with snow is $- 46^{\circ}$, which degree of cold may be produced by mixing the snow or ground ice and nitrous acid at 0° .

If it be required to make it perfectly solid and hard, a mixture of equal parts of the diluted vitriolic acid and nitrous acid should be used with the powdered ice; but then the materials should not be less than $- 10^{\circ}$ before mixing.

If a still greater cold be required than a mixture of this kind can give, which is about $- 56$ degrees, the diluted vitriolic acid alone should be used with snow or powdered ice, and the temperature at which the materials are to be mixed not less than $- 20$ degrees.

Select, according to the intention, either of the three following mixtures:

First, snow or pounded ice two parts, and common salt one part, which produces a cold of $- 5$ degrees.

Second, snow or pounded ice twelve parts, common salt five parts, and a powder, consisting of equal parts of common sal-ammoniac and nitre mixed, five parts, which produces a cold of $- 18$ degrees.

Third, snow or pounded ice twelve parts, common salt five parts, and nitrous ammoniac in powder five parts, which produces a cold of $- 25$ degrees.

The proportions which I have found to be the best for mixing the snow or powdered ice with the different acids at different temperatures are these: viz. at $+ 30$ degrees, seven of the former to four of the nitrous acid; at $+ 5$ degrees, (with a trifling allowance, if any, for a few degrees above or below) three to two; at $- 12$ degrees, four to three, with the mixed acids, and at $- 20$ degrees, with the diluted vitriolic acid, equal parts.

If it be required to prepare the materials in a frigorific mixture without the use of ice, a mixture of the proper strength may be chosen from the table.

It is immaterial, when the exact proportions of each are known, whether the powdered ice be added to the acid, or the acid poured upon that, provided the powdered ice be kept stirred to prevent lumps forming, and the materials be mixed as quick as possible. But when the proportion is not known, it is better to be provided with more powdered ice than is expected to be wanted, and add it to the acid by degrees, until the greatest effect is produced, as shewn by a thermometer.

The consistence is a pretty sure guide to those accustomed to mixtures of this kind, viz. when fresh additions of snow or ice do not readily dissolve in the acid, though well stirred, and the mixture acquires a thickish flocculent appearance.

Snow or powdered ice that have ever been subjected to a cold less than freezing are spoiled, or rendered much less fit for experiments of this kind.

I prefer the method of adding the powdered ice or snow to the acid in a separate vessel, principally because the size of that vessel may be exactly adjusted to the quantity of mixture it is to contain.

A mixture made of diluted nitrous acid, phosphorated soda, and nitrous ammoniac, (by much the most powerful of any compounded of salts with acids,) prepared with the greatest accuracy, is not quite equal to a mixture of snow and nitrous acid, each mixed at $+ 30$ degrees, although very nearly so.

Though quicksilver may be frozen by salts dissolved in acids, it is necessary that the materials be cooled previously to mixing, much lower than when snow or ground ice are used.

If it be required to mix the powdered salts and acids at a low temperature, the best method is this:—Put first the nitrous ammoniac into the tube of such an apparatus as fig. 1, shaking it down level, gently pressing the upper surface smooth; then the phosphorated soda or Glauber's salt: cover this with a circular piece of writing paper, and pour a little melted white wax upon it, and when cold pour upon this the diluted nitrous acid: immerse this in a frigorific mixture till it is sufficiently cold, as found by dipping the thermometer into the liquor occasionally; force a communication through, and stir the whole thoroughly together, contriving that the upper stratum of salt, that is, the phosphorated soda or Glauber's salt, be mixed with the liquor first, and then the nitrous ammoniac; the powdered salts do not require stirring whilst cooling, like snow, for, however hard they are frozen, they will readily dissolve in the acid: care must be taken that the partition be perfect between the salts and the liquor, and that in this and every instance where the materials are to be cooled they be immersed below the surface of the frigorific mixture. The strength of the red fuming nitrous acid used in these experiments I found to be 1,510, and that of the vitriolic acid 1,848.

I have thought it better, for the sake of brevity, not to use in this, as in my former papers, the new chemical names, especially as the old ones are more generally known.

These experiments were chiefly made in a warm room, not far from the fire-side.

I have now finished my proposed plan respecting the best modes of conducting experiments on cold; in which it will appear, that I have reduced the congelation of quicksilver, in any climate, at any season, to a certain and almost as easy a process as that I originally set out with for the freezing of water, (Phil. Transf. vol. LXXVII.) viz. by previously cooling the materials in one mixture, to produce the effect in a second. It may very likely appear to some, that I have been too minute in a few particulars; yet, as perhaps experiments of this kind, all circumstances considered, are inferior to few in the delicacy required to make them succeed completely, I trust I shall be excused by those who choose to repeat them, particularly such as are not in the habit of making experiments of this kind—especially if it secure them from an unsuccessful attempt, and that perhaps without being able to account for it.

Oxford, March 1, 1795.

It is very well known, that vitriolic ether will produce sufficient cold by evaporation to freeze water. This circumstance is noticed by many; and several different methods have been proposed, particularly one by Mr. Cavallo, with a very ingenious apparatus for the purpose (Phil. Transf. vol. LXXI.); nevertheless, as I am upon the same subject, and the following experiments differ, as well in the effect produced, as in the particular mode of conducting them, from any I have met with, I have ventured to mention them.

June

June 29, 1792, temperature of the air 71 degrees, I sunk a thermometer (the bulb being covered with fine lint tied over it and clipped close round) by dipping it in ether, and fanning it to 26 degrees; then by exposing the thermometer to the brisk thorough air of an open window, to 20 degrees; and again, by using some of the same ether, but which had been purified by agitating it with eight times its weight of water, applied exactly as in the last experiment, the thermometer sunk to 12 degrees. Water tried in the same manner at the same temperature, sunk the thermometer to 56 degrees.

A whirling motion was given the thermometer during each experiment.

The lint was renewed for each experiment, and the bulb required to be dipped into the ether thrice; the first time sufficiently to soak it; after which the thermometer was held at the window till it ceased to sink; then a second quick immersion, and likewise a third, exposing the thermometer in like manner after each immersion.

In this manner, a little water in a small tube may be frozen presently, by good ether not purified, at any time, especially if a small wire be used to scratch or scrape the sides of the tube below the surface of the water.

During the warmest weather of last summer, I frequently froze water in this way.

Explanation of Plate XX.

FIG. 1. is a vessel in one piece, open at the bottom; *a a* the body, holding, when inverted, two pints; *b* the tube, holding five ounces; the lower or smaller part (formed by a contraction or lessening of the tube in diameter, merely for the purpose of leaving a small shoulder for a temporary partition), holding rather less than one-fifth of the whole.

Fig. 2. is a vessel consisting of two parts; *a a* the body, holding two pints; *b* the tube, holding five ounces; which, together with the lid *c*, forms a cover to take off and on the vessel.

N. B. This vessel may, if preferred, be used instead of fig. 1. the parts corresponding with it, except in not being open at bottom, and the continuation of the tube upwards just sufficient to serve for a handle.

Fig. 3. is a vessel in one piece, open at the bottom, holding, when inverted, two pints; *b* the tube, holding four ounces and a half.

Fig. 4. a vessel open at bottom, holding, inverted, one pint.

Fig. 5. a cover to fig. 4. *a a* the body, fitting exactly over, and *b* the cup-part (holding three ounces) fitting exactly within the corresponding parts of fig. 4.

Fig. 6. the instrument for grinding the ice into powder; it works upon a short centre-point, and has the edge bevelled contrary ways on each side the point, so as to follow. The fineness of the powder is regulated by the degree of pressure used. The handle is wood, the rest metal; *a* is a sliding cover fitting on the tube in which the ice is ground, to exclude the external air, and to keep the instrument steady; *b* is the shoulder or guard, to prevent the point of the instrument from touching so as to endanger injuring the bottom of the tube. It should be made, so as to fit without grating the inside of the tube in using.

The tubes of each of the vessels should be somewhat shorter than the vessel, so as not quite to reach the bottom of it.

Fig. 7. a thermometer-glass, with the bulb three-fourths full of quicksilver.

Fig. 8. a thermometer with the lower part of the scale-board turned up with a hinge,

for the convenience of taking the temperature of small quantities, or of mixtures in which mineral acids form a part.

N. B. These vessels are represented as in glass, that being undoubtedly fittest for purposes in which corrosive acids are to be used.

IV.

The Description of a New Portable Electrical Machine. Invented by the Rev. W. PEARSON, of Lincoln.

AN electrical machine sufficiently portable, and at the same time possessing power adequate to all the purposes of medical electricity, has long been a desideratum among those professional gentlemen who attend the infirm, and are persuaded that electricity has a salutative influence in many disorders of the human body. The electrophore and medical bottle, as also the prepared ribbon and apparatus attached thereto, do both possess the former property, but are found to be greatly deficient in the latter. Mr. Colman *, in his ingenious treatise on the means proper to be used for restoring suspended respiration, proposes an electrical machine (such as is both portable, and likewise possesses power sufficient to give a shock of 35 inches of coated surface at about a quarter of an inch striking distance) to constitute one important article of his improved apparatus for the recovery of persons apparently drowned or suffocated; but seems to lament that we have no machine answering such a description. A consideration of such magnitude as the want of an instrument which promises to benefit society in so critical a moment as is that of the *apparent extinction of the vital principle*, has turned the mind of the writer of this article frequently upon the subject, as having some relation to his clerical function: he has in theory projected various constructions of a portable machine, which mature consideration convinced him would be objectionable in practice. For to answer all the purposes requisite for constituting an electrical machine suitable for Mr. Colman's purpose, it appears necessary not only that it have the two desired properties of portability and power, but also that its power be guarded so as not to be annihilated by the moisture when used in the open air; that it be capable of being used without a table or stand in cases of necessity; that it be rendered fit for use upon the shortest notice; that when used it charge the coated surface without considerable loss of time; and lastly, that it be not liable to be easily broken.

The writer, however, flatters himself he has at length hit upon a construction which comprehends all these properties, and which he recommends to the notice of instrument-makers, from a conviction that it is practicable. Indeed he has constructed more than one machine, which answer his most sanguine expectation, and which therefore he will the more circumstantially describe.

Fig. 1. Plate XXI. represents a view of the machine with the eye placed directly over it at a small distance; ABCD is a small mahogany box $9\frac{1}{2}$ by $7\frac{1}{2}$ inches within, and 6 inches

* As it is two years since I perused this treatise, and I have not seen it since, I cannot be certain of the exact words of the title, nor whether the name be Colman or Coleman. W. P.

deep, made of half-inch board : upon the bottom of the box lies a plate of glass, also $9\frac{1}{2}$ by $7\frac{1}{2}$ inches, cemented to the wood at the four corners by common electrical cement ; this plate is coated on both surfaces with tin-foil 7 inches by 5, as represented at Q in figure 2 (except that the slips of tin-foil at H and I are pasted or gummed upon the *under* side only), and is used as a substitute for a jar of 35 inches of coated surface, it being of no consequence what the shape of the glass be, provided it have a proper quantity of coating. M E N is a cylinder of glass of four inches diameter, the body of which is $7\frac{1}{2}$ inches long : at M and N are brads or * boxen caps cemented as in other machines, upon the small cylindrical ends of which the cylinder revolves by means of a simple winch C, which may be taken off or put on at pleasure, either by screwing, or by being inserted upon a square shoulder. The central points of revolution of N and M are at about $2\frac{1}{2}$ inches from the top of the box, the first inserted into a circular hole in one end of the box, and the other *let down* in an open place made down through the other, which has a detached piece of similar wood to fit it, and to keep the cylinder in its place. L is the cushion, and E the silk placed in the usual way ; D is a screw with a milled head, which, by the assistance of a tapped nut, placed fast in the inside of the box at 4 inches from the bottom, presses against the elastic part of the *support* of the cushion which is hid from the eye : this support, which may be of elastic wood, coated above D with tin-foil, or of any elastic metal, is screwed fast to the back of the box near the bottom in the inside. The chain at D is hung on the screw at pleasure, as the wood is found to be in a good or bad conducting state.

F is a piece of light wood turned very smooth in a lathe, and neatly covered with tin-foil, its ends being rounded. This piece, which I shall call the collector, is furnished with about a dozen fixed pins of brads, projecting against the side of the cylinder, as represented in the figure, as near as may be without touching it. The collector is 6 inches long, and somewhat more than an inch in diameter, and is supported by two solid glass pieces, C C, of an inch and three quarters in length, and nearly half an inch in diameter, cemented into the side of the box at $4\frac{1}{2}$ inches from the bottom, and each at an inch from the end of the collector : by this means the collector becomes insulated, and has a chain to be hooked on an eye of wire fixed on any part thereof, so as to fall occasionally upon the upper insulated coating of the plate of glass in the bottom of the box : the chain, however, must not be so long as to extend beyond the coating by any motion of the box.

B is a glass tube † of about one-eighth of an inch bore, and about 3 or $3\frac{1}{2}$ inches long, with a ring of brads or horn, that has a male screw, cemented upon the middle of it, to screw into another ring that has a female screw, and is cemented or otherwise fastened into the

* When caps are made of wood, they must be turned so, as to admit the glass neck of the cylinder into a space or cavity which it shall nearly fit. By this disposition, the glass is cemented to the wood at both surfaces, and the cap itself will be stronger. W. P.

† Mr. Nicholson may have been aware, that glass-tubes of a small bore, however thick, are liable to be broken by a shock passing through them, when unguarded ; and also that a lining of any kind will usually prevent such an effect. Mr. Fell of Ulverston, in Lancashire, an excellent electrician, has tubes placed over the wires of a very large battery, lined with paper or tin-foil, which do not break with a discharge that melts pretty thick brass wire. The tubes are used as a guard against any accidental touch when the battery is charged. W. P.

front side of the box at $4\frac{1}{2}$ inches from the bottom. Through this tube (when coated with varnish or substance of any kind within, to prevent its breaking by a discharge) passes a brass wire as thick as will easily move in it, about $4\frac{1}{2}$ inches long, tapped about an inch at each end and a little smoothed; on each end of this wire is a tapped nut which screw back or forwards to or from the ends of the tube, so as to hold the wire in any situation that may be required with regard to the distance of its ends from those of the tube. The wire has also two brass balls, one of which has its diameter less than that of the tube, that it may move through the circular hole into the box, to prevent its falling on the glass plate, which it might do if screwed off and on within the box.

OO are the directors with handles of glass, or baked wood varnished, each six inches long, besides the balls and wires; the balls are fast upon the wires, and the wires screw into sockets on the ends of the handles.

There is yet one part of the apparatus which it was not necessary to exhibit in the plate, viz. the insulating stool; this is made exactly of a size to cover the box, namely $10\frac{1}{2}$ inches by $8\frac{1}{2}$, and has four feet of glass, or baked wood varnished, almost 6 inches long. Underneath that part of the stool which covers the collector slides a little drawer, fig. 3. by means of the end-pieces P P, which drawer contains the tube, with its appendages, milled nut, chains, handles of the directors, and amalgam, and must be taken from the stool when used. The three compartments of the drawer, by being made to fit the directors and tube, prevent their shaking in carriage. When packed, the four feet of the stool go exactly into each corner, and the edge of the drawer just within the side of the box, by which means the stool is kept in its place as a cover; and lastly the wires of the directors pass through the stool, which is of inch plank, at the middle near the two ends, and, by screwing into the fixed nuts in the edges of the box's ends at K and K, until the balls touch the wood of the stool, fix the whole so firm that a handle, such as is used in a chest of drawers, fixed on the centre of the stool, serves for carrying the whole apparatus by, which has the appearance of a lady's small travelling-box, and weighs eight pounds. After having been thus prolix in the description of the separate parts of this small machine, I come next to say a few words about the use of it.—As it is necessary before using any machine to have it perfectly dry, and as the separate parts require to be successively placed before the fire, this machine will be found to possess an advantage even in the preparation, which greatly conduces to expedition; for after the box is disengaged from the stool, and has the tube screwed into its place (both which may be done in one minute), the box may be held in the hand before the fire, where all its parts will be alike dried at the same time; or it may be placed on the end A or side D at any convenient distance from the fire on the floor, as occasion may require, whilst the amalgamated leather is preparing.

The machine being freed from moisture and dust, and amalgam applied to the cylinder, the shock is administered in the following manner, viz. Let the small chain fall from the collector upon the upper coating of the glass plate, and place the inner ball at the required striking distance, suppose a quarter of an inch, as the case may require, and fix it there by means of the adjusting nuts, and the quantity of the charge will be limited in the same manner as by Lane's electrometer; then connect one chain with the conducting wire which passes through the tube, and the other with a brass ring connected with the slip of the under coating at H (as appears in figure 1, or at I, as is most convenient), and hook the

opposite ends upon the wires of the directors, whereby a shock may be sent through any particular part of the body, as well as if the medical jar were used with Lane's electrometer. It is immaterial whether the machine, in this operation, be held on a table or stand, the operator's knee when sitting, the bed of a patient, or even the ground by the side of a river in case of necessity where the patient is apparently drowned: nay, it may be held, by a person standing, in one arm, whilst the other hand turns the winch.

When the spark only is required, the small chain is removed from the collector, and also the long chain, connected with the hook at H, is taken away; the inner ball is then fixed so as to touch the collector, by means of the adjusting nuts; in which case the collector, the wire in the tube, and the second long chain connected therewith, form together one conductor. After the exterior end of this chain is attached to the wire of one of the directors, the spark may be directed into any particular part of the body; when the eye is the part affected, a pointed wire must be used instead of the wire carrying the ball, which must be made to screw into the socket of the director.

When a spark is taken out of the body, the patient must stand on the stool, and hold the chain, connected as before, in his hand; and then the operator, or assistant, may take sparks from him, as in other machines.

If the patient be nervous, or afraid of the spark, it would be safer to hook the chain to the collector for fear of breaking the tube, and hold it at a distance from the edge of the box in a perpendicular direction. The sparks taken from an insulated body are stronger than those sent into any person by reason of the smallness of the conductor, which is perhaps the greatest defect of the machine: this defect may, however, be remedied by placing any rounded metallic substance, such as a large candlestick, upon the stool near the machine, and connecting the chain with it, so as to make it a part of the conductor, and then the sparks will become more dense.

There is yet another way of administering the electric fluid, which cannot properly be called administering either the spark or the shock; in which case the sensation is not so momentary as is that caused by the shock, but more pungent than that of the simple spark; it may be called the interrupted shock, and is thus effected: Let the short chain fall from the collector upon the coating of the plate, and connect one of the long ones with the hook at H or I, and suffer it to be extended on the table; then, the inner ball touching the collector, turn half a dozen rotations before the finger be presented to the outer ball; and because the circle is not completed between the two coated surfaces of the plate by perfect conductors, a continuous stream of dense sparks will issue into the finger, causing a more pungent sensation than would be felt from a simple spark of a much more powerful machine. I should suppose the stimulant power of this mode of electrifying any particular limb to be very great, and I understand it has been practised by some electricians, by means of a charged jar where the circle is not completed by perfect conductors.

If a small pocket-jar, in a case, be added to this machine, it will be capable of producing either a negative or positive charge in it; for if the jar held by the ball connected with its inner coating be presented so as to receive a positive charge on its outer coating from the outer ball of the machine, the inner coating will of course be negatively charged; and if it be placed upon the stool, or inverted goblet, till the hand has taken hold of the outside, the negative charge will be retained.

Thus

Thus this little machine is capable of affording much amusement, and even of trying experiments with, besides its being adapted for medical purposes; and, which is not the smallest recommendation, may be manufactured at a small expence, compared with the larger machines. But, small as it is, I have frequently fired ardent spirits with it, and have seen its power such as to cause 35 inches of coating to discharge at a quarter of an inch striking distance at every third revolution; and at fourteen or fifteen revolutions it will, when in good order, discharge at half an inch. A second machine of this construction, which I lately made for a friend, contains 42 inches of coated surface, with a cylinder of the same size as the one already described, and is better adapted for making experiments, the box being larger, and the insulation more perfect; but is not so portable.

Before the former machine was completed, I apprehended that the proximity of the wood to the collector would take off much of the fluid; but it does not appear that this is the case at all in giving shocks, nor even in taking sparks, unless the distance of the body to be electrified from the ball used be greater than the distance from the collector to the wood, or rather from the adjusting screws to the wood, for here the tendency to escape seems to be the greatest. On this account the parts of the box where the fluid has the greatest tendency to escape, are varnished with several coats of sealing-wax melted in spirits of wine. How far baked wood would be preferable to mahogany in its working state for the front and ends of the box, I have not attempted to ascertain.

As this machine is calculated to keep out the humidity of the air, when the hole for the tube is corked, and all the glass, or insulating parts, well varnished with proper electrical varnish; the exciting power of the cylinder, when packed in good order, will remain but little impaired for several hours, and sometimes days if kept in a dry room, without fresh amalgam. And what may appear rather remarkable, as the stool is near the cylinder when packed, if the chains and tube be first properly fixed, and the drawer removed, a shock may be administered with the stool screwed on, as well as when off, and to all appearance in as powerful a manner: hence the stool may be used as a preservative against humidity in the open air, in cases of necessity.

After what has been said of the construction and uses of this little machine, it may be asked: "Has its efficacy ever been brought to the test?" I answer, Yes. A gentleman, who is a near relative of the writer of this article, was seized with a paralytic affection, during breakfast, on September 15, 1797, which totally deprived him of the use of any part of his left side, but which was not attended with pain. The best medical assistance was resorted to for nearly a fortnight, without producing any apparent alteration. The appetite and tendency to sleep remained nearly as usual; but the distortion of one side of the mouth rendered the speech so inarticulate as to be nearly unintelligible. On the 27th of the same month I was permitted to administer partial shocks through different parts of the affected side, which I began at the striking distance of one-eighth of an inch with the machine already described, while the patient was extended on a sofa; the first two or three shocks sent through the hand, were scarcely if at all felt; by and by a sensation was perceived; a few larger shocks at one-sixth were then administered, which convulsed the hand a little, and were sensibly felt; a great many more, at the same striking distance, were then sent through most of the joints of the diseased side; lastly, a few more, at the distance of one-fourth

one-fourth of an inch, were directed from the shoulder to the hand, and from the knee to the foot. After being thus electrified, the patient felt some pain in the knee, where the largest shocks entered, and upon trial could raise the leg a little from the ground; but the hand and arm were motionless. On the morning of the 28th, the patient, after a good sleep, could raise the leg as before; and, to my great satisfaction, such an alteration had taken place, that by a motion proceeding apparently from the shoulder, the arm was carried back and forward, as if suspended by a pin; but the hand appeared no more than a dead weight. Partial shocks of one-fourth of an inch were administered as on the preceding evening, after which a pain similar to what had been before felt in the knee was now felt in the shoulder; the knee became somewhat stronger, and the arm more active in its motion, but the hand remained apparently dead. In the evening of the 28th, the patient was in bed, and electrified as before, after which the thumb and first finger could be moved in a small degree. This rapid advance on the first application of electricity, could not, I think, be attributed to the effects of medical assistance, which had been omitted to be used a few days before, except that a seton remained (which still remains); nor yet to what is called the "*vis medicatrix naturæ*," for this usually operates slowly. Shocks have continued to be used sometimes from one extremity to the other, in general twice a day, ever since, to the present time, January 13, 1798, whilst the striking distance has been gradually enlarged to nearly 4-10ths of an inch, and the increase of strength and activity has been hitherto regular. The patient at this time can walk into the street, or up and down stairs, without the assistance of a stick, and can handle any thing of small weight in his weaker hand: in short, he is happily become capable of superintending the affairs of his family, and, I trust, has reason to hope for still greater strength by persevering in the means hitherto used. Other instances could be adduced in which this machine has been used with as good effect as a larger one in diseases of the human body; but a particular detail would enlarge the bounds of this article, which perhaps will be deemed already sufficiently long.

Lincoln, January 13, 1798.

V.

A Memoir concerning a remarkable Phenomenon in Meteorology. Read to the Society of Naturalists of Geneva. By M. DE SAUSSURE, October 1797.*

AS soon as I had rendered my hygrometer sufficiently perfect to compare one instrument with another, and at the same time so delicate as to immediately shew the changes in the atmosphere, I was in hope that I might avail myself of it to foretell the changes of the weather. I expected that the hygrometer would move towards *dry* on the approaching fine weather, or towards *moist* when wet weather was at hand; and it is true that, in general, the north-east wind, which usually with us accompanies fine weather, does cause it to move towards *dry*; and, on the other hand, that it indicates humidity during the seasons of rain. But I have since observed a very remarkable exception; namely, that the times of greatest dryness are generally the precursors of rain.

* This paper was communicated to the Editor of *La Décade Philosophique*, &c. by Felix D. Sports, Resident of the French Republic at Geneva. I have translated it from this last work, No. 4. N.

Ever since I made this observation, my thoughts have been frequently directed to account for it; and it was not till lately at Plombieres that I found a satisfactory cause, which I shall make the subject of the present Memoir.

In order to give every possible degree of certainty to my observations, I kept my instruments defended not only from the direct rays of the sun, but from every reflection; and I observed their station daily at the same hours, but more particularly at four in the afternoon, when the greatest dryness usually prevails.

During the two months that I remained at Plombieres, the greatest dryness I observed was on the 2d of August. The hygrometer indicated $68,5^{\circ}$, the thermometer being at $22,5^{\circ}$. Three or four days before, the hygrometer at the same hour had stood higher or nearer to humid, that is to say, 86° , or 87° , though the thermometer was near a degree higher, namely $23,1^{\circ}$, which must have proportionally raised the hygrometer. It rained in the evening of the same day on which it had been driest, namely, on the 2d of August. At the same time that the hygrometer descended, the barometer also fell near two lines. Now I have proved, in my Essay on Hygrometry, and by several experiments, that in an air which is rarefied the hygrometer descends, and denotes a greater degree of dryness. I therefore attributed this extraordinary dryness to the rarefaction of the air.

On my return from Plombieres, I pursued my observations with the same care. On the 29th of August of this present year, at 20 minutes after 4 in the evening, I observed the hygrometer at 74° , while the thermometer stood at $22,5^{\circ}$. On the following day, at 50 minutes after one, I found the thermometer exactly at the same degree as the evening before, namely, $22,5^{\circ}$, the hygrometer being at $59,5^{\circ}$, that is to say, $14,5^{\circ}$ higher than the evening before. I noted this moment, as affording a valuable observation, on account of the identity of height in the thermometer. It was therefore evident that the dryness of the air had been increased, not by its heat, but by some other agent, such as its rarity. In fact, the barometer had descended more than half a line. A wind from the south-west prevailed at the same time, and it rained early the next morning.

But the most striking instance is to be found in my Essay on Hygrometry. I made this observation at Chamouni on the 23d of July 1781. The hygrometer stood at $41,2^{\circ}$, the thermometer indicating at the same time $20,2^{\circ}$. But this degree of heat was far from sufficient to produce such a degree of dryness; for, by calculating from the table at page 87 of my Essay on Hygrometry, we find that the difference of $4\frac{1}{2}$ degrees of heat between that day and the preceding could not depress the hygrometer more than 9 degrees, instead of 20, which it really descended. The excess, namely 11 degrees, must therefore be attributed to another cause, which I conclude to be the rarefaction of the air. The air may be rarefied either by the fall of the barometer, or the direction of the wind. In fact, the south and south-west winds coming from lower countries than ours, and from the sea, must necessarily rise, and thence become rarefied, and consequently, as I have said, cause the hygrometer to advance towards dry. It is probable likewise that the singular elevation of the valley of Chamouni above the level of the sea, was one of the principal causes of the dryness which predominated in that valley on the 26th of July 1781.

We may therefore conceive the reason of this phenomenon, which at first sight appears so strange; namely, that extraordinary dryness should precede rain, and that the hygrometer should become, in this manner, an assistant to the barometer, and afford one of the most cer-

tain

tain indications of change of weather. It is to be observed, that it rained at Chamouni the next morning.

Nature has exhibited signs of dryness which do not deceive the inhabitants of the country, and serve to predict storms long before hand; such as the flaccidity or drooping of plants with large and thin leaves; such as the gourd and the beet in our gardens, the peatstake in the fields, and the cacalia in the mountains. It is observed, that the leaves of these plants droop and incline towards the ground on the approach of stormy weather; and that on the contrary they spring up, and assume an appearance of vigour, when the dew or rain has restored the elasticity and natural freshness to their fibres. I must add, that the heavy rain in the month of September last was preceded by a wind of extraordinary dryness: and as we are desirous of knowing the purpose, or at least the use, of each of the laws of nature, I would remark, that these great droughts which precede storms seem intended to put vegetables into a proper state to obtain the greatest advantage from the rains, on which their growth is to depend. A dry air relaxes and empties their vessels, and gives them the power of absorbing the rain-water which succeeds. This water finds the air through which it falls loaded with carbonic acid and other exhalations which give fertility to plants.

It is in fact observed, that the rain of storms which succeed uncommonly dry weather gives to vegetables a peculiar growth and strength, much greater than are found to succeed other rains or long continued wet weather.

Hence we perceive, that the more attention we pay to natural phenomena, the more reason we find to admire the order and uniformity which prevail in the laws to which they are subject.

VI.

On the various Denominations given to the Alkali of Tartar. By a Correspondent.

To Mr. NICHOLSON, Editor of "The Journal of Natural Philosophy, &c."

SIR,

AMONGST the numerous innovations which have taken place within these few years, nothing perhaps has undergone such an infinite variety of modifications as the chemical nomenclature: so far indeed has the rage for novelty been carried in this respect, that every person who, without a demonstrable necessity for the change, proposes at present either a new system, or an alteration in any one already received, must justly incur the censure of increasing the confusion which such an endless mutation of names has already produced. I shall however beg leave to mention a circumstance which has frequently appeared to me to be a remarkable one. We have no tolerable appellation universally received in our own language for a substance which is indisputably in more ordinary use in the laboratory, and more frequently spoken of in chemical writings, than any other—the pure alkali of tartar. Let us only see what are the appellations which have been given to it.

Kali, the name applied to this salt by the London College of Physicians, is not only improper, as being already that of a genus of plants, but has in fact been long used as the dis-

tinctive appellation of the other fixed alkali, in consequence of its extraction from those vegetables.

Fixed vegetable alkaline salt, the designation of this substance in the Edinburgh Pharmacopœia, to say nothing of its being rather a description than a name, is no less objectionable on account of its impropriety. This alkali does not seem to be any more a product of vegetation than soda.

Potasse, the barbarous corruption of our word *pot-ash* by the French chemists, has with them at least one advantage; it does not signify any thing else: but when re-translated by us into *pot-ash*, it certainly becomes in the highest degree objectionable, as tending to confound a simple substance in a state of chemical purity, with a heterogeneous compound of very different qualities.

Sodium, the appellation proposed by Dr. Hopson, is also inappropriate. This salt is certainly not formed by the process of incineration any more than the other fixed alkali is: indeed barilla, from which the greater part of the soda used in this country is obtained, is as immediately in the state of a cinder as any substance in commerce.

Tartarin, or, better perhaps, *tartarine*, is a word, which, if not every thing we could wish, has several great advantages. 1st. It signifies nothing else. 2dly. It is perfectly appropriate. Tartar is a substance which has been long and universally known, and which contains this alkali only in combination with an acid which is destructible by fire; so that the purest specimen of its carbonate, which is commonly met with in the shops, is obtained from it by mere combustion; on which account it has been long called by several compound names analogous to the one here spoken of, as *salt of tartar*, *alkali of tartar*, &c. 3dly. The name itself recalls to the mind one of the readiest and most ordinary means of distinguishing the solution of this salt from that of soda by the test of the acid of tartar. And 4thly. It has already been used throughout the whole of a work of some importance, which is in the hands of every chemist;—a circumstance perhaps essential to the establishment of any new chemical appellation. As the Latin, on account of its saving the necessity of auxiliary particles, and for some other reasons, seems better adapted to some of the purposes of a chemical nomenclature than the modern languages, the feminine *tartarina* may serve well enough in this case. The word proposed in the French nomenclature will not by this means become absolutely useless:—the classical chemist may still label his common pot-ash "*Potassa Russica*," and its titles, if not of equal antiquity, will not perhaps be inferior in elegance to those of its next-door-neighbour, the "*Cineres Perlati*."

But as it is rather my intention to point out the difficulty than to propose the remedy, I shall add no more on the subject. The wish of every one must be, that by the universal reception of some unobjectionable appellation, we may be enabled to speak of this substance without impropriety, or the appearance of affectation.

I am, Sir, your obedient servant;

PHILONOMUS.

January 18, 1798.

VII.

An Account of some Experiments to determine the Force of fired Gunpowder. By BENJAMIN Count of RUMFORD, F.R.S. M.R.I.A.

[Concluded from page 468.]

AFTER tabulating the experiments, of which an abstract was given in our last number, the Count proceeds to ascertain the law, according to which the elasticities increase in proportion to the quantities of powder used for the charge. For the sake of brevity, as well as because the subject appears to require still further investigation than he has bestowed upon it, I shall refer the reader to the paper itself, instead of attempting to give an abridged account of this part, and shall proceed to relate the other experimental results contained in his paper.

After having shewn the extreme force of fired gunpowder, he adverts to an objection which may be made against his deductions. How does it happen that fire-arms and artillery of all kinds, which certainly are not calculated to withstand so enormous a force, are not always burst when they are used? Instead of answering this question, by asking how it happened that the extremely strong barrel used in his experiment could be burst by the force of gunpowder, if this force be not in fact much greater than it has ever been supposed to be, he proceeds to shew that the combustion of gunpowder, instead of being instantaneous, as Mr. Robins's theory supposes, is much less rapid than has hitherto been apprehended; an observation, which, if established, is certainly sufficient to answer the objection.

He remarks, that it is a well known fact, that on the discharge of fire-arms of all kinds, cannon and mortars as well as muskets, there is always a considerable quantity of unconsumed grains of gunpowder blown out of them; and what is very remarkable, as it leads directly to a discovery of the cause of this effect, these unconsumed grains are not merely blown out of the muzzles of fire-arms, but come out also by their vents or touch-holes, where the fire enters to inflame the charge, as many persons who have had the misfortune to stand with their faces near the touch-hole of a musket, when it has been discharged, have found to their cost.

It appears extremely improbable to our author, if not absolutely impossible, that a grain of gunpowder actually in the chamber of the piece, and completely surrounded by flame, should, by the action of that very flame, be blown out of it without being at the same time set on fire. And, if this be true, he considers it as a most decisive proof not only that the combustion of gunpowder is less rapid than it has generally been thought to be, but that a grain of gunpowder actually on fire, and burning with the utmost violence over the whole of its surface, may be projected with such a velocity into a cold atmosphere, as to extinguish the fire, and suffer the remains of the grain to fall to the ground unchanged, and as inflammable as before.

This extraordinary fact was ascertained beyond all possibility of doubt by the Count's experiments. Having procured from a powder-mill in the neighbourhood of the city of Munich a quantity of gunpowder, all of the same mass, but formed into grains of very different sizes, some as small as the grains of the finest Battle powder, he placed a number of vertical

creens of very thin paper, one behind another, at the distance of 12 inches from each other; and loading a common musket repeatedly with this powder, sometimes without and sometimes with a wad, he fired it against the foremost screen, and observed the quantity and effects of the unconsumed grains of powder which impinged against it.

The screens were so contrived, by means of double frames united by hinges, that the paper could be changed with very little trouble, and it was actually changed after every experiment.

The distance from the muzzle of the gun to the first screen was not always the same; in some of the experiments it was only 8 feet, in others it was 10, and in some 12 feet.

The charge of powder was varied in a great number of different ways; but the most interesting experiments were made with one single large grain of powder, propelled by smaller and larger charges of very fine grained powder.

These large grains never failed to reach the screen; and though they sometimes appeared to have been broken into several pieces by the force of the explosion, yet they frequently reached the screen entire; and sometimes passed through all the screens (five in number) without being broken.

When they were propelled by large charges, and consequently with great velocity, they were seldom on fire when they arrived at the first screen, which was evident not only from their not setting fire to the paper (which they sometimes did), but also from their being found sticking in a soft board, against which they struck, after having passed through all the five screens; or leaving visible marks of their having been impinged against it, and being broken to pieces and dispersed by the blow. These pieces were often found lying on the ground; and from their forms and dimensions, as well as from other appearances, it was often quite evident that the little globe of powder had been on fire, and that its diameter had been diminished by the combustion before the fire was put out, on the globe being projected into the cold atmosphere. The holes made in the screen by the little globe in its passage through them, seemed also to indicate that its diameter had been diminished.

That these globes or large grains of powder were always set on fire by the combustion of the charge, can hardly be doubted. This certainly happened in many of the experiments; for they arrived at the screens on fire, and set fire to the paper: and in the experiments in which they were projected with small velocities, they were often seen to pass through the air on fire; and when this was the case, no vestige was to be found.

They sometimes passed on fire through several of the foremost screens without setting them on fire, and set fire to one or more of the hindmost, and then went on and impinged against the board, which was placed at the distance of 12 inches behind the last screen.

The Count then proceeds to mention another experiment, in which the progressive combustion of gunpowder was shewn in a manner still more striking and not less conclusive.

A small piece of red-hot iron being dropped down into the chamber of a common horse-pistol, and the pistol being elevated to an angle of about 45 degrees, upon dropping down into its barrel, one of the small globes of powder (of the size of a pea), it took fire, and was projected into the atmosphere by the elastic fluid generated in its own combustion, leaving a very beautiful train of light behind it, and disappearing all at once like a falling star.

This amusing experiment was repeated very often, and with globes of different sizes.

When

When very small ones were used singly, they were commonly consumed entirely before they came out of the barrel of the pistol; but when several of them were used together, some, if not all of them, were commonly projected into the atmosphere on fire.

As the slowness of the combustion of gunpowder is undoubtedly the cause which has prevented its enormous and almost incredible force from being discovered, our author deduces, as an evident consequence, that the readiest way to increase its effects, is to contrive matters so as to accelerate its inflammation and combustion. This may be done in various ways; but, in his opinion, the most simple and most effectual manner of doing it would be to set fire to the charge of powder, by shooting (through a small opening) the flame of a smaller charge into the midst of it.

He contrived an instrument on this principle for firing cannon three or four years ago, and it was found, on repeated trials, to be useful, convenient in practice, and not liable to accidents. It likewise supercedes the necessity of using priming, of vent-tubes, port-fires, and matches; and on that account he imagined it might be of use in the British navy. Whether it has been found to be so or not he has not yet heard.

Another infallible method of increasing very considerably the effect of gunpowder in firearms of all sorts and dimensions, would be to cause the bullet to fit the bore exactly, or without windage, in that part of the bore at least where the bullet rests on the charge; for, when the bullet does not completely close the opening of the chamber, not only much of the elastic fluid, generated in the first moment of the combustion of the charge, escapes by the side of the bullet; but, what is of still greater importance, a considerable part of the unconsumed powder is blown out of the chamber along with it in a state of actual combustion, and, getting before the bullet, continues to burn on as it passes through the whole length of the bore; by which the motion of the bullet is much impeded.

The loss of force which arises from this cause, is in some cases almost incredible; and it is by no means difficult to contrive matters so as to render it very apparent, and also to prevent it.

If a common horse-pistol be fired with a loose ball, and so small a charge of powder that the ball shall not be able to penetrate a deal board so deep as to stick in it when fired against it from the distance of six feet; the same ball, discharged from the same pistol with the same charge of powder, may be made to pass quite through one deal board, and bury itself in a second placed behind it, merely by preventing the loss of force which arises from what is called windage, as he found more than once by actual experiment.

He has in his possession a musket, from which, with a common musket charge of powder, he fires two bullets at once with the same velocity that a single bullet is discharged from a musket on the common construction with the same quantity of powder. And, what renders the experiment still more striking, the diameter of the bore of his musket is exactly the same as that of a common musket, except only in that part of it where it joins the chamber, in which part it is just so much contracted, that the bullet, which is next to the powder, may stick fast in it. He adds, that though the bullets are of the common size, and are consequently considerably less in diameter than the bore, means are used which effectually prevent the loss of force by windage; and to this last circumstance, he concludes, it is doubtless owing, in a great measure, that the charge appears to exert so great a force in propelling the bullets.

That the conical form of the lower part of the bore where it unites with the chamber has a considerable share in producing this extraordinary effect, is, however, very certain, as he has found by experiments made with a view merely to ascertain that fact.

The remaining pages of the Count's paper are occupied by a computation, tending to shew, that the force of the elastic fluid, generated in the combustion of gunpowder, may be satisfactorily accounted for upon the supposition that its force depends solely on the elasticity of watery vapour or steam. For this purpose he recurs to the experiments of Mr. Betancour, published in Paris, under the auspices of the Royal Academy of Sciences, in the year 1790, which shew that the elasticity of steam is doubled by every addition of temperature equal to 30° of Fahrenheit. From the Count's reference, it appears that the experiments were carried as far as 280 degrees of that scale, in which case the pressure was found to be equal to about four atmospheres. He affirms, that there does not appear to be any reason why the same law should not hold in higher temperatures, and has therefore extended his computations through thirteen more terms of the geometrical series, the last of which affords an elastic force equal to more than sixty-five thousand atmospheres. As an excuse for not giving this computation in detail, I must simply remark, that something more than a negative reason seems necessary to justify the extension of this law of increase from so limited a scale of experiments. For which reason I shall add no more of this speculative part, than merely that the water of crystallization in the nitre, and the moisture which the charcoal may be conceived to retain, appear to be fully sufficient to account for the explosive force by means of steam only, if the deductions from Mr. Betancour's experiments be admitted.

VIII.

*Observations on Strontian. By Citizen PELLETIER. Read to the National Institute 30th April, 1796.**

STRONTIAN is at present considered by many foreign chemists as a peculiar earth. Its discovery seems to me to be due to Dr. Hope, professor of chemistry at Glasgow; he having first described its characters and chemical properties in a dissertation which he published 4th November, 1793, and which has since been printed in the Transactions of the Royal Society of Edinburgh. This memoir is nevertheless posterior to Dr. Crawford's dissertation on the internal use of muriate of barytes, in which he announces that he thinks it probable that the strontian mineral contains an earth different from barytes †.

M. Klaproth

* Translated from the *Annales de Chimie*, xxi. p. 123; by J. E.—r.

† The salt obtained from the combination of strontian earth with muriatic acid is much more soluble in hot water than in cold, and consequently easily crystallizes by cooling: muriate of barytes, on the contrary, is nearly as soluble in cold water as in hot, and crystallizes by evaporation.

An ounce of distilled water at the temperature of 70 degrees, dissolves 9 drachms and 50 grains of muriate of strontian: the same quantity of water, at the same temperature, only dissolves 3 drachms and 35 grains of muriate of barytes.

The former produces at least 15 degrees of cold by its solution; the latter not more than 5 degrees at the most.

M. Klaproth has also examined strontian, but, as it appears, subsequently to Dr. Hope ; or at least, as he does not speak in his work of Dr. Hope's experiments, there is reason to believe that they were not known to him.

Strontian is also mentioned in M. Schmeisser's mineralogical work, as different from the other known earths.

It is in the state of carbonate that it is found in Argyleshire in the western part of the north of Scotland, accompanying a vein of lead-ore:

Klaproth, Blumenbach, and Sulzer of Ronnebourg, called it *strontianite*; Hope called it *frontite*. I conceive that the name of *strontian*, taken from the place where it is found, may be properly assigned to it, as this word in itself has no signification, and cannot consequently render it liable to be confounded with other substances.

The carbonate of strontian has been for a long time considered as a variety of native carbonate of barytes ; I myself looked upon it as such in 1791, from some assays to which I had subjected a small specimen, which Mr. Greville of London had the goodness to procure me. Having assayed it comparatively with the carbonate of barytes from Anglezark, which was then called witherite, I thought I did not at that time perceive any remarkable difference between the two substances. Both of them when fused by the blow-pipe afforded white opaque vitreous globules, which, when exposed for some time to the air, became reduced to powder. Exposed to distillation in a moderate heat, carbonate of strontian does not, any more than carbonate of barytes, emit any carbonic acid gas, though they are both dissolved by the nitric and muriatic acids with effervescence and a disengagement of this gas. The salts which resulted from these combinations were by no means deliquescent, and I therefore took those of the strontian for nitrate and muriate of barytes, and still rather because their solutions were decomposed by the alkaline, calcareous, and other sulphates, as is the case with the barytic salts.

It is some months since we have been acquainted in France with the labours of M. Klaproth on the carbonate of strontian ; but those of Dr. Hope, although prior, were unknown to us, and it is only within a few days that I have been apprized of them by M. Schmeisser.

In a letter which M. Hermbstede wrote me six months ago, he announced that M. Klaproth had established the properties of strontian as a new earth, the discovery of which had been made several years since by M. Sulzer, and had been published by M. Blumenbach in his treatise on natural history. The distinctive characters which M. Klaproth had found in the carbonate of strontian were :

First, That it was specifically lighter than native carbonate of barytes (witherite) ;

Secondly,

Muriate of barytes affords by evaporation flattened octagonal crystals, two of whose opposite sides are always much longer than the others : muriate of strontian, by rapidly cooling, crystallizes in elongated filaments, and, when slowly cooled, in hexagonal columns, of which three sides are alternately wider, and the others narrower, and terminated in obtuse triangular pyramids. Thus, though the carbonate of strontian much resembles that of barytes, these two substances have very different qualities. It is probable that the strontian mineral has for its basis a new species of earth which has not hitherto been examined, and which it is of importance not to substitute for barytes for medical uses. *Extract from Dr. Crawford's Dissertation.* P.

The above is a translation of Citizen Pelletier's note. Not having Dr. Crawford's tract on the *terra ponderosa salita*, or muriate of barytes, at present by me, I have not been able to examine the accuracy of the quotation, but have no reason to doubt it, except that I think he uses a different nomenclature. T.

Secondly, That it produced with the nitric, muriatic, and other acids, salts which were more soluble than those of barytes;

Thirdly, that the salt which it formed with the muriatic acid, being dissolved in alcohol, gave it the property of burning with a red flame;

Fourthly and lastly, That it might be deprived of its carbonic acid by calcination; and that it became by this means soluble in water, and the most so in boiling water; so that by cooling a portion was separated in a crystalline form.

The specimen of carbonate of strontian, which I had in my collection, was not considerable enough to admit of a great number of experiments; it was however sufficiently so to enable me to repeat a part of those which are related by Messrs. Hope and Klaproth; and what mostly determined me to appropriate it to this purpose was, that Cit. Coquebert had published in No. 5 of the *Journal des Mines*, that from some assays which we had made of the carbonate of strontian, I had doubted whether it contained a peculiar earth. The following are the reasons on which my doubts were founded;

1. It is several years since I had succeeded in disengaging carbonic acid gas from carbonate of barytes by calcination; and, having then dissolved the calcined barytes in hot water, had obtained a crystallization: I could not therefore consider this character as exclusively belonging to strontian.

2. I knew also that calcareous muriate dissolved in alcohol gave it the property of burning with a red flame. This consideration induced me therefore to suspect a mixture of calcareous carbonate in that of strontian, and the specimen which I had was in fact combined with this substance. I shall now describe the experiments which I have made to endeavour to detect this ingredient; and, as they were made comparatively with a similar set on the native carbonate of barytes (witherite), I have thought it proper to present them together, that it may be seen in what respects the carbonate of strontian resembles, and in what it differs from it.

Comparison of Strontian and Barytes.

1. CARBONATE of barytes is found with sulphate of barytes in a lead-mine at Anglezark, (near Chorley) in Lancashire*. Carbonate of strontian is found at Strontian in Argyleshire, also accompanying a lead-ore with sulphate of barytes.

2. The carbonate of barytes from Anglezark, taken internally, is poisonous, so that in that country it is known by the name of rat-stone [*pierre contre les rats*]. A little dog to which I had given fifteen grains was seized with vomiting, and died eight hours afterwards; and having given to another, of apparently equal strength fifteen grains of carbonate of barytes obtained from the decomposition of the sulphate, it was also seized with vomiting, and died fifteen hours afterwards: the latter was opened by Cit. Chaussier. Another dog to which I had given a like quantity of carbonate of barytes, prepared from sulphate of barytes from the ci-devant province of Auvergne, had vomitings, but did not die, though he took it two days successively: he vomited each time. I purpose to repeat these experiments with carbonates of barytes procured from different barytic sulphates, and especially those which are not accompanied by any metallic ores. The carbonate of strontian, on the contrary, may be taken internally without danger. I gave 20 grains of it to a little dog, but he was

* See the *Manchester Memoirs*, iii. 598. T.

not seized with any vomiting, and twenty hours afterwards I did not at all perceive that he had felt the smallest inconvenience. It will be proper to repeat this experiment with stronger doses. Blumenbach was also convinced that the carbonate of strontian, taken internally, did not at all derange the animal economy. These observations, therefore, point out a difference between strontian and barytes.

3. The colour of the carbonate of barytes from Anglezark is a grey white: it is sometimes found crystallized, but more commonly in a striated mass. Its specific gravity is from 4.2919 to 4.3710. The colour of carbonate of strontian is a light green, though it is sometimes found colourless and transparent: it is striated, and sometimes of a regular crystalline form. Its specific gravity is from 3.6583 to 3.6750*. This carbonate is consequently lighter than that of barytes.

4. Native carbonate of barytes, exposed to a fire which is not too violent, scarcely loses any thing of its weight: in a stronger one it attacks the crucible, and passes into fusion. Carbonate of strontian also retains the carbonic acid pretty strongly; but with caution, and a proper continuance of heat, the carbonic acid may be separated in the proportion of five or six hundredths of the salt, without its attacking the crucible: some care must, however, be taken not to have the fire too strong, as the earth would, in that case, attack the crucible, and run into a chrysolite-coloured glass. The carbonic acid therefore is less strongly retained in carbonate of strontian than in that of barytes.

5. Messrs. Hope and Klaproth had observed, that calcined strontian was soluble in water, and that, when boiling, it dissolved a sufficient quantity to afford crystals by cooling; so that these two chemists regarded this property in strontian as a distinctive character. M. Klaproth especially never succeeded in calcining the native carbonic of barytes sufficiently to try its solubility in that state. When he gave it but a small degree of heat, it was not deprived of its carbonic acid; and when he applied a greater, it became vitrified. Dr. Hope announced, in a supplement to his memoir, that he had found means to calcine the native carbonate of barytes in a black-lead crucible, and that he found this earth, thus calcined, was, as well as strontian, soluble in boiling water, and susceptible of crystallization; which properties he has accordingly ceased to consider, since that time, as characters peculiar to strontian. I have also succeeded in separating with facility the carbonic acid from both the native and artificial carbonates of barytes, as well as that of strontian, without using black-lead crucibles; and I shall here describe the method which has constantly proved successful with me, and the comparative experiments which I have made on this subject.

Process for separating the Carbonic Acid from the Carbonates of Barytes and Strontian.

1. TO 100 grains of native carbonate of barytes in powder, I added 10 grains of powdered charcoal; and, the whole being well mixed, I made it into a stiff paste with boiled starch, and rolled it into a ball. Having put into a crucible a little fresh-burnt charcoal

* In the specific gravities in the original, that of distilled water is supposed to be 10, the decimal point being put one place more forward than is here done. I have ventured, in conformity to the more usual, and, as I think, more convenient, method of putting unity for this common measure, to point them as above. T.

in powder, and laid the ball on it, I covered it with powdered charcoal, and luted on a cover with common loam. Things being thus disposed, the crucible was kept in a very strong fire for a full hour, which time was sufficient to disengage the carbonic acid. The crucible being cold, I opened it, and found the little ball perfectly compact; but it now weighed only 70 grains. I then triturated it in a glass-mortar with about nine ounces of boiling water, filtrated the liquor, and, in order to dispose it the more to crystallization, put it into a glass retort, to separate a portion of the water by distillation. By cooling, there were formed in the retort crystals several lines in length.

2. One hundred grains of carbonate of barytes prepared from the sulphate by the ordinary processes were treated with 10 grains of powdered charcoal, as above described, and the crucible equally exposed to heat for only an hour. Having then treated the residue with boiling water, and concentrated the liquor in a retort, I obtained, on its cooling, crystals similar to those before mentioned.

3. One hundred grains of carbonate of strontian being treated in the same manner, the residue, after calcination, weighed only 72 grains. Its solution in hot water appeared to me more saturated than that of barytes; and, without having recourse to concentration, I obtained crystals on its cooling, though I had used about the same quantity of water as in the preceding experiments. I do not, however, believe that strontian is much more soluble than barytes; for in several other experiments I have had solutions of barytes so fully saturated as to crystallize by cooling confusedly and in a mass. Thus it is very evident that the action of fire separates carbonic acid from barytes and strontian, and that these earths then become soluble in water, and in larger quantity if boiling, so as to give crystals by cooling. This character does not therefore belong exclusively to strontian, as M. Klaproth supposes.

I have also remarked that the aqueous solutions, both of barytes and strontian, when pure or calcined, have an odour somewhat similar to that of caustic pot-ash or soda, or what is commonly called a lixivious odour.

Habitudes of the Carbonates of Barytes and Strontian.

With nitric acid. NATIVE carbonate of barytes was totally dissolved by diluted nitric acid, and the solution attended with a disengagement of carbonic acid gas, in the proportion of twenty-two parts in the hundred. The concentrated liquor afforded crystals, the most ordinary figure of which was octahedral.

One hundred grains of carbonate of strontian were also dissolved in nitric acid; but the disengagement of carbonic acid gas was more considerable than in the preceding experiment, the proportion being thirty hundredths. The salt which results from this combination also crystallizes in octahedrons.

[To be continued.]

PHILOSOPHICAL NEWS AND ACCOUNTS OF BOOKS.

ON the first of Brumaire (21 Oct. 1797.) Citizen Garnerin made the experiment of the parachute at the Garden de Mousseaux*. This experiment has not been before attempted at Paris. Blanchard had the notion soon after the discovery of balloons; and at several different towns, particularly Lisle, he let fall from the vessel of his balloon dogs and other animals. Some years ago he ventured to descend in person in an experiment he made at Bâle; but either from the bad construction of his parachute, or by falling among trees, he had the misfortune to break one of his legs.

Citizen Garnerin was more fortunate, and has given the most satisfactory proofs of his skill, firmness, and intrepidity, which the impatient public had seemed to doubt at the Garden Biron. Notwithstanding the haste with which this philosopher was obliged to make his preparations, and several accidents which happened to his apparatus, he ascended from the Garden de Mousseaux at half past five in the evening. Between the balloon and the vessel was placed the parachute, half opened, and forming a kind of tent over the aerial traveller. The wind, which through the course of the day had been violent, was now become calm, as if to favour his enterprise, and carried him to the northward, over the plain of Mousseaux. Full of that interest, that insurmountable emotion which seizes us on beholding a man quit the earth and advance towards the clouds by an apparatus so majestic, the eyes of the spectators were fixed on the balloon, which rose with rapidity. When it was at a considerable height, the parachute and vessel were seen at once to separate from the balloon; the latter of which burst (*celui-ci éclate*), emptied itself, and floated down with the wind. The parachute unfolded itself, while the vessel which served as ballast drew it towards the earth. Its fall was at first slow and vertical; but soon afterwards it exhibited a kind of balancing or vibration, and a rotation gradually increasing, which might be compared with that of a leaf falling from a tree. Cries expressive of the general terror and astonishment were heard on all sides. The crowd rushed towards the place of his descent, where at length the aeronaut landed, and without injury. Emotions of joy and congratulation succeeded those of alarm, and he was brought back in triumph to the Garden of Mousseaux.

Such was the experiment. The narrator then proceeds to give the following detail:—The parachute, which resembles a vast umbrella, is of cloth, and its diameter, when unfolded, is 25 feet. Citizen Garnerin estimates the height from which he fell at 300 toises, but C. Say reckons it no more than one-third of that space†. He had 75 pounds of ballast in his car at the moment of his fall, which he says he should have thrown out, if he had not been apprehensive of wounding the spectators below. C. Say thinks he would have done very wrong; because the danger to which he was exposed did not arise from the velocity of his fall, but the vibration of the car, which might have struck him against the earth, or against walls, trees, or other prominences. Now, if he had thrown out his ballast, the remarker thinks he would have rendered the vibration much more rapid, extensive, and dangerous, and perhaps even have caused his parachute to overset, and the whole to fall in a mass.

The author explains the theory of the parachute by observing that the resistance of the air

* This account was drawn up by J. B. Say, Editor of *La Décade philosophique, littéraire, et politique*, No. 4.

† In the way of rough illustration, this may be stated at once and a half the height of St. Paul's Cathedral in London. N.

and gravitation are two forces which act at the same time on the whole system of the machine; that gravitation acts as if the whole mass were united at its centre of gravity, while the resistance acts as if the whole of this mass was united at another centre little distant from the centre of gravity of the cloth which forms the parachute, and very different from the former. The parachute may be considered as suspended in the air by this centre of resistance.

If the centre of gravity be not vertically beneath the centre of resistance when the parachute is properly placed, it is evident (says he) that it will incline to one side, descend obliquely, oscillate, and the smallest irregularity in its figure will cause it to turn round its vertical axis. It is also important that these two centres should be at a distance from each other, since the oscillations will more readily take place the nearer they are. If they were coincident, there would be no cause why the whole apparatus should not overset. C. Say therefore proposes that the car or part of the ballast should be suspended at a considerable distance below the parachute.

ACCOUNTS OF BOOKS.

Recherches expérimentales sur le Principe de la Communication latérale du Mouvement dans les Fluides, appliqué à l'Explication de différens Phénomènes hydrauliques. Par le Citoyen J. B. Venturi, Professeur de Physique expérimentale à Modène, Membre de la Société Italienne, &c. &c. A Paris, chez Houel & Ducros, Rue du Bacq, No. 940—Théophile Barrois, Rue Haute-feuille, No. 22. Ann. VI. 1797.—Or, *Experimental Researches concerning the Principle of the lateral Communication of Motion in Fluids, applied to the Explanation of various Hydraulic Phenomena.* By Citizen J. B. Venturi, Professor of Experimental Philosophy at Modena, &c. &c.

I HAVE been favoured with a copy of this curious and interesting work by the learned Professor, which I have read with much pleasure. The commission nominated for that purpose by the National Institute of France have given a very correct analysis, which I shall chiefly follow in my account.

Citizen Venturi has introduced an horizontal current of water into a vessel filled with the same fluid at rest. This stream entering the vessel with a certain velocity, passes through a portion of the fluid, and is then received in an inclined channel, the bottom of which gradually rises, until it passes over the border or rim of the vessel itself. The effect is found to be, not only that the stream itself passes out of the vessel through the channel, but carries along with it the fluid contained in the vessel; so that after a short time no more of the fluid remains than was originally below the aperture at which the stream enters. This fact is adopted as a principle or primitive phenomenon by the author, under the denomination of the lateral communication of motion in fluids, and to this he refers many important hydraulic facts. He does not undertake to give an explanation of this principle, but shews, p. 37, that the mutual attraction of the particles of water is far from being a sufficient cause to account for it.

The first phenomenon which the author proposes to explain by this established principle is the emission of a fluid through different adjutages applied to the reservoir which contains it. It is known that the vein of fluid which issues from an orifice or perforation through a thin plate, becomes contracted, so as to exhibit a section equal to about $\frac{1}{4}$ of the orifice itself, supposed to be circular; and that the place of the greatest contraction is usually at the distance

tance

tance of one semi-diameter of the orifice itself. If a small adjutage be adapted to the orifice, having its internal cavity of the same conoidal form as the fluid itself affects in that interval, the expenditure is the same as by the simple orifice. But if at the extremity of this adjutage a cylindric tube be affixed, of a greater diameter than that of the contracted vein, or a divergent conical tube, the expence of fluid increases, and may exceed the double of that which passes through the aperture in the thin plate, though the adjutage possess an horizontal or even ascending direction.

By the interposition of a small adjutage, adapted to the form of the contracted vein, Citizen Venturi ascertained, in the first place, that there is an increase of velocity in the tubes he employed, though the velocity of emission itself be less than that of the stream which issues from an hole in a thin plate. He afterwards proves, by the fact, that the interior velocity and expenditure of fluid, which is increased through tubes, even in the horizontal or ascending direction, is owing to the pressure of the atmosphere. If the smallest hole be made in the side of the tube near the place of contraction of the vein, the increased expenditure does not take place; and when a vertical tube is inserted in such a hole, the lower end of which tube is immersed in water or mercury, it is found that aspiration takes place, and the water or mercury rises; and this aspiration in conical tubes is less in proportion, as the place of insertion of the upright tube is more remote from the section where the greatest contraction would have taken place. And, lastly, the difference between the expenditure of fluid, through an orifice made in a thin plate, and that which is observed through an additional tube, does not take place in vacuo.

The influence of the weight of the atmosphere on the horizontal or ascending flux being thus established, the author considers it as a secondary cause, referable to, and explicable by, his principle of the lateral communication of motion in fluids. In conical divergent tubes, for example, the effect of this lateral communication is, that the central cylindrical jet, having for its basis the section of the contracted vein, carries with it the lateral fluid which would have remained stagnant in the enlarged part of the cone. Hence a vacuum tends to be produced in this enlarged part which surrounds the central cylindrical stream; the pressure of the atmosphere becomes active to supply the void, and is exerted on the surface of the reservoir, so as to increase the velocity of the fluid at the interior extremity of the tube.

The author proves, that the velocity or total expenditure of fluid through an aperture of given dimensions, may be increased by a proper adjutage in the proportion of 24 to 10: he applies this result to the construction of the funnels of chimneys. He determines the loss of emitted fluid, which may be sustained by sinuosity in pipes. He shews by experiment, that a pipe which is enlarged in any part, affords a much less quantity of fluid than if it were throughout of a diameter equal to that of its smallest section. This, as he remarks, is a circumstance to which sufficient attention has not been paid in the construction of hydraulic machines. It is not enough to avoid elbows and contractions; for it sometimes happens, that by an intermediate enlargement the whole of the advantage arising from other judicious dispositions of the parts of the machine is lost.

There are two causes of the increase of expenditure through descending pipes. The first is owing to the lateral communication of motion which takes place in descending pipes, in the same manner as in those which possess an horizontal situation; the second arises from the acceleration by gravity which takes place in the fluid while it falls through the descending

ing tube. This second kind of augmentation was known to the ancients, though they possessed no good theory nor decisive experiments respecting it. The author endeavours to establish a theory on the principle of virtual ascension combined with the pressure of the atmosphere. His deductions are confirmed by experiment, in which he has succeeded so far as to separate the two causes of augmentation, and assigned to each their respective degree of influence.

Citizen Venturi then proceeds to different objects of enquiry, to which his principle seemed applicable. He gives the theory of the water-blowing * machine, and he determines by calculation the quantity of air which one of these machines can afford in a given time. He observes, that the natural falls of water in the mountains always produce a local wind; and he even thinks, that the falling streams in the internal parts of mountains are in some instances the cause of the winds which issue from caves. He proves, by the facts, that it is possible in certain instances to carry off, without any machinery, the waters from a spot of ground, though it may be situated on a lower level than that of the channel which is to receive the water.

The whirlpools, or circular eddies of water so frequent in rivers, are, according to the theory of our author, the effect of motion communicated from the parts of the current which are most rapid, to those lateral parts which are least so. In the application of this principle, he points out the circumstances adapted to produce such eddies at the surface or at the bottom of rivers. He concludes, that every movement of this kind destroys a part of the force of the current, and that in a channel through which water constantly flows, the height of this fluid will be greater than it would have been if the dimensions of the channel had been uniformly reduced to the measure of its smallest section.

There is another kind of whirling motion somewhat different in its nature from these last. It is produced in the water of a reservoir, when it is suffered to flow through an horizontal orifice. The author deduces the theory of these vortices from the doctrine of central forces. The form of the hollow-funnel, which in this case opens through the fluid of the reservoir, is a curve of the 64th species of the lines of the third order, enumerated by Newton. Theory and experiment both unite here in proving, that it is not only possible, but that there really exists in nature a vortex, the concavity of which is convex towards the axis, and of which the revolutions of its different parts follow the ratio of the square of the distance from the centre. Daniel Bernoulli was in the wrong, in his Hydrodynamics, to reproach Newton for having supposed a vortex to be moved according to this law.

In the last place, the author considers that lateral communication of motion which takes place in the air as well as in the water. This is the cause of such local and partial

* *Soufflet d'eau*. It is also called *trompe*, but we have no appropriate name for this engine in English. The reader may consult Lewis's Philosophical Commerce of Arts, and Chaptal's Elements of Chemistry, for descriptions. It consists of an upright pipe, through which a shower of water is made to fall. This shower carries down a mass of air with it, which is received beneath a kind of tub, and conducted to the furnace by means of a pipe. The most remarkable natural phenomenon of this kind is the squall at sea. When a cloud suddenly falls in the form of rain, it drives down a portion of the atmosphere, which glides rapidly along the surface of the sea, and is capable of suddenly upsetting vessels, or carrying away their masts. It may easily be imagined, that seamen are very attentive to look out for this phenomenon, and to guard against it in time by lowering their sails, &c. N.

winds as sometimes blow contrary to the direction of the general wind. It is by virtue of the same principle, that the resonant vibration, excited laterally in the extremity of an organ-pipe, is communicated to the whole column of air contained in the pipe itself.

From the same principle, the author deduces the augmentation of force which sound receives in conical divergent tubes, compared with those of a cylindrical form. On this occasion, he points out the remarkable differences which appear to take place between the resonant vibrations of air contained in a tube, and the sonorous pulsations propagated through the open atmosphere.

In an Appendix, Citizen Venturi relates different experiments which he has made to determine the convergence and velocity of the fluid filaments which press forward to issue out of a reservoir by an orifice through a thin plate. He proves, by a very clear experiment, that the contraction of the vein is made at a greater distance from the orifice under strong than under weak pressures. He explains, why in a right-lined orifice, the sides of the contracted vein correspond with the angles of the orifice and the angles with the sides. He examines the expenditure through a tube, the extremity of which is thrust into the reservoir itself, according to the method of Borda in the Memoirs of the Academy of Sciences for the year 1766.

The Commissaries of the Institute appointed to examine this work of Citizen Venturi, without undertaking to decide respecting all the applications of his principle, give him full credit for the acuteness and sagacity he has displayed in this curious experimental course. The author himself is indeed sufficiently candid to admit that every thing of this nature, which is not confirmed by direct experiment, ought to form a subject of discussion and enquiry. "I have not," says he, page 9, "insisted upon theoretical considerations, excepting so far as they combine with the facts, and so far as it was necessary to unite those facts in a single point of view. The reader may, if he pleases, even omit the small portion of theory, and consider my propositions simply as the result of experiment."

There is no doubt but that the whole work will tend to confirm the reputation of its author as a skilful experimentalist, and enlightened observer.

Considerations on the Doctrine of Phlogiston and the Decomposition of Water. By Joseph Priestley, LL. D. F. R. S. &c. Philadelphia printed, 1796.

Observations on the Doctrine of Phlogiston and the Composition of Water. Part II. By Joseph Priestley, LL. D. F. R. S. Philadelphia printed, 1797.

Two Lectures on Combustion. Supplementary to a Course of Lectures on Chemistry, read at Nassau Hall, containing an Examination of Dr. Priestley's Considerations on the Doctrine of Phlogiston and the Decomposition of Water. By John Maclean, Professor of Mathematics and Natural Philosophy in the College of New Jersey. Philadelphia printed, 1797.

As I hope to give a fuller account in future of Dr. Priestley's observations in favour of the old chemical system, I shall for the present content myself with announcing the titles of the three last works.

The Medical Repository, Vol. I. Nos. I. and II. octavo. The two numbers contain 287 pages, closely printed. New-York, printed for T. and J. Swords.

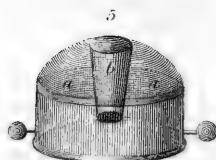
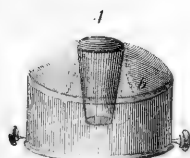
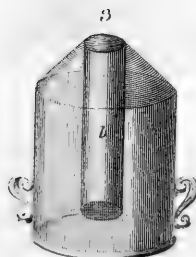
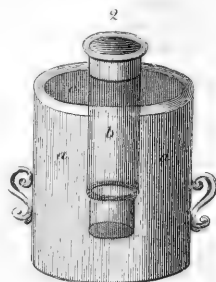
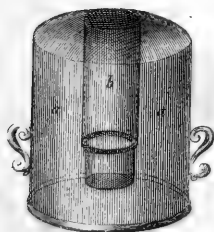
The authors and editors of this work are Doctors Samuel L. Mitchill, Member of the Legislative Assembly of the State of New-York, F.R.S. Edin. Professor of Chemistry in Columbia College, and Doctors Edward Miller, and E. H. Smith, of New-York. The first Number appeared in July 1797, and the second in the month of November last. It is intended to be published in quarterly numbers of at least 100 octavo pages each, at the price of half a dollar per number. It consists of medical essays or communications, a review of new publications, not only such as are strictly medical, but also those which relate to agriculture and other branches of natural history, philosophy, &c. or may be in any manner related to the objects contemplated in the plan of a medical repository; and lastly, medical facts, hints, enquiries, and news.

It is a well known fact, that the public will not assist in any periodical work until they have received proof, for a certain length of time, of the accuracy, spirit, and ability, with which the conductors are able from their own sources, whether original or derivative, to support it. A work of the kind before us is peculiarly calculated for the diffusion of knowledge in a country like America, where, from various circumstances, the written sources of intellectual acquisition are much more limited than in Europe. It is equally calculated to accelerate improvement in the whole of that considerable part of the globe, wherein the language of England is current. The present numbers shew no want either of ability or industry on the part of the authors, and will, it is to be hoped, meet the success they merit.

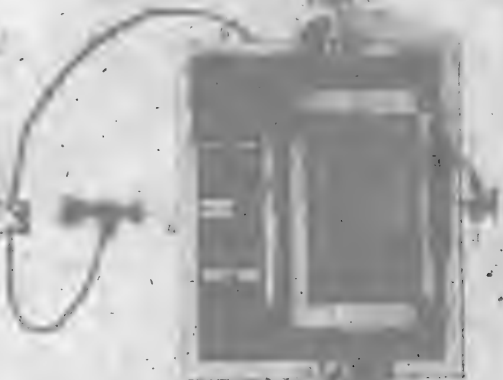
MY young correspondent from Liverpool has shewn considerable ability in his meditations and conjectures on the many important chemical facts he mentions. When facts can be so disposed as by mutual illustration to prove an entire theory, the individual who makes the arrangement, may, perhaps, do more for the advancement of science, than the performer of many solitary experiments: but when this cannot be done, the chief use of imperfect conclusions or conjectures must be to point out new and decisive experiments. W. S. may support the hope of making such experiments, by reflecting that the greatest discoveries have been made by men who did not possess the means of acquiring much apparatus. Apothecaries' phials, Florence flasks, basons, cups, saucers, a blow-pipe and charcoal, common tobacco-pipes and garden-pots, as substitutes for crucibles and a furnace, and the ordinary bellows, together with a few chemical materials, would constitute no mean apparatus for philosophical experiment. When he shall turn his thoughts towards the experimental researches, to which his observations evidently lead, I am confident he will be of opinion, that the publication of his conjectures, in their present state, would not be desirable.

Mr. Walker's freezing Process

Fig. 1.



U. S. GOVERNMENT PRINTING OFFICE



M.^r Pearson's portable Electrical Machine.

Fig. 1.

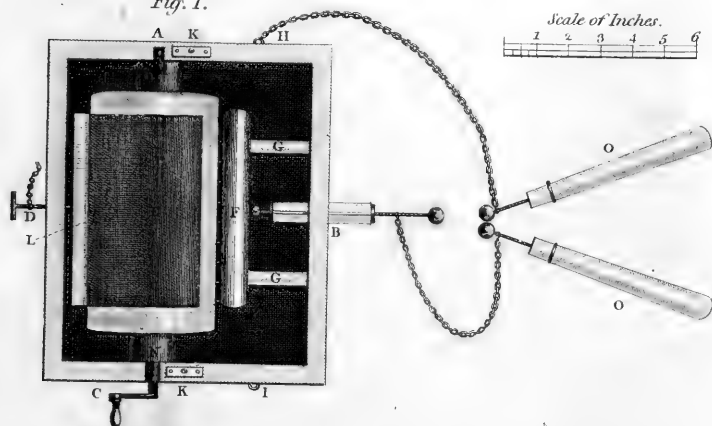


Fig. 2.

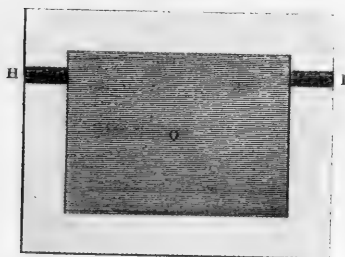


Fig. 3.





A
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OF
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AND
THE ARTS.

MARCH 1798.

ARTICLE I.

*Observations on Strontian. By Citizen PELLETIER. Read to the National Institute
30th April, 1796.*

[Concluded from p. 222.]

Habitudes of the Carbonates of Barytes and Strontian.

With muriatic acid. I DISSOLVED 100 grains of native carbonate of barytes in muriatic acid, and obtained 22 grains of carbonic acid gas. The solution being evaporated afforded crystals in the form of short flattened prisms, or hexagonal plates, the weight of which was 138 grains. I also dissolved in the same acid 100 grains of carbonate of barytes, obtained by the decomposition of barytic sulphate: the disengagement of carbonic acid gas also amounted to 22 grains, and the muriate crystallized like the preceding, so that there was not any difference in their appearance *.

It

* The success which Dr. Crawford met with in the internal use of muriate of barytes, in the treatment of scrophulous diseases, begins to be known in France; and several physicians already prescribe this new medicine. As, however, it is very active, and sometimes even dangerous, it cannot be too strongly recommended that it be given at first only in a very small dose, and that the effects which it may produce during its administration be accurately observed. The Society of Health is at present engaged in a general work, in which it is intended to determine what advantages medicine may derive from the use of muriate of barytes. They have also appointed commissaries to observe its effects in the treatment of horses; and Cit. Hufard and Biron, who are intrusted with these latter experiments, have already given both the muriate and carbonate of barytes to horses seized with the fever [farcin]. Both these medicines given to different horses in the quantity of two drachms a day seemed for a while to have effected a perfect cure: they were even considered as cured at the end of a fortnight;

It is not the same with the carbonate of strontian : this was totally dissolved by muriatic acid, but the disengagement of carbonic acid gas amounted to thirty hundredths, and the salt obtained from this combination was in long needles or very slender rhomboidal crystals, terminated by a pyramid with two faces : the prisms are sometimes hexahedral. This salt is also more soluble than muriate of barytes. I obtained 176 grains of the muriate from 100 grains of the carbonate. Dr. Crawford was one of the first who observed the great difference which there was in the form of the crystals of the muriates of barytes and strontian, as well as their different degrees of solubility in water, and thence suspected that these two substances could not be of the same nature.

With sulphuric acid. Native carbonate of barytes reduced to powder is decomposed by sulphuric acid, with disengagement of carbonic acid gas. The result of this combination is sulphate of barytes, insoluble in water.

Carbonate of strontian is also decomposed by sulphuric acid, with disengagement of carbonic acid gas; and the compound which is obtained is also but little soluble in water. Dr. Hope has observed that four ounces of distilled water only dissolved half a grain of it, and that if muriate of barytes be added to this solution, a precipitation of sulphate of barytes takes place : sulphate of strontian is therefore more soluble than that of barytes.

With acetic acid. The acetic acid disengages the carbonic acid from carbonate of barytes.

The same acid also decomposes carbonate of strontian. The salts which result from these combinations, viz. the acetites of barytes and strontian, are obtained in a crystalline form, and are not deliquescent.

Red Flame of Alcohol, holding in solution Nitrate or Muriate of Strontian or Lime.

AMONGST the characters by which Messrs. Hope, Klaproth, and others have distinguished strontian from barytes, there is one which they agree in considering as peculiarly distinctive. Chemists have observed that alcohol, in which either nitrate or muriate of barytes is dissolved, burns with a flame of a white yellow, whilst alcohol holding in solution nitrate or muriate of strontian burns with a flame of a carmine red. Dr. Hope relates in his memoir that Dr. Ash had so long since as 1787 observed the particular colour which muriate of strontian gives to the flame of alcohol. The experiments repeated before the pupils of the polytechnic school presented the same results; but as the nitrate and muriate of lime also communicate to alcohol the property of burning with a red flame, I thought it right to ascertain whether the nitrate and muriate of strontian did not contain lime. For this purpose I tried the following experiments :

when the one to which the muriate of barytes had been given died without any apparent circumstance to indicate his death : the second, that which had taken the artificial carbonate of barytes, also died suddenly some days afterwards. The two horses having been opened, all the viscera were found in a sound state, and exhibited no indication, either of the effect of the barytic preparations, or of the repercussion of the disease. The experiments are continued on other horses.

Since this article has been compiled, I have been informed, that a third horse which was under a course of carbonate of barytes, also died suddenly. It seems, therefore, more and more evident, that the carbonates of barytes, whether native or artificial, may prove mortal when taken internally. P.

Experiments to ascertain whether Strontian contains Lime.

1. TO a solution of nitrate of barytes I added fluoric acid, perfectly pure and free from sulphuric acid: the mixture took place without precipitation.

2. To a solution of nitrate of strontian I added the same fluoric acid: the mixture also took place without precipitation.

3. To solutions of the nitrates of barytes and strontian put into separate vessels I added two or three drops of nitrate of lime, and afterwards some fluoric acid: this acid then very soon produced a white precipitate, which was fluuate of lime.

It follows, from these comparative experiments, that the nitrate of strontian did not contain lime; for, if it had contained ever so little, the fluoric acid would have occasioned a precipitation. This acid has so strong an affinity with lime that it takes it from the sulphuric acid, when sulphate of lime is holden in solution in water; so that when a few drops of fluoric acid are added to a selenitic water, a precipitation takes place as readily as when the oxalic acid is used. The fluoric acid presents therefore an excellent means of detecting the presence of lime. Very pure fluuate of ammoniac may also be used; for, if this be mixed with the nitrate, muriate, or sulphate of lime, a decomposition takes place by double affinity, and the precipitate is fluuate of lime.

Nitrate of Strontian is not precipitated, as Nitrate of Barytes is, by the Prussiates of Pot-ash or Lime.

WE have seen, that the characters of strontian in which it appears most similar to barytes, exhibit, nevertheless, differences sufficiently well marked, when they are subjected to a rigorous examination. The following experiment, which is owing to the observations of Dr. Hope, presents a more striking distinction between the two earths. It was known that nitrate of barytes is totally precipitated by the prussiates of pot-ash and lime. Dr. Hope having added prussiate of pot-ash to a solution of nitrate of strontian, perceived only a slight precipitation, occasioned by the iron which accompanies the carbonate of strontian. I had a mind to ascertain this myself, and therefore prepared three solutions, viz. 1. A solution of native carbonate of barytes in nitric acid: 2. A solution in the same acid of carbonate of barytes, prepared by the decomposition of barytic sulphate: 3. A solution of carbonate of strontian in the same acid. These three solutions being placed in separate vessels, I added a sufficient quantity of prussiate of pot-ash, to decompose them entirely. There was in effect an abundant precipitate in the nitrates of barytes, and the supernatant liquors being tried with carbonate of pot-ash gave no sign of further precipitation. The nitrate of strontian, on the contrary, gave only a weak blue precipitation, in consequence of the iron which it contained; and an excess of prussiate of pot-ash precipitated nothing more: but the supernatant liquor, tried with carbonate of pot-ash, afforded a very abundant white precipitate, which was carbonate of strontian.

Thus it appears manifest, that prussiate of pot-ash does not at all decompose the nitrate of strontian, whereas it totally decomposes the nitrate of barytes. This method points out, therefore, a distinctive character between the two earths. I shall not here examine the nature of the precipitate which is obtained from the nitrate of barytes when decomposed by prussiate of pot-ash, nor whether the decomposition takes place in consequence of the forma-

tion of a trifule, or of a mixture of sulphuric acid, which the prussiate of pot-ash may sometimes contain. The comparative experiments which I have stated having been made with the same prussiate of pot-ash, the nitrate of strontian ought to have been decomposed, as well as that of barytes, if these earths had been of the same nature.

Constituent Parts of the Native Carbonates of Strontian and Barytes.

TO detail at large the different experiments which I have made to determine the constituent parts of the native carbonates of strontian and barytes, would be too prolix: I shall therefore only say, that it was by solution in muriatic acid that I determined the quantity of carbonic acid gas, and by calcination that of the earth. The mean result which I obtained was,

In 100 grains of native carbonate of barytes, or witherite,			In 100 grains of carbonate of strontian,		
Barytes,	-	62	Strontian,	-	62
Carbonic acid,	-	22	Carbonic acid,	-	30
Water,	-	16	Water,	-	8
		100			100

Conclusion.

I HAVE not been able to carry my researches further respecting strontian, not having any more to submit to new experiments: as soon as I can procure some, I shall continue them, as I perceive that it will be advantageous to establish the difference between it and barytes by more numerous and more striking characters. The results of my examination, and that of Messrs. Hope and Klaproth, are:

- 1st, That the carbonate of strontian is neither poisonous nor emetic, as both the native and artificial carbonates of barytes are;
- 2dly, That the carbonate of strontian is specifically lighter than that of barytes;
- 3dly, That it more easily parts with it's carbonic acid, and contains also a larger proportion of it than carbonate of barytes does;
- 4thly, That calcined strontian is soluble in cold and hot water, but more abundantly in the latter, so as to afford crystals by cooling;—a property, indeed, which it possesses in common with barytes;
- 5thly, That the muriate and other salts of strontian are more soluble than the homologous barytic salts; and the nitrate and muriate communicate to alcohol the property of burning with a red flame, whereas the same barytic salts tinge the flame of their alcoholic solution with a yellowish blue;
- 6thly, That strontian does not contain lime;
- 7thly and lastly, That the nitrate of strontian is not decomposed by prussiate of pot-ash, which decomposes the nitrate of barytes.

All these characters already establish a sufficiently distinguishable difference between strontian and barytes, and a still greater between it and all other known earths, so that it may be considered as a peculiar earth.

CON-

CONTINUATION.

IN the memoir which I read to the Institute the 30th of April last, on the earth known by the name of strontian, I remarked, that this earth was, when calcined, very soluble in boiling water, and that a portion became separated in a crystalline form by cooling. I also remarked, that barytes acquired by calcination the property of becoming soluble in boiling water, and also affording crystals by cooling. It is in this state of crystallization that strontian and barytes must be found pure; and if they are of the same nature, as some chemists conceive, they ought to give similar results when submitted to comparative experiments. It has been thought that the properties which this earth exhibits in its combinations with the acids might be attributed to the presence of carbonate of lime in the carbonate of strontian; but calcined strontian dissolved in water cannot be suspected to contain lime, and it is therefore in this state that I have thought proper to procure some, in order to examine it comparatively with barytes, which I have also taken the pains to prepare in like manner in crystals, and perfectly deprived of carbonic acid. I shall here relate to the Institute the experiments which I have made on this subject.

1. I first put into a small capsule sixteen grains of pure crystallized barytes, with thirty-six grains [demi-gros] of pure nitric acid of $12\frac{1}{2}^{\circ}$. The combination took place with disengagement of caloric, and the crystals of barytes without being dissolved soon presented opaque crystals, which were nitrate of barytes. I added 144 grains [deux gros] of distilled water, and the whole was dissolved.

2. Sixteen grains of crystals of strontian were treated with thirty-six grains of the same acid. The combination took place with disengagement of caloric, but the crystals were dissolved, and did not become opaque as in the preceding experiment. I added 144 grains of water, in order to have a solution similar in strength to the former.

3. A portion of the solution, No. 1, or that of nitrate of barytes prepared with pure barytes, being put into a glass, I added to it a pretty considerable quantity of prussiate of pot-ash. A precipitation took place, and the liquor was not afterwards precipitated by the addition of carbonate of potash.

The other portion of the nitrate being evaporated, afforded crystals of barytic nitrate in octahedrons.

4. A portion of the solution, No. 2, or of the nitrate of pure strontian, tried in like manner with prussiate of pot-ash, afforded a slight precipitation; but, although I added an excess of the prussiate, the supernatant liquor still yielded a pretty abundant precipitate with carbonate of potash.

The other portion of the solution of nitrate of strontian being evaporated, afforded octahedral crystals, which were more soluble than those of the barytic nitrate.

5. Fifty-four grains of barytes, pure and in crystals, being treated with 144 grains [deux gros] of weak nitric acid, the solution took place with heat, but without effervescence. I added 144 grains of distilled water, and placed the solution to evaporate in a sand-bath slightly heated; and when a portion of the water was evaporated, there were formed crystals in hexagonal plates similar to those of muriate of barytes. Their weight was thirty-two grains.

Twelve grains of this muriate, triturated in a glass mortar with half an ounce of alcohol,

were

were entirely dissolved. I put the whole into 3 matrafs, and endeavoured to favour the solution by means of heat; a portion of the salt remained, however, undissolved at the bottom of the matrafs.

Having set the solution on fire, the alcohol burned with a yellowish flame.

6. Fifty-four grains of crystals of strontian being treated comparatively with the same muriatic acid, the solution also took place with heat. The liquor being evaporated in an equal degree with that of the muriate of barytes (which, although upon the fire, had already crystallized) still remained fluid; but, on being taken off the fire, it crystallized in a mass, and in needles like muriate of strontian. This salt is therefore more soluble in water than the muriate of barytes. The weight of the muriate of strontian obtained in this experiment was thirty-eight grains.

This muriate is also more soluble in alcohol; for, having triturated twelve grains in a glass mortar with seventy-two grains of alcohol, they were entirely dissolved. I added, however, a sufficient quantity of alcohol to make the whole half an ounce, as in the preceding experiment; and having then set it on fire, it burned with a flame of a beautiful red.

7. A saturated [aqueous] solution of pure barytes affords by the addition of a few drops of malic acid a white precipitate, which is malate of barytes; but the same acid does not produce any precipitation in a saturated solution of strontian: it follows, therefore, that malate of strontian is more soluble than malate of barytes.

These experiments continue, therefore, to establish a difference between pure barytes and pure strontian.

Carbonate of strontian has hitherto been found only in one place; Strontian in Argyleshire: but M. Meyer, apothecary at Stettin, has lately announced that this earth is found in combination with sulphuric acid in the sulphate of barytes at Freyberg in Saxony. In consequence of this observation I analysed a piece of barytic sulphate from Saxony, but the earth which I obtained from it was only barytes, and not strontian. I presume that the sulphate which I assayed was different from that of which M. Meyer speaks; but I have, at all events, reason to know that strontian exists in other places as well as Argyleshire. Citizen Guyot, who travelled into Scotland with the Citizen Delessere, sent me some years since several specimens of minerals which he had himself procured on the spot, amongst which there is one which is labelled in the handwriting of Citizen Guyot "*Barytes from Leadhills*" in Scotland. I have examined this specimen, and find that it is carbonate of strontian. Its colour is a greenish white, and it seems formed by the union of a number of prisms, which gives it a striated appearance. Its specific gravity is 3.6195. This carbonate, treated with muriatic acid, yields $31\frac{1}{2}$ per cent. of carbonic acid gas, and affords a muriate which crystallizes in needles, and which, when dissolved in alcohol, communicates to it the property of burning with a red flame: in short, this muriate appears to me perfectly similar to muriate of strontian. I have also treated the stone from Leadhills with the other acids, and the products which I have obtained demonstrate that the base of this carbonate differs in no respect from strontian: here, therefore, we have this earth as well as at Strontian, and very probably the researches of mineralogists will discover it in many other places.

II.

Abstract of Two Memoirs on a New Method of obtaining Barytes pure, and on the Properties of this Earth compared with those of Strontian. By Citizens FOURCROY and VAUQUELIN. Read to the National Institute, 30th April and 21st September 1796.*

THE difficulty of obtaining barytes pure, and the almost total impossibility of separating this earth from the carbonic acid by calcination of its carbonate, are already sufficiently known. In a course of Experiments undertaken jointly by Citizens Fourcroy and Vauquelin, to determine the characters and distinctive properties of the salts and compounds formed by barytes, one of the points which they were most desirous of attaining, was of course to obtain this earth in a real state of purity. Citizen Vauquelin having at length discovered a means of effecting this purpose by the decomposition of nitrate of barytes by the action of fire, it became more easy to those chemists to examine such properties of this earth as had hitherto been unknown to them. The two memoirs of which we are here about to give an abstract, show the most remarkable properties which characterize both this earth and strontian.

1. Nitrate of barytes in octahedral crystals, being exposed to the action of fire in a porcelain retort, melts, swells, and gives out much oxygenous and azotic gas, with scarcely any nitrous vapour; and the retort being suffered to cool when no more elastic fluid is disengaged, there is found on breaking it a grey, solid, but somewhat porous, mass of a harsh taste, and more caustic than quick-lime, which is pure barytes.
2. Exposed to the blow-pipe on a piece of charcoal, this earth fuses, bubbles up, and runs into globules which quickly penetrate into the charcoal.
3. In the air it effloresces, cracks, bursts, swells up, heats and whitens, and, becoming thus rapidly flaked, it absorbs 0.22 of its weight of water and carbonic acid.
4. It absorbs water with extreme avidity, melts with hissing, heats considerably, solidifies the water, and crystallizes and hardens with it in such a manner as to become a very tenacious cement, capable of adhering very strongly to glass. A little more water changes it into a very bulky white powder. If it be entirely covered with water, it dissolves in it with a violent hissing, and then crystallizes in transparent needles, which adhere together, and form compages like the particles of beaten plaster.
5. Cold water dissolves a twenty-fifth part of its weight; boiling water more than half: the latter by cooling deposits very beautiful transparent prisms, which effloresce and become pulverulent when exposed to the air.
6. A solution of barytes has its surface more readily incruited with a pellicle by exposure to the air, and deposits a more abundant precipitate by carbonic acid than lime-water.
7. The oxalic, citric, phosphoric, and phosphorous acids precipitate this solution, and the precipitates are re-dissolved by the addition of an excess of the acids by which they are formed.
8. This solution decomposes those of soap, and of the nitrates of mercury, lead, and

* Translated from *Annales de Chimie*, XXI. p. 276, by J. F.—r.

silver, precipitating the first black, the second white, and the last of a fawn-colour. An excess of the barytic solution renders the oxides of lead and silver soluble.

9. Barytes thus prepared is soluble in alcohol. It is dreadfully poisonous, and kills animals. Its most remarkable and most characteristic properties are its extreme crystallizability, which distinguishes it from all the other earths hitherto known in chemistry, and its great solubility in water.

The discovery by Messrs Hope and Klaproth of these two latter properties in strontian induced us however to think for a while, with Citizens Pelletier and Coquebert, that barytes was so similar to it as that they could not but be considered as very nearly allied, and perhaps even one and the same earth; but having received some fragments of strontianite, of which we had not before been enabled to investigate the properties ourselves, we were eager to examine this new earth comparatively with barytes; and the result of this examination, as related in the second memoir, which we are now abstracting, shewed us that, notwithstanding some very strong analogies, these two earthy substances are really different, as Citizen Pelletier has concluded from his own researches relative to this subject. The following are the principal facts stated in this second memoir.

Native carbonate of strontian has a shade of light green, which that of barytes has not; it requires a little more muriatic acid for its solution, and contains more carbonic acid. Muriate of strontian crystallizes by cooling, whilst that of barytes crystallizes by evaporation: the former in hexagonal prisms, the latter in inclined plates. The solution of muriate of strontian thickens into a jelly by evaporation, whilst that of muriate of barytes dries in crystalline plates: the former, which is very soluble in alcohol, makes it burn with a flame of a beautiful purple; the latter, which is almost insoluble in this liquid, gives it a yellow flame.

The greatest and most remarkable difference which exists between strontian and barytes is, that strontian has less affinity with acids than barytes, and even than the fixed alkalies; so that a solution of barytes precipitates muriate of strontian in white flocks.

Nitrate of strontian differs from that of barytes; 1stly, in giving more nitrous vapour when decomposed by fire, in consequence of its retaining the nitric acid less strongly than barytes; and, 2dly, in being three times as soluble in water.

Strontian obtained from its nitrate by the action of fire is less harsh than barytes; it does not fuse in like manner by the blow-pipe, but glitters with a phosphoric flame; it is almost ten times less soluble in water; and it seems to be less powerful in its attractions than lime, which it does not separate from acids. When precipitated by the oxalic acid, it is not re-dissolved by an excess of this acid, as barytes is; but the sulphate of strontian is on the contrary resolvable by an excess of sulphuric acid, which that of barytes is not.

The aqueous solution of strontian is not precipitated by the gallic acid, whereas that of barytes affords with this acid a greenish precipitate.

The authors of these memoirs conclude from the Experiments (the results of which are here stated) that strontian exhibits more properties which are different from those of barytes than of such as are analogous to them; and that, striking as are the latter, it must nevertheless be concluded, from the whole of the phenomena, that these earths are really two different substances; a conclusion which is more particularly evident in respect of their different degrees of affinity, and of strontian not being a poison, whereas barytes is a very active and violent one.

Several

Several of the properties which they have described had been before observed by Messrs. Klaproth, Hope, and Pelletier; but several also are of their own discovery. We have not here spoken of such of the results afforded by the two earths in their combinations, as were perfectly similar to each other; it having been our principal object to point out those specific characters by which they are to be distinguished.

It is clear that we may reasonably entertain a hope, that more extensive experiments shall one day point out uses both of barytes and strontian, as well as of their combinations, of great importance in respect of the arts, when they shall have been found more abundantly in nature, and when the progress of science shall have perfected the means of obtaining them in a state of purity. The extreme solidity which barytes assumes when flaked, may lead us to presume that it may be of use for making very hard and durable cements, or perhaps even mordants for the application of different matters on stone, glass, pottery and other substances of a like nature.

III.

Extract of a Letter from Count MUSSIN PUSCHKIN, Vice-President of the Department of Mines at Petersburg. On the Salts, Precipitates, and Amalgam of Platina; on Cobalt; on Antimonial Soap; and on the Decomposition of Soap by the Acid Extracts of Colouring Matters.*

I. On the Salts and Precipitates of Platina.

THE brick-coloured salt, which is obtained by pouring muriate of ammoniac into a solution of platina, is totally soluble in water, and deposits, when dissolved in boiling water, a black matter, which M. Fourcroy believes to be iron; but, in my opinion, it seems rather to be plumbago, though I have not yet made the requisite experiments to ascertain this fact. A specimen of this substance accompanies this letter.

In order that the whole of the salt may dissolve, it is necessary to boil it for a long time with water; and when it is perceived that no more of the salt is taken up, the water must be decanted, and fresh water poured into the vessel. An ounce of the salt required upon the whole between eight and nine pounds of water poured on and decanted at five different times. By this means the whole of the salt was not only dissolved, but its colour immediately became a beautiful orange; and by these crystallizations, by evaporation on the water-bath and new solutions, after which the fluid was left to a very slow evaporation, the orange-colour of the crystals became converted into a most brilliant topaz-colour, and by new ebullitions was found no longer to deposit any black matter. I have not yet succeeded in obtaining larger crystals than those which Werner calls *very small*; but, by the assistance of a good English microscope, all those which the topaz-coloured salt afforded were observed to be perfectly transparent, and with scarcely any exception affected the form either of an octahedral pyramid, or that of a polyhedron composed of six perfect hexagons united together by eight isosceles triangles. There were some less regular crystal, which partook of the nature of both these figures.

* Annales de Chimie, XXIV. 205.

The alkalis precipitated (though with difficulty) from the aqueous solutions a small quantity of yellow powder, resembling Naples-yellow. I suppose, however, that this precipitate was composed of crystals, though none could be discerned with the magnifier: and it did not appear to be soluble in water; for the nitrous acid, by remaining upon it for forty-eight hours, did not appear to have affected it in the least. The light yellow precipitate which is obtained by vegetable alkali, after sal-ammoniac has thrown down the whole of the brick-coloured precipitate, was separated by the filter, and submitted without edulcoration to the action of nitrous acid in the proportion of nearly half an ounce of the acid to one dram of the precipitate, in conical vessels, such, for example, as a wine-glass, in which the precipitate occupied the smallest possible space, and the acid covered it a finger's depth, and presented a considerable surface to the air. In three or four days the acid takes up more or less, according to the temperature, and assumes the consistence of a jelly, at first yellow, but afterwards of a chrysolite green more or less consistent, but always in a certain degree moist. Before the blow-pipe this jelly becomes converted into a black substance, probably the imperfect oxide of platina. This experiment appeared surprising to me at first; but I recollected that Margraff, by detonating saltpetre upon platina, and then washing and filtering, had likewise obtained a jelly which may have been of the same nature as mine. I must observe that I satisfied myself in the most convincing manner, that the acid and alkali I made use of were absolutely pure, and that the platina alone, that is to say in the metallic state, did not convert the nitrous acid into a jelly.

Urine, whether fresh or putrid, precipitates platina in the form of a super-compound salt, at the same time that a grey yellow precipitate is formed, which is neither saline nor soluble in water. Part of this precipitate is formed a few moments after the urine is poured into the solution of platina: the salt is formed afterwards. In order to obtain this salt, it is necessary that the solution of platina should be very concentrated, and carry with it an abundant portion of the first precipitate. I have not yet attempted to dissolve this salt in boiling water, but I think it would exhibit a very beautiful topaz-yellow; for some of the crystals, when observed in the microscope, were semi-transparent, and had not the red colour of the salt precipitated by the muriate of ammoniac. With respect to the grey-yellow precipitate, I suppose it to be phosphate of iron contaminated with some foreign substance, and perhaps mixed with a small quantity of the precipitate of platina.

When urine is poured into a solution of the red salt in water, a beautiful lemon-yellow precipitate is formed, and the grey matter afterwards falls down slowly. This may perhaps afford a new method of separating iron from platina, if this grey precipitate should in fact prove to be the phosphate of iron.

II. *On the Amalgam of Platina.*

REFLECTING on the facility with which the super-compound salts of platina are reduced in the fire, I thought that an amalgam of platina might perhaps be obtained by triturating these salts with mercury. To ascertain this, I took a dram of the orange-coloured salt of platina, and triturated it with an equal weight of mercury in a mortar of chalcedony.

In a few minutes the salt lost its colour, becoming at first brown, and afterwards greenish-brown; the matter was reduced to a very fine powder. Another dram of mercury was then added and the trituration continued, when the powder became grey; a third dram of mercury began to form an amalgam, and when the quantity amounted to six drams the amalgam was perfect. The whole operation scarcely employed twenty minutes. I added mercury to the quantity of nine times the weight of the salt, notwithstanding which the amalgam was very tenacious; a fact which is indeed surprising, when it is considered that the salt contains no more than about 40 parts of platina in the hundred. In my Experiment therefore 24 grains of platina were sufficient to give consistence to 540 grains of mercury. This amalgam was easily spread out under the pestle; it perfectly well received the impressions of the most delicate seals; its grain was very close and brilliant, and in no respect inferior to that of the best amalgam of tin.

Though the sight of a perfect amalgam of platina made in a few minutes, the production of which had cost Lewis several weeks, and Sickingen several days, gave me pleasure, I had not less satisfaction in observing the singular phenomenon I shall proceed to describe:

Being desirous of clearing my amalgam of its saline parts by washing, I trituated it in water in a glass mortar, and had scarcely given a few strokes of the pestle before I observed the surface of the amalgam to be covered with a black powder, mixed with some yellow particles. In less than ten minutes the whole of the amalgam disappeared, instead of which I had this black powder. The yellow matter, which consisted of undecomposed salt of platina, having likewise disappeared by trituration; the black powder was seen, upon decanting the water, in extremely brilliant masses (*parcelles*), which probably were platina. The mercury had therefore passed with the greatest facility to the state of black oxide, nearly approaching the metallic state. A portion of running mercury was found beneath, in the quantity of about two drams. Having taken a portion of the amalgam in the palm of my hand, and rubbed it with my finger, the same decomposition took place in a few instants, and left a black powder interspersed with brilliant particles. On the supposition that this oxide would easily pass to the state of cinnabar in the humid way, I poured the sulphate of ammoniac (Bequin's volatile liver of sulphur) upon it, and in less than 24 hours the powder became of a dull red, intermixed with metallic particles, which were evidently platina. I ascertained afterwards by some experiments, that various metallic substances, and all the animal matters which I tried, decompose this amalgam by simple contact. For these reasons it is absolutely necessary to use a glass or siliceous pestle and mortar in the composition of the amalgam.

In what manner are we to explain the speedy reduction of the platina without oxidation of the mercury during the formation of the amalgam, as well as the much more speedy oxidation of the mercury when brought into contact with water, or metallic or animal matters? This appears to me to be a very difficult question, of which I expect the solution from more enlightened chemists. For the small quantity of oxygen in the salt of platina is surely insufficient to deprive so large a portion of mercury of its metallic state; besides which, in the dry trituration, such as that which is made in the hollow of the hand, the acid is not dissolved. Is it likely that there may be a decomposition of atmospheric air, and

a disengagement of caloric from the vital air * ? This must be determined by new experiments, which I have good reason to expect from the illustrious philosopher to whom I have the honour to address myself. I sublimed the oxidized powder of mercury in a small glass retort. It afforded metallic mercury, and the muriate of mercury. The substance which remained at the bottom of the retort was friable, light, and of a colour inclining to a deep grey yellow. The small quantity of the powder of amalgam which I subjected to experiment was not sufficient to admit of an examination of this residue. I shall not fail to repeat the experiment more at large, and acquaint you with the results. In the mean time I request that the contents of this letter may serve as a supplement to the history of platina, and beg that you will give it a place in your Annals under that title.

III. *Concerning Cobalt.*

WITH regard to the experiments I have made on cobalt, I must suspend my information until I shall have been able to procure the regulus of cobalt in a quantity somewhat considerable, for the purpose of repeating my experiments in the large way. Those which I have already made have afforded several interesting phenomena, particularly with regard to the effects of cold as a chemical agent. At present, I shall only remark, that the oxides of cobalt, which have hitherto been supposed, after the methods of the best chemists, to be absolutely pure, are nevertheless contaminated with much foreign matters.

IV. *On the Antimonial Soap.*

PROFESSOR RUDOLPHI has made an antimonial soap, after my method of preparing the mercurial soap. He finds it very active.

V. *On the Decomposition of Soaps by the Acid Extracts of Colouring Matters.*

I HAVE observed, in decomposing soaps, that the acid extracts of colouring matter, such, for example, as that of Brazil wood by the diluted acetous acid, perfectly decompose soaps, whether made with oil or wax. The oleo-colorant or cero-colorant compounds thus obtained are absolutely insoluble in water, and afford extremely brilliant colours, which may perhaps be advantageously used in paintings, particularly encaustic †. It would be likewise interesting

* The author thinks, and with reason, that the oxygene united to the platina is far from being sufficient to oxidize large a quantity of mercury; but he has not perhaps sufficiently attended to the facility with which the least oxidizable metals pass to the state of oxide, when they are rendered fluid, and their parts disengaged by their affinity with mercury. This has long been observed with regard to the amalgam of gold, which soon becomes covered with a purple oxide by mere exposure to the air. Citizen Guyton, in a memoir on certain properties of platina, read to the National Institute the first Messidor, in the year 4, has exhibited a new proof of this principle in the oxidation of an amalgam of platina, obtained by the direct combination of the two metals, by means of an elevated temperature, and by the simple effect of the affinity which was announced by the rank which platina occupies in the scale of adhesions to mercury. *Note of the Editors of the Annales.*

† Citizen Chausser has already communicated, in his lessons to the Polytechnic School, several experiments of this kind, which have perfectly succeeded. The soap of copper afforded a very brilliant green, which it is presume

interesting to ascertain precisely whether the colouring matter has not more analogy with earthen than has hitherto been thought, and whether it would not be advantageous, particularly in red dyes, to employ the soaps of tin and of cochineal.

IV.

Observations on the Fundamental Property of the Lever; with a Proof of the Principle assumed by Archimedes in his Demonstration. By the Rev. S. VINCE, A.M. F.R.S.*

THE want of a demonstration of the property of the lever upon clear and self-evident principles has justly been considered as a great desideratum (defect) in the science of mechanics, as the most important parts of that branch of natural philosophy are founded upon it. Archimedes was, I believe, the first who attempted it. He supposes, that if two equal bodies be placed upon a lever, their effect to turn it about any point is the same as if they were placed in the middle point between them. This proposition is by no means self-evident, and therefore the investigation which is founded upon it has been rejected as imperfect. Huygens observes, that some mathematicians, not satisfied with the principle here taken for granted, have, by altering the form of the demonstration, endeavoured to render its defects less sensible, but without success. He then attempts a demonstration of his own, in which he takes for granted, that if the same weight be removed to a greater distance from the fulcrum, the effect to turn about the lever will be greater: this is a principle by no means to be admitted, when we are supposed to be totally ignorant of the effects of weights upon a lever at different distances from the fulcrum. Moreover, if it were self-evident, his demonstration only holds when the lengths of the arms are commensurable. Sir I. Newton has given a demonstration, in which it is supposed, that if a given weight act in any direction, and any radii be drawn from the fulcrum to the line of direction, the effect to turn the lever will be the same, on whichever of the radii it acts. But some of the most eminent mathematicians since his time have objected to this principle, as being far from self-evident; and, in consequence thereof, have attempted to demonstrate the proposition upon more clear and satisfactory principles. The demonstration by M^r Laurin, as far as it goes, is certainly very satisfactory; but as he collects the truth of the proposition only from induction, and has not extended it to the case where the arms are incommensurable, his demonstration is imperfect. The demonstration given by Dr. Hamilton, in his *Essays*, depends upon this proposition, that when a body is at rest, and acted upon by three forces, they will be as the three sides of a triangle parallel to the directions of the forces. Now this is true when the three forces act at any point of a body; whereas, considering the lever as the body, the three forces act at different points, and

assumed was known to the ancient masters. On this subject, consult the 3d cahier of the *Journal de l'Ecole Polytechnique*, p. 426 and 427.

* *Phil. Trans.* M.DCC.XCIV. 33.

therefore the principle, as applied by the author, is certainly not applicable. If in this demonstration we suppose a plane body, in which the three forces act, instead of simply a lever, then the three forces being actually directed to the same point of the body, the body would be at rest. But in reasoning from this to the case of the lever, the same difficulties would arise as in the proof of Sir I. Newton. But admitting that all other objections could be removed, the demonstration fails when any two of the forces are parallel. Another demonstration is founded upon this principle, that if two non-elastic bodies meet with equal quantities of motion, they will, after impact, continue at rest; and hence it is concluded, that if a lever which is in equilibrio be put in motion, the motions of the two bodies must be equal; and therefore the pressures of these bodies upon the lever at rest to put it in motion, must be as their motions. Now in the first place, this is comparing the effects of pressure and motion, the relation of the measures of which, or whether they admit of any relation, we are totally unacquainted with. Moreover, they act under very different circumstances; for in the former case the bodies acted immediately on each other, and in the latter they act by means of a lever, the properties of which we are supposed to be ignorant of. When forces act on a body considered as a point, or directly against the same point of any body, we only estimate the effect of these forces to move the body out of its place, and no rotatory motion is either generated, or any causes to produce it considered in the investigation. When we therefore apply the same proposition to investigate the effect of forces to generate a rotatory motion, we manifestly apply it to a case which is not contained in it, nor to which there is a single principle in the proposition applicable. The demonstration given by Mr. Landen, in his Memoirs, is founded upon self-evident principles; nor do I see any objections to his reasoning upon them. But as his investigation consists of several cases, and is besides very long and tedious, something more simple is still much to be wished for, proper to be introduced in an elementary treatise of mechanics, so as not to perplex the young student, either by the length of the demonstration or want of evidence in its principles. What I here propose to offer will, I hope, render the whole business not only very simple, but also perfectly satisfactory.

The demonstration given by Archimedes would be very satisfactory and elegant, provided the principle on which it is founded could be clearly proved, viz. that *two equal powers at the extremities, or their sum at the middle, of a lever, would have equal effects to move about any point*. Now, that the effects will be the same, so far as respects any progressive motion being communicated to the lever when at liberty to move freely, is sufficiently clear; but there is no evidence whatever that the effects will be the same to give the lever a rotatory motion about any point, because a very different motion is there produced, and we are supposed to know nothing about the efficacy of a force at different distances from the fulcrum to produce such a motion. Besides, the two motions are not only different, but the same forces are known to produce different effects in the two cases; so that in the former case the two equal powers, as the extremities of the arms, produce equal effects in generating a progressive motion; but in the latter case, they do not produce equal effects in generating a rotatory motion. We cannot, therefore, reason from one to the other. The principle, however, may be thus proved:

Let

Let A C, Pl. XXII. Fig. 1. be two equal bodies on a straight lever, A P moveable about P. Bisection A C in B; produce P A to Q and take B Q = B P, and suppose the end Q to be sustained by a prop. Then as A and C are similarly situated in respect to each end of the lever, that is, A P = C Q, and A Q = C P, the prop and fulcrum must bear equal parts of the whole weight; and therefore the prop at Q will be pressed with a weight equal to A. Now take away the weights A and C, and put a weight at B equal to their sum; and then the weight at B being equally distant from Q and P, the prop and fulcrum must sustain equal parts of the whole weight, and therefore the prop will now also sustain a weight equal to A. Hence, if the prop Q be taken away, the moving force to turn the lever about P in both cases must evidently be the same; therefore the effects of A and C upon the lever to turn it about any point, are the same as when they are both placed in the middle point between them. And the same is manifestly true if A and C be placed without the fulcrum and prop. If therefore A C be a cylindrical lever of uniform density, its effect to turn itself about any point will be the same as if the whole were collected into the middle point B; which follows from what has been already proved, by conceiving the whole cylinder to be divided into an infinite number of laminæ perpendicular to its axis of equal thicknesses.

The principle therefore assumed by Archimedes is thus established upon the most self-evident principles, that is, that equal bodies at equal distances must produce equal effects; which is manifest from this consideration, that when all the circumstances in the cause are equal, the effects must be equal. Thus the whole demonstration of Archimedes is rendered perfectly complete, and at the same time it is very short and simple. The other part of the demonstration we shall here insert for the use of those who may not be acquainted with it.

Let X Y, Fig. 2. be a cylinder, which bisection in A, on which point it would manifestly rest. Take any point Z, and bisection Z X in B, and Z Y in C; then, from what has been proved, the effects of the two parts Z X, Z Y, to turn the lever about A, are the same as if the weight of each part were collected into B and C respectively, which weights are manifestly as Z X, Z Y, and which therefore conceive to be placed at B and C. Now A B = A X — X B = $\frac{1}{2}$ X Y — $\frac{1}{2}$ X Z = $\frac{1}{2}$ Y Z; and A C = A Y — Y C = $\frac{1}{2}$ X Y — $\frac{1}{2}$ Z Y = $\frac{1}{2}$ X Z; consequently A B : A C :: $\frac{1}{2}$ Y Z : $\frac{1}{2}$ X Z :: Y Z : X Z :: the weight at C : the weight at B.

The property of the straight lever being thus established, every thing relative to the bent lever immediately follows.

V.

Observations on the Acid of Tin, and the Analysis of its Ores. Read at the Sitting of the Class of Mathematical and Philosophical Sciences of the National Institute of France, the first of Messidor, in the Year 5. By Citizen GUTTON.*

IT has long been observed that the nitric acid calcines tin instead of dissolving it, and the phlogistic hypothesis was incapable of explaining this phenomenon. It remained among

* Inserted in the XXIVth volume of the *Annales de Chimie*.

the number of facts for which the most fruitful imagination could not afford a probable solution. This difficulty has vanished since it has been discovered that the metals are capable of being converted into acids by a sufficient quantity of oxygene. Tin is one of the metals which must have most effectually contributed to fix the attention of chemists upon this combination. In this manner it is seen that the precipitation of gold by tin in the form of a purple oxide is simply, as I have announced in the *Elements of Chemistry* of the Academy of Dijon, the result of the action of the stannic acid upon gold. This oxigenation of tin is much more evidently seen in the operation which I have described in the first volume of the *Chemical Dictionary* of the *Encyclopedia*, p. 632; an operation which seems well calculated to exhibit at the same instant every fundamental principle of the pneumatic chemistry. For when the diluted nitric acid and tin are treated simply in the retort, the result is oxide of tin and nitrate of ammoniac, the latter of which is formed by the azote or basis of the acid, and the hydrogen of the water, without the appearance of nitrous gas at any period of the operation.

No further difficulty remained therefore with respect to the oxidation of tin by the nitric acid, as it is evidently effected by the decomposition of the acid and the affinity of the tin for oxygene. But no account has yet been given of the state in which this metal exists in its ores, particularly in the crystals of tin (*zinn-graupen*, *zinnstein*, of the Germans), nor of the causes which oppose their solution in acids.

This question presented itself to my consideration on reading the publication of Mr. Klaproth on the ores of tin in the second volume of *Analyses* with which he has enriched chemistry and mineralogy.

He begins by comparing the products of the same ore treated with alkaline reducing fluxes, and with charcoal alone. The difference convinced him, that in the first process the alkali retains a considerable portion of the metal in the state of oxide. He afterwards endeavoured to complete the analysis in the humid way. For this purpose he followed Bergman's process, which consists in digesting the powdered ore in concentrated sulphuric acid; adding muriatic acid after the cooling, and precipitating the solution by soda. By this treatment 131 parts of precipitate ought to be afforded for every 100 parts of metal contained in the ore. Mr. Klaproth, in fact, obtained a solution of about 0,19 of that rather scarce tin ore which the English call wood-tin; but he could not succeed in charging the muriatic acid with any perceptible quantity of tin, by subjecting the other more common varieties to the same treatment.

Reflecting on the nature of the obstacle which might probably oppose the action of the muriatic acid on a substance which he estimated to contain 0,99 of oxide of tin, he imagined that it could be nothing but an excess of oxygene.

To remove this excess, he first attempted to treat the ore in the retort with a mixture of sulphur. Most of the sulphur was sublimed, and the residue, slightly agglutinated, had still the same grey whitish appearance as after the pulverization, excepting that some brilliant gold-coloured specks were seen; namely, sulphate of tin, or *aurum-musivum*. The mineral, however, was not more soluble in the muriatic acid than before.

After many other trials equally unsuccessful, the celebrated chemist of Berlin had recourse to that substance which in his hands has become so powerful an instrument of analysis.

lysis. He treated his ores with six parts of potash in a crucible of silver, and the success was beyond his expectations. By the first operation, 0,91 were rendered soluble in water, precipitated and afterwards taken up by the muriatic acid. The muriate of tin being then decomposed by carbonate of soda, a pure oxide was obtained, which was easily re dissolved in the same acid, and precipitated by zinc. This precipitate, fused in a crucible with tallow, afforded a button of the same weight as had been obtained from the same kind of ore in the dry way.

By this means he was authorized to conclude as follows, with regard to the brown ore of Schiackwald;

In 100 parts	{	Tin	-	-	-	75
		Iron	-	-	-	0,5
		Silex	-	-	-	0,75
		Oxygene	-	-	-	23,75
						100

I have treated in the same manner a crystal of tin ore of this kind, also from Schiackwald; not because I had any doubt of the success announced by a chemist who has so long been in possession of well-earned confidence; but this success seemed to be a confirmation of the conjecture on which Mr. Klaproth had founded his experiments, and I supposed that there was good reason to doubt whether the complete saturation or supersaturation of tin by oxygene were the true cause of its insolubility in the muriatic acid. For I could not at any period of the operation discern either the substance which might seize this excess of oxygene, or any trace of the phenomena which must have accompanied its disengagement.

In order to observe the circumstances with more facility, I operated in a small crucible of platina over a reverberatory lamp of Argand (sur une lampe d'Argand à réchauf*). Fifty-five centigrammes of the brown crystals of tin, reduced into fine powder, were well mixed with six times the quantity of potash purified by alcohol, and dried. The mixture was moistened with a few drops of water, then evaporated to dryness, and afterwards heated to the commencement of fusion. After the first operation, hot water was poured on the mass, and took up more than half the mineral. Muriatic acid first precipitated it, and afterwards dissolved it with the greatest facility. The precipitate or metallic oxide, reproduced by carbonate of potash, was in fact completely soluble by the same acid, as Mr. Klaproth had affirmed.

But after having thus witnessed the facts, my first doubt concerning the direct solubility of this ore was rather strengthened than removed. For it cannot be affirmed that the excess of oxygene was dissipated during the fusion with potash, because the metal could not form a soluble combination with the alkali, but in consequence of its oxidation to the last degree, or, to speak more correctly, its acidification; so that the lixivium of the filtered residue is a true stannate, or perhaps a stannic $\frac{1}{2}$, of tin (potash).

* The author intends soon to give a description of this lamp.

† Mr. Hatchett in his paper on the molybdate of lead and the molybdic acid, inserted in the Philosophical Transactions of London for 1796, compares precisely the action of the nitric acid on molybdena, to superoxygene it, to that which produces the same effect on tin. It may conversely be inferred that tin is acidified like molybdena. G.

In the mean time, as we are forced to admit that all the oxygene of the ore is found in the alkaline solution, the more effectual action of the acid upon the metal cannot be supposed to arise from the loss of a portion of this principle, because it continues still in the same state of saturation; neither is there any sign of oxygenated muriatic acid gas; and it would likewise be difficult to conceive, why in this circumstance there should rather be a disengagement of the gas than when the acid is digested on the ore, as is observed in the oxides of manganese and of lead.

To establish this point of theory on a more decisive experiment, I dissolved six grammes of tin in the nitric acid, and evaporated several times to dryness the new acid which was successively poured on. I suppose it cannot be doubted but that the tin in this state had combined with as much oxygene as it is capable of fixing. Nevertheless, the mass of white oxide, washed until the water which came off caused no further change in vegetable colours, was very soluble in muriatic acid.

What therefore is the cause of the insolubility of the ore, which consists also of tin and oxygene with scarce one hundredth part of foreign matter? This cause is to be found only in the state of aggregation of the latter. This assertion cannot be thought strange, except on account of the little regard which has hitherto been paid to its energy. If combinations be the result of affinity or elective attraction, is not this attraction itself a power which may be rendered ineffectual by the sum of the forces which act in the contrary direction? These truths surely cannot appear repulsive to Mr. Klaproth, who has rendered them so evident, by shewing that the ruby, the sapphire and the adamantine spar, of which the elements are in their own nature so easily soluble, do not resist the ordinary methods of analysis but in consequence of the aggregation of their integral parts.

VI.

On Fairy Rings.

THE appearance in the grass, commonly called Fairy Rings, is well known. It consists either of a ring of grass of more luxuriant vegetation than the rest, or a kind of circular path in which the vegetation is more defective than elsewhere. It appears to be pretty well ascertained, that the latter state precedes the former. Two causes are assigned for this phenomenon: the one, which cannot be controverted, is the running of a fungus; the other, which has been considered as an effusion of theory, is grounded on a supposition that the explosion of lightning may produce effects of the same kind on the ground as Dr. Priestley's battery was found to produce on the polished surface of a plate of metal, that is to say, a series of concentric rings. Some observations, which I find in my common-place book, appear to shew that this last effect may, in certain circumstances, take place.

On Tuesday the 19th of June, 1781, a very powerful thunder-storm passed over the western extremity of London. I was then at Battersea, and made no other remark on the phenomena than that the explosions, which were very marked and distinct, were in many instances forked at the lower end, but never at the top; whence it follows, that the clouds were in the positive state for the most part. On the following Sunday, namely the 24th,

I happened

I happened to be in Kensington Gardens, in every part of which extensive piece of ground the lightning had left some marks of its agency, chiefly by discoloration of the grass in zigzag streaks, some of which were fifty or sixty yards in length. Instances of this superficial course of the lightning along the ground before it enters the earth are sufficiently frequent. But the circumstance which attracted my attention the most was seen in a small grove of trees at the angular point of one of the walks. Plate XXII. figure 3. exhibits a sketch of part of the gardens, in which the angular point is denoted by the letter A. Figure 4. represents the position of the trees. The numbers express the distances between the trees in feet.

Close to the stem of the tree A was a hole in the ground four inches long and two wide, proceeding to the southward; and at two feet further to the south was another similar hole. Between these two holes the ground was torn away. This appearance is represented in figure 3, in which the letter A represents the trunk of the tree. The grass was very much scorched to the distance of about three feet in every direction from the trunk, and in this burned space there were several other smaller holes.

Close to the trunk of B, on the south side, there was a hole in the ground.

Near the tree C there was also a hole in the ground, and it was surrounded with a faint ring of burnt grass at a little distance; but as the grass was grown again, it seemed probable, that the ring was occasioned by some earlier stroke than that on Tuesday.

The tree D was surrounded by a ring of 6 feet radius, and 18 inches broad. Within the ring the grass was fresh; but on the surface of the ring the grass and the ground were much burned. To the eastward of the tree upon the ring itself were two holes, in which the ground had the appearance of ashes. The tree E had half a faint ring to the westward.

The tree F was surrounded by a faint ring of two yards radius. Within the ring the grass was unhurt. To the westward, at about three feet distance from the inner ring, was part of another similar ring, of much the same appearance; the verdure being unhurt in the interval between the rings.

I imagined the leaves of the trees were a little curled, but could observe no blasted boughs; a circumstance which, together with the other facts, appears to indicate that these appearances were produced by the recent storm of the 19th.

VII.

Experimental Researches to ascertain the Nature of the Process by which the Eye adapts itself to produce distinct Vision.

[Concluded from page 479.]

IN the eel there is a transparent horny convex covering at some distance before the eye, to defend it from external accidents. This covering to an eye fitted to see in air, would entirely take off the effects arising from change of figure in the cornea; but in water, where no such change could be attended with advantage, such a covering is employed as an external defence.

In the eyes of fishes the ciliary processes are entirely wanting. The crystalline lens is spherical and imbedded in the vitreous humour, which is inclosed in cells of a stronger texture than in other animals.

The iris does not admit of motion; this is taken notice of by Haller; and the reason probably is, that the light in water is never too strong for the eye to bear.

There is a muscle situated between the retina and the sclerotic coat, which is, as Mr. Home thinks, common to all fish. This muscle is particularly described by Mr. Haller; and its use is stated to be that of bringing the retina nearer the crystalline lens for the purpose of seeing objects at a greater distance. Mr. Hunter called it the choroidé muscle, and has preserved several preparations of it.

This muscle has a tendinous centre round the optic nerve, at which part it is attached to the sclerotic coat; the muscular fibres are short, and go off from the central tendon in all directions; the shape of the muscle is nearly that of a horse-shoe; anteriorly it is attached to the choroide coats, and by means of that to the sclerotic. Its action tends evidently to bring the retina forwards; and in general the optic nerve in fishes makes a bend where it enters the eye, to admit of this motion without the nerve being stretched.

In those fishes that have the sclerotic coat completely covered with bone, the whole adjustment to great distances must be produced by the action of the choroide muscle; but in the others, which are by far the greater number, this effect will be much assisted by the action of the strait muscles pulling the eye-ball against the socket, and compressing the posterior part, which, as it is the only membranous part in many fishes, would appear to be formed so for that purpose.

In fishes, the eye in its natural easy state appears to be adjusted to near objects, requiring some change to adapt it to see distant ones; in this respect differing entirely from the bird, the quadruped, and the human.

The preceding observations, on the structure of the eye, indicate two methods of adjusting the eye; one for seeing in air, the other for seeing in water; and that the crystalline lens, as the most conspicuous part, appears to have engrossed the whole attention of former enquirers, who in general do not seem to have paid sufficient attention to the elongation of the axis and change of curvature in the cornea. That the axis of vision is really lengthened, and the lens moved forward, appears highly probable from the whole of the facts; and since the elongation of the eye, the change of position of the crystalline, or the alteration of curvature in the cornea, have severally appeared to be the cause of the requisite adjustment, the combination of all three must undoubtedly be sufficient to effect the purpose.

In the Philosophical Transactions for 1797, page 1, Mr. Home explains certain morbid affections of the strait muscles and cornea of the eye, and considers their treatment. He states the uses of the muscles to be; first, that of moving the eye-balls in different directions; secondly, that of causing the two eyes to correspond; and thirdly, that of producing adjustment by their lateral pressure. Imperfection in any one of these different actions must be considered as a disease. Three different cases occur in practice: namely, indistinct vision, double vision, and squinting.

Indistinct vision is explained to consist of an inability in the muscles to support that degree of tension which is requisite to adjust the eye to near objects. In this morbid affection of the strait muscles they may be capable of performing all the intermediate contractions

as usual, but not the extreme degrees without considerable pain. As these symptoms have not been accounted for before in this way, the author adduces some curious instances of similar affections of other muscles, and concludes that bad effects must necessarily arise from every thing which irritates or weakens the parts themselves, or the general habit; and that such means ought to be adopted as may soothe the parts in their sensations, and quiet and strengthen their actions, since in that way only the muscular fibres can possibly recover their tone.

On the subject of double vision the author remarks, that many opinions have been advanced to account for the single appearance of objects contemplated by both eyes. The opinion of Dr. Reid of Glasgow, that vision is single when the impressions are made on correspondent parts of the retina in the two eyes, and double when this is not the case, appears to him to be strongly confirmed by the facts. This want of correspondence may be produced by some change in the refracting media of one of the eyes, or else by a want of similar actions in the muscles of both eyes respectively. The former takes place after the crystalline lens of one or both eyes has been extracted, and the convex lens made use of, to produce the requisite focal adjustment is not duly placed. The latter is more particularly the object of medical treatment. Mr. Home advises repose, that is to say, that the disordered eye should be covered for a time. For, as he remarks, the first object of attention, with regard to strained or over-fatigued muscles, must be to put them into an easy state, and confine them from motion; and this practice is no less applicable to the muscles of the eye than those of other parts.

As double vision is produced by a moderate derangement of the optic axes, squinting is effected by a much greater derangement. In the case of squinting, the author proves that the object is not seen by both eyes; but that one eye, more or less perfect, is directed to the object, while the other, which in such cases is imperfect, is drawn aside by habit, in order that its operation may not disturb the perception received by the other eye. The greater strength, shortness and straitness of the adductor muscle causes the deviation to be made towards the nose. These doctrines are illustrated by apposite cases. Squinting takes place in three different circumstances; that is to say, where one eye has only an indistinct vision; where both eyes are capable of seeing objects, but the one is less perfect than the other; and where the muscles of one eye have, from practice, as in the case of frequently looking through telescopes, acquired a power of moving it independently of the other.

When squinting arises from absolute imperfection in the eye, there is no cure. Where it arises from weakness only in the sight of one eye, it may in some instances be got the better of by confining the person to the use of one eye by covering the other; in this way the muscles, from constant use, become perfect in the habit of directing the organ, and acquire strength and power of adjustment. The time required must depend upon the weakness of the sight, and length of time during which the muscles have been left to themselves.

The Author shews, that the cornea is not, like the cuticle, devoid of life; though, like tendons and ligaments, it is neither supplied with red blood nor possesses sensibility, but is made up of membranous ligaments, which are continuations of the tunica conjunctiva and the tendons of the four strait muscles. When wounded, it commonly unites, like other

living parts, by the first intention; and in some cases with inflammation exceeding the limits of adhesion; and the whole internal cavity of the eye proceeds to a state of suppuration. These stages of inflammation are only met with in parts possessed of life.

From the opinion of the cornea being void of life, the opacities which are found to take place on it have been supposed to arise from inanimate matter laid over its surface. And under that notion acrid and irritating applications have been used to remove it, such as powdered glass, powdered sugar, &c. Such applications being of service, have confirmed the opinion.

Dr. Home considers the cornea as a ligamentous part, and, as such, weak in its vital powers. This arises from such parts having no vessels carrying red blood. When they inflame, which is a state of increased action, they therefore require a different mode of treatment from the other parts of the body, whose vital powers are strong in consequence of being largely supplied with red blood.

The truly healthy inflammation requires an increased action in the parts affected; and if this, either from weakness or indolence, is not kept up, the inflammation does not go rapidly through its stages, but remains in a state between resolution and suppuration. In ligamentous structures the actions must therefore be roused and supported, when under inflammation, to promote resolution, and to prevent the parts from falling into an indolent diseased state. This is however attended with difficulty, and they too often become considerably thickened by a decomposition of coagulating lymph during the adhesive state of inflammation, which in the cornea renders it opaque. The thickening of the parts remains after the inflammation is gone, and can only be removed by absorption, which is best effected by the application of very stimulating medicines.

He extends these principles to all ligamentous structures. These require a peculiar treatment, which may be illustrated both in inflammations of the joints, and of the cornea of the eye; the applications made use of with the greatest advantage in both cases being of a very stimulating kind.

The instance of the cure of an opacity of the cornea in the eyes of Tobit by the gall of a fish, as related in the apocryphal book which bears his name; with other circumstances of like practice among the Arabians; and also certain facts and observations which occurred in his own practice, are related as proofs of the advantage of stimulating applications to the cornea. In some old cases of opacity, he found gall the best application. He used it pure, and also diluted. The gall of quadrupeds, in these trials, gave more pain than the gall of fish. The painful sensation was very severe for an hour or two, and then went off, and the beneficial effects appeared to be in proportion to the local violence at the time of its application. The object of his observations having been to explain the principle upon which those effects depend, this knowledge may, as he observes, regulate the practice suitable to be adopted in such cases. It will shew the impropriety of using such medicines while the inflammatory action is increasing, and will point out their adoption the moment the inflammation appears to be at a stand, instead of postponing these remedies till an indolent unhealthy state takes place, which too often terminates in opacities not to be afterwards removed by any application.

VIII.

Experiments and Observations on the Inflexion, Reflexion, and Colours of Light.

By HENRY BROUGHAM, Jun. Esq.*

IT has always appeared wonderful to me, since nature seems to delight in those close analogies which enable her to preserve simplicity, and even uniformity in variety, that there should be no dispositions in the parts of light with respect to inflexion and reflexion analogous or similar to their different refrangibility. In order to ascertain the existence of such properties, I began a course of Experiments and Observations, a short account of which forms the substance of this paper. For the sake of perspicuity I shall begin with the analytical branch of the subject, comprehending my observations under two parts: flexion, or the bending of the rays in their passage by bodies, and reflexion. And I shall conclude by applying the principles there established to the explanation of phenomena, in the way of synthesis.

As in every experimental enquiry much depends on the attention paid to the minutest circumstances, in justice to myself I ought to mention that each experiment was set down as particularly as possible, immediately after it was made; that they were all repeated every favourable day for nearly a year, and before various persons; and as any thing like a preconceived opinion with respect to matter of theory that is in dispute, will, it is more than probable, influence us in the manner of drawing our conclusions, and even in the manner of recording the experiments that lead to these, I have endeavoured, as much as possible, to keep in view the saying of the Brahmin, "that he who obstinately adheres to any set of opinions, may bring himself at last to believe that the fresh sandal-wood is a flame of fire."

PART I. *Of Flexion.*

IN order to fix our ideas on a subject which has never been treated of with mathematical precision, we shall suppose, for the present, that all the parts of light are equally acted upon in their passage by bodies, and deduce several of the most important propositions which occur, without mentioning the demonstrations.

Def. 1. If a ray passes within a certain distance of any body, it is bent inwards; this we shall call inflexion. 2. If it passes at a still greater distance, it is turned away; this may be termed deflexion. 3. The angle of inflexion is that which the inflected ray makes with the line drawn parallel to the edge of the inflecting body; and the angle of incidence is that made by the ray before inflexion, at the point where it meets the parallel. And so of the angle of deflexion.

Proposition I. The force by which bodies inflect and deflect the rays, acts in lines perpendicular to their surfaces.

Prop. II. The sines of inflexion and deflexion are each of them to the sine of incidence in a given ratio—(and what this ratio is, we shall afterwards shew).

Prop. III. The bending force is to the propelling force of light as the sine of the difference between the angles of inflexion (or deflexion) and incidence to the cosine of the angle of inflexion or deflexion).

* Philosophical Transactions, M.DCCXCVI. 1.

† Asiatic Researches, vol. i. p. 224.

Prop. IV. The rays of light may be made to revolve round a centre in a spiral orbit.

Prop. V. If the inflecting surface *bc* be of considerable extent, and a plane, then the curve described may be found by help of the *Prop. XLII. Book I. of Principia*; provided only the proportion of the force to the distance be given. Thus, if the bending force be inversely as the distance, the curve cannot be found; for, in order to obtain its equation, a curvilinear area must be squared, which, in this case, is a conic hyperbola; the relation, however, between its ordinates and abscissæ may be obtained in fluxions, thus: $y \dot{y} + b \dot{y} = a^2 \dot{z}^2$.

If the force (which is most probable) be inversely as the square of the distance, the curve to be squared is the cubic hyperbola; species *LXV. genus III. of Newton's Enumeration*; and this being quadrable, the curve described by the light will be the *parabola campaniformis pura*; Species *LXIX. of Newton*.

If the force be inversely as the cube of the distance, the curve is a circular arch, and that of deflexion is a conic hyperbola*. If the inflecting body be a globe or cylinder, and the force be inversely as the square of the distance from the surface; then, by *Prop. LXXI. Book I. of Principia*, the attraction to the centre is inversely as the square of the distance from that centre; and therefore, by *Prop. XI. and XIII. of the same Book*, the ray moves in an ellipse by the inflecting, and in an hyperbola by the deflecting force; each having one focus in the centre of the body. The truth of these things mathematicians will easily determine.

Prop. VI. If a ray fall on a specular surface, it will be bent before incidence into a curve having two points of contrary flexure, and then will be bent back the contrary way into an equal and similar curve, as in *Fig. 1. Pl. XXIII.*

Corollary to these propositions. If a pencil of rays fall converging on an interposed body, the shadow will be less than the body by twice the sine of inflexion.

And if a pencil fall diverging on the body, the shadow will be greater than the body by twice the sine of inflexion; but less than it should be, if the rays had passed without bending, by twice the sine of the difference between the angles of inflection and incidence. The sine or angle of incidence is greater than the sine or angle of inflexion, when the incident rays make an acute angle with the body; but, when they make an obtuse or right angle, then the sine or angle of inflexion is less than that of incidence. The sine of incidence is greater than that of deflexion, if the angle made by the incident ray with the body is obtuse; but less, if that angle be acute or right. If a globe or circle be held in a beam of light, the rays may be made to converge to a focus.

Hitherto it has been supposed that the parts of which light consists have all the same disposition to be acted upon by bodies which inflect and deflect them; but we shall now see that this is by no means the case.

Observation I. Into my darkened chamber I let a beam of the sun's light through a hole in a metal plate (fixed in the window-shut) of $\frac{1}{10}$ of an inch diameter; and all other light being absorbed by black cloth hung before the window and in the room, at the hole I placed a prism of glass, whose refracting angle was 45 degrees, and which was covered all over with black paper, except a small part on each side, which was free from impurities, and through which the light was refracted, so as to form a distinct and tolerably homogeneous spectrum on a chart at six feet from the window. In the rays, at two feet from the

* *Principia, Lib. i. Prop. 8.*

prism, I placed a black unpolished pin (whose diameter was everywhere one tenth of an inch) parallel to the chart and in a vertical position. Its shadow was formed in the spectrum on the chart, and had a considerable penumbra, especially in the brightest red, for it was by no means of the same thickness in all its parts; that in violet was broadest and most distinct; that in the red narrowest and most confused; and that in the intermediate colours was of an intermediate thickness and degree of distinctness. It was not bounded by straight but by curvilinear sides, convex towards the axis, to which they approached as to an asymptote, and that nearest in the least refrangible rays, as is represented in Fig. 2, where A B is the axis, I K L M N A, and H G F E D A the two outlines. Nor could this be owing to any irregularity in the pin, for the same thing happened in all sorts of bodies that were used; and also if the prism was moved on its axis, so that the colours might ascend and descend on these bodies; still, wherever the red fell, it made the least, and the violet the greatest shadow.

Obs. II. In the place of the pin I fixed a screen, having in it a large hole, on which was a brass plate pierced with a small hole $\frac{1}{4}$ of an inch in diameter. Then causing an assistant to move the prism slowly on its axis, I observed the round image made by the different rays passing through the hole to the chart; that made by the red was greatest, by the violet least, and by the intermediate rays, of an intermediate size. Also, when at the back of the hole, I held a sharp blade of a knife, so as to produce the fringes mentioned by Grimaldo and Newton; those fringes in the red were broadest and most moved inwards towards the shadow, and most dilated when the knife was moved over the hole, and the hole itself on the chart was more dilated during the motion when illuminated by the red, than when illuminated by any other of the rays, and least of all when illuminated by the violet. Now in Observation I. the angle of incidence of the red rays was equal to that of the violet and all the rest, and yet the angle of inflexion was greatest and least in the violet; and indeed the difference between the two was greater than appears at first from the experiment; for that part of the shadow which was formed by the violet, fell at a greater distance from the point of incidence than did that part which was formed by the red, from the divergence of the different rays upwards by the refraction, as appears in Fig. 3, where D E is the window, F G the beam propagated through the hole F, refracted by the prism K I H and pointing on the chart O P q s; the spectrum v r, being separated in L r, the red rays incident on the pin C D at C; and M v, the violet incident at D; the shadow of D C being formed in v r, so that v being farther from D, than r is from C, therefore (by the propositions formerly laid down) the shadow in v should be considerably less than in r, if the rays were equally inflected. Lastly, in *Obs. II.* the angle of the red's incidence was nearly equal to that of the violet's, by the motion of the prism, and the consequent motion of the colours; only that if there was any difference it was on the side of the violet: and yet the violet was least inflected, and the red most inflected; and so of the second inflexion by the knife-blade: wherefore I conclude that the rays of the sun's light differ in degree of inflexibility, and that those which are least refrangible are most inflexible.

Obs. III. My room being darkened as before, and a conical beam propagated through the small hole in the window shut; at this hole I placed a hollow prism made of broken plates of mirror, and of such an angle that, when filled with distilled water, it cast a spec-

trum on an horizontal table, and was there received on a chart seven feet from the window; I then placed on the same table, and in the rays, between the chart and the prism, at three inches from the chart, two sharp knife-blades, with even edges, and fixed to a board with wax, so as to make an angle with one another; moving them nearer and nearer, till I saw the fringes appear in the red light on the chart, and then in the orange and other colours successively. I then withdrew one, and the fringes became faint and narrow, and not all within the shadow of the remaining knife, but at its edge, and even in the light of the spectrum. Lastly, when I slowly approached the other, they moved into the shadow, and became broader and farther separated one from another; there being the like fringes in both shadows. This I repeated in all the rays, and plainly saw, that at the approach of the knife the fringes became broader and farther removed from one another, and from the light, in the red than in the violet, or any of the other rays.

Obs. IV. In repeating the foregoing Experiment, I observed a very curious phenomenon. When the angle of the knife-blades was so held in any of the rays, as to make the hyperbolic fringes described by Newton*, and these being always of the colour in which they were held; moving the angle a little, so as to make the fringes out of the light that went to the top of any one division of the spectrum, and also out of that which went near the bottom of the next, the fringes were made of two colours; one part was of the highest colour, and the other of the lowest; but both were on the ground of the highest. Thus, if held on the confine of the green and blue, the upper half of each fringe was blue, the under green, but both parts in the blue division of the spectrum; and trying the same in all the rays, it was evident that the red was moved farther into the orange, and the orange into the yellow, than the blue was into the indigo, or the indigo into the violet. Now in Obs. III. the fringes were formed by the inflexion of one knife, and were moved into its shadow, and separated and dilated by the deflexion of the other; and this most in the red, and least in the violet. Likewise in Obs. IV. the fringes of one colour were deflected into the region of the next, and this most in the red, and least in the violet; although in both observations the violet, from the position of the chart, was farthest from the angle, and consequently, had the rays been equally deflected, the violet should have been farthest moved, and most dilated by the deflexion: but, seeing that, at equal angles of incidence in the third, and at less in the fourth Observation, the red was most, and the violet less, deflected; it is evident that the most inflexible rays are also most deflexible.

Having thus found that the parts of light differ in flexibility, I wished next to learn two things; in what proportion the angle of inflexion is to that of deflexion at equal incidences; and secondly, what proportion the different flexibilities of the different rays bear to one another. But the nature of the coloured fringes must first be understood; so that I defer this enquiry till after I have made use of the principles now laid down for the explanation of natural phenomena, and proceed in the mean time to

PART II.—Of Reflexion.

THAT bodies reflect light by a repulsive power extending to some distance from their surfaces, has never been denied since the time of Sir Isaac Newton†. Now this power ex-

* Optics, Book iii. Obs. 2.

† Ibid. Book iii. Part III. Prop. 8.

tends to a distance much greater than that of apparent contact, at which an attraction again begins still at a distance, though less than that at which before there was a repulsion; as will appear by the following demonstration, which occurs to me, and which is general with respect to the theory of Boolevich*. In Fig. 4, let the body A have for P an attraction which at the distance of A P is proportional to P M; then let P move towards A, so as to come to the situation P', and let the attraction here be P' M'; as it is continual during the motion of P to P', M M' is a curve line. Now in the case of the attraction of bodies for light, and for one another, P M is less than P' M'; and consequently M M' does not ever return into itself, and therefore it must go *ad infinitum*, having its arc between A B and A C, to which it approaches as asymptotes, the abscissa always representing the distance, and the ordinate the attraction at that distance. Let P' now continue its motion to P'', and M' will move M''; and if P'' meets A, or the bodies come into perfect contact, P'' M'' will be infinite; so that the attraction being changed into cohesion will be infinite, and the bodies inseparable, contrary to universal experience; so that P can never come nearer to A than a given distance. In the case of gravity P M is inversely as the square of A P, so that the curve N M M''' is the cubic hyperbola; but the demonstration holds, whatever be the proportion of the force to the distance. It appears then, that flexion, refraction, and reflexion, are performed by a force acting at a definite distance; and it is reasonable to think even *a priori*, that, as this same force in other circumstances is exerted to a different degree on the different parts of light in refracting, inflecting, and deflecting them, it should also be exercised with the like variations in reflecting them. Let us attend to the proof which enables us to change conjecture into conviction.

Obj. I. The sun shining into my darkened chamber through a small hole, $\frac{1}{16}$ th of an inch in diameter, I placed a pin of $\frac{1}{32}$ th of an inch diameter in the cone of light (one half inch from the hole) inclined to the rays at an angle of about 45° ; and its shadow was received on a chart parallel to it, at the distance of two feet. The shadow was surrounded by the three fringes on each side, discovered by Grimaldo; beyond these there were two streaks of white light diverging from the shadow, and mottled with bright colours, very irregularly scattered up and down; but on using another pin, whose surface was well polished, and placing it nearer the hole than before, the colours in the streaks became much brighter (and the streaks themselves narrower), being extended from one side to the other, so that, except in a very few points here and there, no white was now to be seen; and, on moving the pin, the colours moved also. But they disappeared if the pin was, deprived of its polish by being held in the flame of a candle, or if a roll of paper was used instead of the pin; also they were much brighter in direct than in reflected light, and in the light of the sun at the focus of a lens, than in his direct unrefracted light. Placing a piece of paper round the hole in the window-shut, I observed the colours continued there; and inclining the chart to the point where they left off, I saw them continued on it and then proceed as before to the shadow. If the pin was held horizontally, or nearly so, they were seen of a great size on the floor the walls and roof of the room, forming a large circle; and if the chart was laid horizontally, and the pin held between the hole and it, in a vertical position, the circle was seen on the chart, and became an oval by inclining the pin a little to the horizon.

* Nova Theoria Philosophiæ Naturalis.

Obj. II. Having produced a clear set of colours, as in the last Observation, I viewed them as attentively as possible, and found that they were divided into sets, sometimes separated by a gleam of white light, sometimes by a line of shadow, and sometimes contiguous, or even running a little into one another. They were spectra or images of the sun, for they varied with the luminous body by whose rays they were formed, and with the size of the beam in which the pin was held; and when by placing it between my eye and the candle a little to one side I let the colours fall on my retina, I plainly saw that they resembled the candle in shape and size (though a little distended), and also in motion, since, if the flame was blown upon, they had the like agitation. The colours, therefore, which fell on the chart were images of the sun: they had parallel sides pretty distinctly defined, but the ends were confused and semicircular, like those of the prismatic spectrum. Like it, too, they were oblong, and in some the length exceeded the breadth six, even eight, times. The breadth was, as I found by measurement, exactly equal to that of the sun's image received on a chart as far from the pin as the image was, and the length was always to the breadth at all distances in the same ratio, but not in all positions of the pin; for, if it was moved on its axis, the images moved towards the shadow on one side, and from it on the other, becoming longer and longer (the breadth remaining the same) the nearer they came to the shadow on the one side, and shorter in the same proportion the farther they went from it on the other.

Obj. III. Having picked out an image that appeared very bright and well defined, I let it through a hole with moveable sides in the upper part of a sort of desk, which moved to any opening by hinges, and had a chart for its under side, on which the image fell, and I shut the hole so close as to prevent any of the others from coming through. I then had a full opportunity of examining it in all respects, and I counted in it distinctly the seven prismatic colours; the red was farthest from the shadow of the pin, and from the pin itself; then the orange; then the yellow, green, blue, and indigo; and the violet nearest of all: in short, it was exactly similar to a prismatic spectrum much diminished in length and breadth, and turned horizontally on the wall opposite to the prism, with the red farthest away. In figure 5, *sc* is the pin reflecting the rays *CP* and *CO*, which pass through *PO* the hole in the desk *ED*, to the chart or bottom of the desk *RTSD*; and from there the spectrum *IK* divided into its colours, *I* being violet, and *K* red. On moving the hole in the desk, and letting through other images, the colours were not in all arranged the same way: but I moved the pin on its axis, and observed those where the order was inverted, to move, not only with respect to the pin, but also with respect to the contiguous images; and I was surprised to see them assume the order of colours first mentioned, namely the red outermost, and the violet innermost. In like manner, the images which before the motion were regular, on moving into the places left by the others had always the order of their colours inverted, so that the thing must be owing to some irregularities in the pin's surface; for those which were made by a small glass tube filled with quicksilver, and freed from scratches by a blow-pipe, preserved during the motion the proper order of colours. Another irregularity in the arrangement was also observable even in the glass tube; for two contiguous images, by mixing one with another for two or three successions, appeared each to have outermost a dull colour between red and violet, and innermost a green; but here

unless

unless the succession continued through all the images, the outermost of all was red, and the innermost image had universally violet in the inside.

Obj. IV I placed at a hole in the window-shut a prism to refract the rays, and received the spectrum at the distance of six feet from the window on a chart; then at the distance of two feet I placed a screen with a hole in the middle of it, through which I let pass successively the different rays. At the distance of one inch from the hole, between it and the chart, I placed the reflecting cylindrical body; the images were found on the chart and walls of the room round to the sides of the hole on the screen, and were always wholly of the colour in which they were formed, except in the confines of the green, where a small quantity of white light fell, and made them of all the seven colours; but this was almost wholly prevented by using a prism with a greater refracting angle, and holding the pin and screen farther from it. I then removed the screen, and left the reflector in its place, so as it might reach through the rays; and thus there were formed images having in them, from top to bottom, the seven colours, one after another, the lowest division being red, the highest violet. They were inclined considerably towards their tops, and were much broader at the bottom or red parts than at the tops or violet parts. And lastly, the reflector being moved so that the images might be disturbed (as in the former experiment made in the white light), the red was most, the violet least dilated. In case these effects might be owing to any peculiarities in the shape or position of the reflector, I placed at three feet from the prism a lens of four inches breadth, to collect the rays to a focus, six feet beyond which I held a chart, and there received the spectrum inverted, the red being uppermost, and the violet undermost; holding the reflector at two feet from the focus and four from the chart, the images were formed just as before, only inverted, inclining towards the violet, of greater breadth towards the red, and more distended towards the same quarter when the reflector was moved.

Obj. V. Things remaining as in the last part of the last Experiment; at the focus of the lens I placed a second prism, which refracted the rays into a white beam*, and this I received on a screen with a hole in the middle, through which a small part of it passed, and falling on the reflector placed behind was formed by it into images after the manner of the first Experiment, each having in regular order the seven prismatic colours. One of the brightest and most distinct I let pass through a hole in the second screen, and it fell on the chart. I then caused an assistant to intercept the red rays between the first prism and the lens, and immediately the red part of the image vanished; and when the violet was intercepted, the violet of the image vanished; and if the green was intercepted, the green was wanting in the image. In short, whatever colours were stopped, the same were missing in the image. In Fig. 6. the rays passing through the hole C of the window, A B are refracted by the prism P M N, and separated into D V, D G, and D R, violet, green and red; which, being collected into a focus F by the lens L, are there again refracted by a prism P' M' N', and formed into a white beam a b m n, part of which is intercepted by the screen S S', and part passes through the hole h, as h H to H on the chart X Y Z W, and part is reflected by the body o q into a set of images which are received on a screen T U, and one of them, r g v, let pass to W X Y Z; but when an obstacle E stops D R, r the red va-

* Optics, Book ii. Part II. Prop. 2.

nishes; and if DG be stopped, g the green vanishes; and if DV be stopped, v disappears. Lastly, if DR and DG be stopped, g and r vanish.

Obs. VI. Having produced a set of bright images, I let one pass through the desk described in the third Experiment, and received it on a small lens $\frac{1}{2}$ inch broad to collect the rays into a focus, which I received on the chart by moving it a little on its hinge; and by all the observations I could make, and all the tests I could think of, it was white inclining to yellow, and of the same nature and constitution with the sun's direct light: but if any ray was stopped before coming to the lens, the focus was a mixture of the remaining rays; and the chart being moved a little farther round, the image was formed on it, the colours being in an inverted order. At the focus I held a reflector, and there were formed images of all the seven colours, as in the sun's direct light (Exp. 1.); if the light was sufficiently strong, and the desk near the window-shut hole, one of these could even be collected by a second lens into a white focus. This experiment is rendered more uniform by substituting for the lens a concave metallic mirror, and placing at the focus another mirror to reduce the rays into a beam which may be made of any composition we please by stopping one or more of the colours at the hole in the desk. I observed in the course of these experiments a phenomenon worth mentioning; if a comb (as in Newton's experiment *) be very swiftly moved before one of the images or more, a sensation of white is produced: but this is still more evident if the pin be swiftly moved round its axis; for then the images move also, and, running into one another, cause a sensation of perfect whiteness.

Obs. VII. I let an image through the hole in the desk, and viewed it through a glass prism, holding its axis parallel to the sides of the image, and its refracting angle upwards. I found that if the image was bright, and free from white light, the colours were not changed by the refraction; but if it was mixed and diluted with white, the prism, decomposing the white, caused the image to appear violet at one side, and red at the other; yet still this only confused the colours of the image without changing them. Farther, if the prism was moved on its axis, the violet was lifted higher than the red or any of the other colours. Nor was the constitution of the colours at all changed by reflexion from a pin or mirror, except in so far as they were mixed by a concave one, as mentioned in the last experiment. If a pin was held behind the hole to reflect the colours, it formed other images of the colour in which it was held, and as far as I could judge threw the red to the greatest distance and breadth and inclination. Nor were the colours of the image changed by reflexion from natural bodies, for these were all of the colours in which they were held, but brightest in that which they were disposed to reflect most copiously. Likewise the rings of colour made by thin plates were broadest in the red, and narrowest in the violet; and the like happened to the fringes that surround the shadows of bodies. Lastly, the shadows of bodies were themselves broadest in the violet, and narrowest in the red.

Obs. VIII. I filled with water a glass tube, whose diameter was $\frac{1}{4}$ th of an inch, and consequently the radius of curvature $\frac{1}{2}$ th, and whose sides were $\frac{1}{16}$ th of an inch thick; then standing at four feet from a candle I held the tube $\frac{1}{4}$ th of an inch from my eye, so that the light of the candle might be refracted through it, and moved my eye-lids close enough to prevent the extraneous scattered light from entering along with that which was regularly

refracted. I saw several images of the candle, all highly coloured, and the colours were, in order from the candle outwards, red, orange, and so on to violet. I then filled the tube with clear diluted sulphuric acid, and dropped a small piece of chalk to the bottom, when immediately an effervescence took place by the escape of fixed air, which rose in bubbles through the tube; and looking at the candle through one of these, I saw the images formed with the colours still in the same order, but a little larger than before:

We are now to see to what conclusions these experiments lead us. The first experiment shews, that all sorts of lights, whether direct or reflected or refracted, produce colours by reflexion from a curve surface. From the second we learn that these colours are distinct images or spectra of the luminous body, much dilated in length, but not at all in breadth, and that the angle of incidence being changed, the dilatation of the images is also changed; and from the third experiment it appears, that each full image is composed of seven colours, red, orange, yellow, green, blue, indigo, and violet; and that the proper order is red outermost, and violet innermost, the rest being in their order. The fourth experiment shews, that these images are produced, not by any accidental or new modification impressed on the rays, but by the white light being decomposed by reflexion; that the mean rays, or those at the confine of the green and blue, are reflected at an angle equal to that of incidence, and the red at a less, the violet at a greater angle. Experiments 5th and 6th prove, beyond a doubt, the decomposition and separation of the rays by reflexion; for in both we see that the colours in the images are those, and those only, which were mixed in the ray by reflexion or refraction before and at incidence, whilst the 6th is (in addition) a proof that all the rays of any one image, if mixed together, compound a beam exactly similar to the beam that was at first decomposed. The seventh experiment shews, that the colours into which the rays are separated by reflexion are homogeneous and unchangeable; that they differ in flexibility and refrangibility; that they bear the same part in forming images by reflexion, and fringes by flexion, and colours from thin plates, which the rays separated by the prism do. And in the 8th experiment we see, that when the rays are placed in the same situation with respect to refraction, whether out of a rarer into a denser, or a denser into a rarer medium, in which they before were with respect to reflexion, the position of the colours produced is diametrically opposite in the two cases. Seeing, then, that in all sorts of light, direct, refracted, reflected, simple and homogeneous or heterogeneous, and compounded, and in whatever way the separation and mixture may have been made, some of the rays, at equal or the same incidences, are constantly reflected nearer the perpendicular than the mean rays, and others not so near; and seeing that by such reflexion the compound ray of whatever kind is separated into parts so simple that they can never more be changed; and considering the different places to which these parts are reflected; it is evident, that the sun's light consists of parts different in reflexivity, and that those which are least refrangible are most reflexible. By reflexivity I here mean a disposition to be reflected near to the perpendicular in any degree.

Although I have given what I take to be sufficient proof of this property of light, yet I am aware that something more is requisite. It will be asked, why does neither a plane, a common convex, nor a common concave mirror separate the rays by reflexion? This is what has always hindered us from even suspecting such a thing as different reflexivity. I shall, however, take an opportunity of removing this obstacle in the second part of the plan, when

when I come to explain the reason of the colours made by the reflecting body, and the manner of their formation. At present I shall only caution those who may wish to repeat the above experiments, that the hole in the window-shut must be small, the room quite dark, the pin well polished, and the desk, chart, &c. placed at a distance from the pin not greater than three feet, otherwise the images will be dilute and dim; nor, on the other hand, less than six inches, otherwise they will be too short, and the colours not far enough separated one from another.

My next object of enquiry was the different degrees of reflexibility belonging to each ray. It appears, not only from mathematical considerations sufficiently obvious, but also from the experiments I have related, that though the different rays have, at the same or equal incidences, different angles of reflexion, yet each ray is constant to itself in degree of reflexibility, and that its sine of reflexion bears always the same ratio to its sine of incidence. The question then is, what are the sines of reflexion of the different rays, the sine of incidence being the same to all?

Obj. IX. In summer, at noon, when the sun's light was exceedingly strong, and there was not the vestige of a cloud in the sky, I produced an uncommonly fine set of images by fixing, at an inch from the small hole $\frac{1}{50}$ th of an inch diameter, a pin $\frac{1}{4}$ th of an inch diameter. One of the brightest of these I let pass through the desk to the chart below at $2\frac{1}{2}$ feet from the pin, and the image was three inches from the shadow, in a straight line. I delineated it carefully by drawing two parallel lines for the sides, and marking the semi-circular ends. Then with the point of a small needle I marked the confines of the contiguous colours on one of the parallel sides, and afterwards drew across the image parallel lines. This operation I repeated with the same and different images at many distances from the pin, and on different days, with various sorts of pins and sizes of holes, &c. &c. and all these repetitions were made before I once examined the result of any one measurement, that I might be unprejudiced in trying the thing over again. I then compared the sketches of divided images which I thus obtained, and found sufficient reason to conclude, that the differences between the sines of reflexion in the different rays were in the harmonical order. For the divisions were nearly as $\frac{1}{2}, \frac{1}{4}, \frac{1}{8}, \frac{1}{12}, \frac{1}{16}, \frac{1}{20}, \frac{1}{24}, \frac{1}{30}, \frac{1}{36}, \frac{1}{40}$, which, when compounded with the scale, gave $1, \frac{1}{2}, \frac{1}{3}, \frac{2}{5}, \frac{3}{7}, \frac{4}{9}, \frac{5}{11}, \frac{6}{13}, \frac{7}{15}, \frac{8}{17}$; and these are exactly the change of the notes in an octave, obtained by taking the sums of the octave, and a second major, a third major, a fourth, a fifth, a sixth major, a seventh major, and an eighth, instead of the difference between a double octave and a second major, a third major, and so on. Thus the spectrum by reflexion is divided exactly as the spectrum by refraction, only that the former is inverted, and the different rays have reflexibilities, that are inversely as their refrangibilities. Having settled this (I flatter myself) curious and important point, I proceeded next to enquire into the absolute reflexibility of the extreme colours; for if this be known, the angle of incidence being given, the angle of reflexion of all the different rays may be found. For obtaining a solution of this problem I made the following experiment:

Obj. X. The sun shining strongly through the small hole in the window-shut, and the rays diverging into a cone whose base fell on an horizontal chart $2\frac{1}{2}$ feet from the hole, between the hole and chart I placed a screen which had a plate and small hole in it. The rays passing through this fell on a small pin, so placed that the images formed might

be at right angles to the shadow; one of these I measured together with its distance from the shadow, the distance of the shadow from the hole, the breadth of the shadow, and the diameter of the pin: these measures were as follows. In fig. 7, C is the centre, and B *o* n the circumference of the pin, G M the chart, and G D a line in it, being the axis of all the images, at right angles to C D, the distance of C from D the centre of the shadow, and also to the shadow itself; G E is the parallel side of the image, G being red, E violet, and F the confine of the green and blue. C e is a radius parallel to E D, and C A another drawn through B, the point where O B is incident at the angle O B A, to which (by what was before shown) A B F is equal. By measurement G E is $\frac{1}{4}$ th of an inch, C B $\frac{5}{8}$ th, C D $\frac{1}{2}$; now the shadow being lessened by a penumbra, this added to half the shadow, and their sum to the distance between the penumbra and the violet, gave E D $\frac{11}{16}$ ths of an inch. From whence it is easy to calculate, that the angle of incidence being $77^{\circ} 20'$, the angle of the red's reflection A B G is $75^{\circ} 50'$ and that of the violets $78^{\circ} 51'$. Now the natural sines of $77^{\circ} 20'$, $75^{\circ} 50'$, and $78^{\circ} 51'$, are as 9756, 9695, and 9811; or as 250, 248, and 251, which are very nearly as $77\frac{1}{2}$, 77, and 78; and making an allowance for the omissions made in the reductions, the errors in the operations and measurements, they may be accounted as accurately in the above proportion. Now their extremes 77 and 78 are the very proportions of the red's refrangibility to the violet's*. So that the reflexivity of the red is to that of the violets as the refrangibilities inversely. But it is obvious that the sine of incidence is not the same in the two cases; for in the one it is equal to that of the mean rays reflection; while in the other none of the rays are refracted at an angle equal to that of incidence, otherwise they would not be refracted at all. This, however, being a consequence of the essential distinction in the circumstances, does not impair the beautiful analogy which we have seen is preserved in the two operations, and which proves them to be different exertions of the same power. Now we may find, from the data obtained, the sines of all the rays in the spectrum by adding to 77, the lengths of the spaces into which it is divided, and which are, without any sensible error, as the differences of those sines. The sines of the red will be from $77\frac{1}{8}$ to $77\frac{1}{2}$; the orange from $77\frac{1}{8}$ to $77\frac{1}{2}$; the yellow from $77\frac{1}{8}$ to $77\frac{1}{2}$; the green from $77\frac{1}{8}$ to $77\frac{1}{2}$; the blue from $77\frac{1}{8}$ to $77\frac{1}{2}$; the indigo from $77\frac{1}{8}$ to $77\frac{1}{2}$; the violet from $77\frac{1}{8}$ to 78. So that, the sine of incidence being given, that of the reflection of all the different rays may be found; and the angle of incidence being $50^{\circ} 48'$, the angles of reflexion are as follows: Of the extreme red $50^{\circ} 21'$; of the orange $50^{\circ} 27'$; of the yellow $50^{\circ} 32'$; of the green $50^{\circ} 39'$; of the blue $50^{\circ} 48'$; of the indigo $50^{\circ} 57'$; of the violet $51^{\circ} 3'$; and of the extreme violet $51^{\circ} 15'$.

I shall conclude this part of the subject with a few remarks on the physical cause of reflexivity. As light is reflected by a power extending to some distance from the reflecting surface, the different reflexivity of its parts arises from a constitutional disposition of these to be acted upon differently by the power. And as these parts are of different sizes, those which are largest will be acted upon most strongly. I shall not hesitate to go a step farther. In fig. 8, let EC be the reflecting surface, DH the perpendicular, and AB a ray incident at B, and produced to F, and reflected into GB; draw GH parallel to FB, and GF to HB; then HB : (HG :) BF :: sin. HGB : sin. HBG or :: sin. GBF : sin. HBG. But GBF is

* Optics, Book i. Part I. Prop. 7.

the supplement of GBA, the sum of the angles of reflexion and incidence; wherefore HB : BF :: the sine of the sum of the angles of reflexion and incidence, to the sine of the angle of reflexion; so that if I be the angle of incidence, R that of reflexion, V the velocity of light, and F the reflecting force; $F = \frac{V \times \sin. (R + I)}{\sin. R}$. By accommodating this formula

to the different cases, we obtain F in all the rays; and the ratio of F in one set to F in another being required, we have (by striking out V, which is constant) $F : F' :: \frac{\sin. (R + I)}{\sin. R} : \frac{\sin. (R' + I')}{\sin. R'}$. Suppose we would know F and F' in the red and violet re-

spectively; $I = 50^\circ 48' - R = 50^\circ 21'$, and $R' = 51^\circ 15'$; then $F : F' :: \frac{\sin. 101^\circ 9'}{\sin. 50^\circ 21'} : \frac{\sin. 102^\circ 3'}{\sin. 51^\circ 15'}$.

Performing the division in each by logarithms, and finding the natural sines corresponding to the quotients; $F : F' :: 1275 : 1253$. But the force exerted on the red is to that exerted on the violet as the size of the red to the size of the violet (by hypothesis); therefore the red particles are to the violet as 1275 to 1253. This may be extended to all the other colours by similar calculations; their sizes lying between 1275 and 1253, which are the extreme red and extreme violet; thus the red will be from 1275 to $1272\frac{1}{2}$; the orange from $1272\frac{1}{2}$ to 1270; the yellow from 1270 to 1267; the green from 1267 to 1264; the blue from 1264 to 1260; the indigo from 1260 to 1258; and the violet from 1258 to 1253.

All this follows mathematically, on the supposition that the parts of light are acted upon in proportion to their sizes: and to say the truth, I see no other proportion in which we can reasonably suppose them to be influenced; for such an action is not only conformable to the universal laws of attraction and repulsion, but also to the following arguments. If the action be not in the simple ratio, it must either be in a lower or in a higher. Let it be in a lower, as that of the square root, then the size of the red would be to the size of the violet as the squares of the forces; that is, as 1625625 to 1572009: a difference evidently too great; and *à fortiori* of the cube or any other root. On the other hand, if the action were in a higher ratio, as that of the square, then the particles would be as the square roots of the forces, or nearly as 35.70 to 35.39, a difference evidently too small; for if the size of the red particles were only $\frac{1}{16}$ ths greater than that of the violet, and the velocity of both were equal, the momentum and consequently the intensity of the red could not so much exceed that of the violet as we find it does; and as seems to me to be proved by the experiment of Buffon (on accidental colours), who found, that after looking at a white object, when he shut his eyes, it first became violet, then blue or a mixture of blue and the other colours, and last of all red: so in the impression of the white, compounded of the impressions of all the other rays mixed together, the violet was first obliterated or weakest, and the red last or strongest. To this reasoning on the intensity of the particles as owing to their size, I see only two objections that can be made. The one is, that the intensity is increased when the rays are thrown into a focus: but we must recollect, that the rays in this case are mixed, and their particles so blended as to be increased in size; for the number of separate rays thrown into one place will not increase their intensity sensibly. The other objection is that passage in Newton, where he says, "that the orange and yellow are the most lumi-

nous of all the colours, affecting the senses most strongly *." Now, besides that this is an assertion opposed by the positive experiment just now quoted, I think an answer may be thus made to it: The white light, from which the spectrum is never free, which inclines to yellow, and which is composed also of red, abounds in the yellow and orange of the spectrum; so that both of these colours derive their superior lustre rather than intensity from this circumstance; or, if they have any degree of the latter more than the red, it is in fact owing to their mixture with the red and the other rays which are all in the white.

[To be continued.]

IX.

An Account of the Manner in which Heat is propagated in Fluids, and its general Consequences in the Economy of the Universe. By BENJAMIN Count of RUMFORD.

[Concluded from p. 348.]

IN order to ascertain the action of water upon ice retained beneath it, Count Rumford took a cylindrical glass jar, 4,7 inches in diameter, and 13,8 inches high. Into this he put 43,87 cubic inches, or 1 lb. 11 $\frac{1}{2}$ oz. troy of water. When the jar had been placed in a freezing mixture, and the water was congealed, the ice adhered firmly to the bottom and sides of the jar, and was just three inches high. The jar was then placed in a mixture of pounded ice and pure water, and kept in that situation for four hours, in order that the cake of ice might be brought to the temperature of 32 degrees.

The jar still standing in a shallow dish in the pounded ice and water, the surface of which cold mixture was just on a level with the surface of the ice in the jar, 73 $\frac{1}{2}$ oz. troy of boiling-hot water were gently poured in, which filled it to the height of eight inches above the surface of the jar.

In these experiments it was evidently of the greatest consequence to prevent those irregularities which would arise from the action of pouring. The expedient first adopted for this purpose was to cover the ice with a circular piece of strong paper, which was gently removed after the pouring. But this not being thought sufficiently effectual, a flat shallow dish of light wood, half an inch deep, and somewhat less externally than the diameter of the jar, was provided. Its bottom was about a quarter of an inch thick, and was perforated with a great number of small holes, which gave it the appearance of a sieve. This perforated wooden dish, having been previously made ice-cold, was placed on the surface of the ice in the jar, and the hot water was gently poured into the dish through a long wooden tube. As the perforated dish floated and remained constantly at the surface of the water, and as the water passing through many hundreds of small holes was not projected downwards with force, the violent motions in the mass of water in the jar were thus in a great measure prevented. The water was not suffered to issue from the wooden tube in a perpendicular stream; but, the bore being closed, it was made to issue horizontally through a number of small holes in the sides of the tube at its lower end.

As soon as the operation of pouring the hot water into the jar was finished, the perforated dish was carefully removed, and the jar was covered with a circular wooden cover, from the centre of which a small mercurial thermometer was suspended. The irregularities of the experiments were greatly diminished by the expedient of the wooden dish. One more improvement however was added to the manipulation. It consisted in pouring $8\frac{1}{2}$ cubic inches of ice-cold water upon the ice in the jar, previous to the introduction of the wooden dish. This water covered the surface to the height of 0,478 of an inch.

Much as I could have wished to relate the whole of the interesting experiments in this essay, the requisite attention to brevity in this abridgment has made it necessary to omit every matter of subordinate detail. For the same reason I forbear to state the results afforded by the two first methods of trial. Those with the complete apparatus were as follows; the temperature of the room being 41 degrees.

No. of the Experiment.	Time the hot Water was on the Ice.	Temperature of the hot Water one inch below its surface.		Quantity of Ice melted.
		At the Beginning.	At the End.	
	Minutes.			Grains.
No. 25	10	192°	182°	580
26	30	190	165	914
27	180	190	95	3200

From the results of these three experiments, the Count proceeds to determine how much ice was melted in the act of pouring the water into the jar, and consequently the rate at which it was melted in the ordinary course of the experiment; supposing equal quantities to be melted in equal times. I give the reasoning in his own words.

As in the 27th experiment 3200 grains were melted in 180 minutes, and in the 25th experiment 580 grains were melted in 10 minutes, we may safely conclude that the same quantity must have been melted in the same time (10 minutes) in the 27th experiment. If therefore from 3200 grains, the quantity melted in 180 minutes in this last experiment, we deduct 580 grains for the quantity melted during the first ten minutes, there will remain 2620 grains for the quantity melted in the succeeding 170 minutes, when, the motions occasioned in the water on its being poured into the jar having subsided, we may suppose the process of melting the ice to have gone on regularly.

But if in the regular course of the experiment no more than 2620 grains were melted in 170 minutes, it is evident that not more than 154 grains could have been melted in the ordinary course of the process in ten minutes; for 170 minutes : 2620 grains :: 10 minutes : 154 grains. If therefore from 580 grains, the quantity of ice actually melted in ten minutes in the 25th experiment, we deduct 154 grains, there remains 426 for the quantity melted in pouring the water into the jar.

Let us see now how far this agrees with the result of the 26th experiment. In this experiment 914 grains of ice were melted in 30 minutes. If from this quantity we deduct 426 grains, the quantity which, according to the foregoing computation, must have been

melted

melted in pouring the hot water into the jar, there will remain 478 grains for the quantity melted in the ordinary course of the process in 30 minutes, which gives 159 grains for the quantity melted in ten minutes; which differs very little from the result of the foregoing computation, by which it appeared to be = 154 grains. This difference, however, small as it is, is sufficient to prove an important fact, namely, that the effects produced by the motion into which the hot water had been thrown on being poured into the jar, had not ceased entirely in 10 minutes, or when an end was put to the 5th experiment. We shall therefore come nearer the truth, if, in our endeavours to discover the quantity of ice melted in any given time in the ordinary course of the experiment, we found our computation on the results of the two experiments, No. 26 and No. 27.

In the latter of these experiments 3200 grains of ice were melted in 180 minutes, and in the former 914 grains were melted in 30 minutes. If, therefore, from 3200 grains, the quantity melted in 180 minutes, we take the quantity melted in the first 30 minutes = 914 grains, there will remain 2286 grains for the quantity melted in the succeeding 150 minutes, and this gives 152 grains for the quantity melted in 60 minutes. By the former computation it turned out to be 154 grains.

But if 152 grains of ice is the quantity melted in 10 minutes in the ordinary course of the process, three times that quantity or 456 grains only would have been melted in this manner in the 30 minutes during which the 26th experiment lasted; and deducting this quantity from 914 grains, the quantity actually melted in that experiment, the remainder 458 grains shews how much must have been melted in the pouring the hot water on the ice, or in consequence of the motion into which the water was thrown in the performance of that operation. By the preceding computation this quantity turned out to be 426 grains.

From the result of these computations the Count thinks we may safely conclude, that in the ordinary course of the experiments not more than 152 grains of ice were melted by the hot water in ten minutes.

He then proceeds to give an account of several experiments, in which the water employed to melt the ice was at a much lower temperature.

The previous congelation of water in the jar to the depth of four inches, the subsequent reduction to the temperature of 32° by the external contact of ice and water, and the weighing of the whole jar and ice contained therein, were performed as in the former experiments. The jar, which had been wiped with a dry cold napkin before weighing, was then replaced in the ice and water to the depth of its internal contents of ice. The quantity of $73\frac{1}{2}$ ounces troy of water at the temperature of 41° (which was also that of the room) was then poured in with the same precaution, of the wooden tube, dish, &c. as before. The results of a set of experiments were these:

Number of the Experiment.	Temperature of the Water in the jar one inch below its surface.		Temperature of the Air.	Time the Water remained on the Ice.	Quantity of Ice melted.
	Beginning of Experiment.	End of Experiment.		Minutes.	Grains.
No. 28	41°	40°	41°	10	203
29	41	40	41	10	270
30	41	40	41	10	237
31	41	40	41	10	228
32	41	38	41	30	617
33	41	38	41	30	585

By the results of these experiments, and their correspondence with each other, the extraordinary fact is established, that boiling water does not fuse more water while standing on its surface in a given time, than water at the temperature of 41° , or only nine degrees above freezing.

Indeed, as the Count observes, there is reason to conclude that it does not thaw so much. For the quantity melted in ten minutes by the hot water was 152 grains: and in the experiments with the cold water the quantity was $189\frac{1}{2}$ grains. He was too much interested in these researches to leave this anomaly unexamined. It remained to be ascertained, whether the latter experiments were not affected, as well as those with hot water, by the agitation of the water in pouring in, and to what extent; and also how far the action of the external air, by cooling the outer part of the cylinder of heated water, and causing it to descend, might impede the rising currents of cooled and expanded water from the surface of the ice. This last object was numerically determined by clothing the jar with cotton wool in some experiments, by surrounding it totally with ice and water in others, and comparing these results with such as were afforded when all the part of the jar above the ice had been left exposed to the atmosphere. For these numbers, the judicious precautions of experiment, and the comparative remarks upon them, I must again refer the philosopher to the Treatise itself, which every one who wishes to investigate the subject still farther, to repeat the experiments, or to draw more extensive theoretical inferences from them, must necessarily consult. The concluding Table, drawn from the others which precede, is as follows:

		Ice melted in 30 min. Grains.	
In the experiments in which the part of the jar occupied by the water was exposed uncovered to the air; then at 61° .	With boiling hot water	-	558 $\frac{1}{2}$
	With water at the temperature 61°	-	646
	With water at the temperature 41°	-	374
In the experiments in which the part of the jar occupied by the water was surrounded by pounded ice and water, and consequently was at 32° .	With boiling hot water	-	399 $\frac{1}{2}$
	With water at the temperature 61°	-	661
	With water at the temperature 41°	-	542

The Count considers these experiments as the most unquestionable proof that water is a perfect *non-conductor of heat*, and that heat is propagated in it only in consequence of the motions which the heat occasions in the insulated and solitary particles of that fluid. He remarks, that this discovery affords an insight into the nature of the mechanical process which takes place in chemical solutions; and he thinks that it will enable us to account in a satisfactory manner for all the various phenomena of chemical affinity, vegetation, and perhaps all the other motions among the inanimate bodies on the surface of the globe.

But without dwelling upon these circumstances, less immediately deducible from the nature of the communication of heat through water, the author hastens to apply his discoveries to the great operations which are regulated and performed on the surface of the globe by virtue of its most imperfect conducting power, and the law of its expansion and contraction, from the changes of temperature. This subject occupies the third and concluding chapter of his essay. I shall give the substance as nearly in the words of the author as the purposes of abridgment will allow.

Though

Though summer and winter, spring and autumn, and all the variety of the seasons, are produced by the simple and admirable contrivance of the inclination of the axis of the earth to the plane of the ecliptic; yet this mechanical disposition would not have been alone sufficient to produce that gradual change of temperature which the necessities of animal and vegetable life appear to require. It seems necessary not only that the changes should be gradual, but that the extremes of heat and cold should be mitigated by some equalizing power.

The cold air from the northern and southern regions of the earth rushes towards the heated parts in the regular winds which are known to prevail on its surface; and the return of these masses of air, which, when they become heated, rise and flow back in the upper departments of the atmosphere towards their former station, must greatly tend to preserve a more equal temperature of climates than would else have prevailed. But the agency of water in producing the same effect has been overlooked, though the remarkable law of its condensation by cold renders it wonderfully suited to that purpose.

M. de Luc has shewn, that by cooling water from the boiling point 80° to the freezing point 0 , if the whole contraction be divided into 80 equal portions, the condensation or loss of bulk for each 1° will be respectively, beginning at 80° , as follows: 18.0 ;— 16.2 ;— 13.8 ;— 11.5 ;— 9.3 ;— 7.1 ;— 3.9 ;—and 0.2 . Whence it is seen, that the increase of specific gravity, by cooling when near the mean temperature in England, is at least ninety times less than when the water is near boiling hot.

The vast extent of the ocean, and its great depth, but still more its numerous currents, and the power of water to absorb a vast quantity of heat, render it peculiarly well adapted to serve as an equalizer of temperature.

On the retreat of the sun after the solstice, it is closely followed by the cold winds from the regions of eternal frost, which are continually endeavouring to press on towards the equator. As the power of the sun to warm the surface of the earth and the air diminishes very fast in high latitudes on the days growing shorter, it soon becomes too weak to keep back the dense atmosphere which presses in from the polar regions, and the cold increases very fast.

There is however a circumstance by which these rapid advances of winter are in some measure moderated. The earth, but more especially the water, having imbibed a vast quantity of heat during the long summer days, while they received the influence of the sun's vivifying beams; this heat being given off to the cold air which rushes in from the polar region, serves to warm it and soften it; and consequently to diminish the impetuosity of its motion, and take off the keenness of its blast. But as the cold air still continues to flow in as the sun retires, the accumulated heat of summer is soon exhausted, and all solid and fluid bodies are reduced to the temperature of freezing water. In this stage the cold in the atmosphere increases very fast, and would probably increase still faster, were it not for the vast quantity of heat which is communicated to the air by the watery vapours, which are first condensed, and then congealed in the atmosphere, and which afterwards fall upon the earth in the form of snow; and by that still larger quantity which is given off by the water in the rivers and lakes and in the ground, upon its being frozen.

But in very cold countries the ground is frozen and covered with snow, and all the lakes and rivers are frozen over in the very beginning of winter. The cold then first begins to be
extreme,

extreme, and there appears to be no source of heat left which is sufficient to moderate it in any sensible degree.

It has been shewn—the Count thinks he may venture to say proved—in the most satisfactory manner, that liquids part with their heat only in consequence of their internal motions, and that the more rapid these motions are, the more rapid is the communication of the heat; that these motions are produced by the change in the specific gravity of the liquid occasioned by the change of temperature; and of course that they are more rapid as the specific gravity of the liquid is more changed by any given change of temperature.

But it has been shewn that the change in the specific gravity of water is extremely small, which takes place in any given change of temperature below the mean temperature of the atmosphere; and particularly when the temperature of the water is very near the freezing point; and hence it follows that water must give off its heat very slowly when it is near freezing.

But this is not all. When water is cooled to within eight or nine degrees of the freezing point, it not only ceases to be farther condensed, but is actually expanded by farther diminutions of its heat; and this expansion goes on, as the heat is diminished, as long as the water can be kept fluid; and when it is changed to ice it expands even still more, and the ice floats on the surface of the uncongealed part of the fluid.

It is well known that there is no communication of heat between two bodies, as long as they are both at the same temperature; and it is likewise known that the tendency of heat to pass from a hot body into one which is colder, with which it is in contact, is greater, as the difference is greater in the temperatures of the two bodies.

Suppose now that a mass of very cold air reposes on the quiet surface of a large lake of fresh water, at the temperature of 55° of Fahrenheit's thermometer. The particles of water at the surface, on giving off a part of their heat to the cold air with which they are in contact, and in consequence of this loss of heat becoming specifically heavier than those hotter particles on which they repose, must of course descend. This descent of the particles which have been cooled, necessarily forces other hotter particles to the surface; and these being cooled in their turns, bend their course downwards; and the whole mass of water is put into motion, and continues in motion as long as the process of cooling goes on.

Before he proceeds to trace this operation through all its various stages, the Count endeavours to remove an objection which may perhaps be made to his explanation of this phenomenon. As he supposes the mass of air which rests on the surface of the water to be *very cold*, and as he has taken it for granted that there is no communication whatever of heat between the particles of water in contact with this very cold air, and the neighbouring warmer particles of water, it may be asked, how it happens that these particles at the surface are not so much cooled as to be immediately changed to ice? To this he answers, that there are two causes which conspire to prevent the *immediate* formation of ice at the surface of the water. First, the specific gravity of the particle of water at the surface being increased at the same moment when it parts with heat, it begins to descend as soon as it begins to be cooled; and before the air has had time to rob it of all its heat, it escapes and gets out of its reach: and, Secondly, air being a bad conductor of heat, it cannot receive and transmit, or transport it with sufficient celerity to cool the surface of water so suddenly as to embarrass the motions of the particles of that liquid in the operation of giving it off.

But

But to return to our lake. As soon as the water in cooling has arrived at the temperature of about 40° , as at that temperature it ceases to be farther condensed, its internal motion ceases, and those of its particles which happen to be at its surface remain there; and after being cooled down to the freezing point they give off their latent heat, and ice begins to be formed.

As soon as the surface of the water is covered with ice, the communication of heat from the water to the atmosphere is rendered extremely slow and difficult; for ice, being a bad conductor of heat, forms a very warm covering to the water; and moreover it prevents the water from being agitated by the wind. Farther, as the temperature of the ice at its lower surface is always very nearly the same as that of the particles of liquid water with which it is in contact (the warmer particles of this fluid, in consequence of their greater specific gravity, taking their places below), the communication of heat between the water and the ice is necessarily very slow on that account.

As soon as the upper surface of the ice is covered with snow (which commonly happens soon after the ice is formed), this is an additional and very powerful obstacle to prevent the escape of the heat out of the water; and though the most intense cold may reign in the atmosphere, the increase of the thickness of the ice will be very slow.

During this time the mass of water which remains unfrozen will lose *no part of its heat*; on the contrary, it will continually be receiving heat from the ground. This heat, which is accumulated in the earth during the summer, will not only serve in some measure to replace that which is communicated to the atmosphere through the ice, and prevent its being furnished at the expense of the latent heat of the water in contact with its surface; but, when the temperature of the air is not much below that of freezing, this supply of heat from below will be quite sufficient to replace that which the air carries off, and the thickness of the ice will not increase.

Whenever the temperature of the air is not actually colder than freezing water, the heat which rises from the bottom of the lake will be all employed in melting the ice at its under surface, and diminishing its thickness.

It will indeed frequently happen, when the ice is very thick, and especially when its upper surface is covered with deep snow, that the melting of the ice at its under surface will be going on, when the temperature of the atmosphere is considerably below the freezing point.

As the particles of the water, which, receiving heat from the ground at the bottom of the lake, acquire a higher temperature than that of 40 degrees (and being expanded, and becoming specifically lighter by this additional heat, rise to the upper surface of the fluid water, and give off their sensible heat to the under surface of the ice), never return to the bottom, this communication of the heat which exhales from the earth produces very little motion in the mass of the water; and this circumstance is no doubt very favourable to the preservation of the heat of the water.

When a strong wind prevails, and the surface of the water is much agitated, ice is not formed, even though the whole mass of water should, by a long continuance of cold weather, have been previously cooled down to that point to which it is necessary that it should be brought, in order that its internal motions may cease, and that it may be disposed to congeal. For though the particles at and near the surface may no longer have any ten-

dency to descend, on being farther cooled, yet, as they have so considerable a quantity of sensible heat (eight or ten degrees) to dispose of, after their condensation by cold ceases; and as the agitation into which the water is thrown by the wind does not permit any particle to remain long enough in contact with the cold air to give off all its heat at once; there is a continual succession of fresh particles at the surface, all of which give off heat to the air, but none of them have time to be cooled sufficiently to form ice. The water will therefore lose a vast quantity of heat; and as soon as the wind ceases, if the cold should continue, ice will be formed very rapidly.

But it is not merely the agitation of the water which renders the communication of the heat very rapid; the agitation of the wind also tends to produce the same effect.

On the return of spring, the snow melting before the sun, as he advances and his rays become more powerful, all the heat which the earth exhales is employed in dissolving the ice at its under surface, while the sun on the other side acts still more powerfully to produce the same effect.

Though ice is transparent, yet it is not perfectly so; and as the light which is stopped, in its passage through it, cannot fail to generate heat *when* and *where* it is stopped or absorbed, it is by no means surprising that snow should be found to melt when exposed in the sun's rays, even when the temperature of the air in the shade is considerably below the point of freezing. Snow exposed to the sun melts long before the even surface of ice begins to be sensibly softened by its beams; and it is not till some time after all the hills are bare that the ice on the lakes and rivers breaks up.

The rays which penetrate a bank of snow, being often reflected and refracted, descend deep into it, and the heat is deposited in a place where it is not exposed to be carried off by the cold air of the atmosphere; but the rays which fall upon the horizontal and smooth surface of the ice are mostly reflected upwards into the atmosphere: and if any part of them are stopped at the surface of the ice, the heat generated by them *there* is instantaneously carried off by the cold air, and a particle of water is no sooner made fluid than it is again frozen.

Hence we see, that the snow which in cold countries covers the ice that is formed on the surface of fresh water, not only prevents the heat of the water from being carried off by the air during the winter, but also assists very powerfully in thawing the ice early in the spring.

Let us now see what the consequences would have been, had the condensation of water with cold followed the law which obtains in regard to all other fluids.

As the internal motion of the water could not have failed to continue as long as its specific gravity continued to be increased by parting with heat, ice would not have begun to be formed till the whole mass of water had arrived at the temperature of 32° of Fahrenheit's thermometer.

To see what an enormous quantity of heat would be lost when the water is deep, in consequence of its whole mass being cooled in this manner, we have only to compute how much ice this heat would melt, or how much water it would heat, from the point of freezing to that of boiling.

It has been shewn by experiments, that any given quantity of ice requires as much heat to melt it as an equal quantity of fluid water loses in cooling 140°; consequently the quantity of ice which might be melted by the heat given off by any given quantity of water in

cooling any given number of degrees, is to the given quantity of water as the number of degrees which it is cooled to 140° .

Hence it follows, that when the temperature of the water is 8° above the freezing point, it gives off in cooling, down to that temperature, as much heat as would melt $\frac{1}{5}$ ths or $\frac{1}{3}$ ths of its weight of ice : the water therefore which is cooled from the temperature of 40° to that of 32° , if it be 35 feet deep, will give off as much heat, in being so cooled, as would melt a covering of ice two feet thick.

But this even is not all; for, as the particles of water, on being cooled at the surface, would in consequence of the increase of their specific gravity, on parting with a portion of their heat, immediately descend to the bottom, the greatest part of the heat accumulated during the summer in the earth on which the water reposes would be carried off and lost before the water began to freeze; and when ice was once formed, its thickness would increase with great rapidity, and would continue increasing during the whole winter. And it seems very probable, that, in climates which are now temperate, the water in the large lakes would be frozen to such a depth in the course of a severe winter, that the heat of the ensuing summer would not be sufficient to thaw them; and should this once happen, the following winter could hardly fail to change the whole mass of its waters to one solid body of ice, which never more could recover its liquid form, but must remain immovable till the end of time.

In the month of February, after a frost which had lasted a month, the temperature of the air being 38° , M. De Saussure found the temperature of the water at the Lake of Geneva, at the surface, at 41° , and at the depth of 1000 feet, at 40° . Had the frost continued but a little longer, ice would have been formed; but had the constitution of water been such, that the whole mass of that fluid in the lake must have been cooled down to the temperature of 32° before ice could have been formed, this event could not have happened till the water had given off as much heat as would be sufficient to melt a covering of ice above 57 feet thick.

This quantity of heat would be sufficient to heat to the point of boiling a quantity of ice-cold water as large as the lake, and 49 feet deep.

When we trace still further the astonishing effects which are produced in the world by the operations of that simple law, which has been found to obtain in the condensation of water on its being deprived of heat, we shall find more and more reason to admire the wisdom of the contrivance.

That high latitudes might be habitable, it was necessary that vegetables should be protected from the effects of the chilling frosts of a long and severe winter; but if it be true that watery liquids do not part with their heat but in consequence of their internal motion, and if these motions are occasioned merely by the change produced in the specific gravity of those particles of the liquid which receive heat, or which part with it, who does not see how very powerfully the sudden diminution and final cessation of the condensation of water in cooling, as soon as its temperature approaches to the freezing point, operates to prevent the sap in vegetables from being frozen?

But if, for the purposes of life and vegetation, it be necessary that the ground, the rivers,

the lakes, and the trees, be defended against the cold winds from the poles, it may be asked, how this inundation of cold air is to be warmed? The Count answers, By the waters of the ocean, which there is the greatest reason to think were not only designed principally for that use, but particularly prepared for it.

Sea water contains a large proportion of salt in solution; and the condensation of a saline solution on its being cooled, follows a law which is extremely different from that observed in regard to pure water, and which (as may easily be shown) renders it peculiarly well adapted for communicating heat to the cold winds which blow over its surface.

As sea water continues to be condensed as it goes on to cool, even after it has passed the point at which fresh water freezes, the particles at the surface, instead of remaining there, after the mass of the water had been cooled to about 40° , and preventing the other warmer particles below from coming in their turns, and giving off their heat to the cold air (as we have seen always when fresh or pure water is so cooled)—these cooled particles of salt water descend as soon as they have parted with their heat, and in moving downward force other warmer particles to move upwards; and in consequence of this continual succession of warm particles which come to the surface of the sea, a vast deal of heat is communicated to the air—incomparably more than could possibly be communicated to it by an equal quantity of fresh water at the same temperature, as will appear by the following computation:

Without taking into the account that very great advantage which sea water possesses over fresh water, considered as an equalizer of the temperature of the atmosphere, which arises from the comparative *lowness of the point of its congelation*; supposing even sea water to freeze at as high a temperature as fresh water, namely at 32° ; and supposing (what is strictly true) that as soon as either sea water or fresh water is frozen at its surface, and this ice covered with snow, the communication of heat from the water to the atmosphere ceases almost entirely; the Count proceeds to determine how much more heat would, even on this supposition, be communicated to the air by salt water than by fresh water, after both have arrived at the temperature of 40° .

When fresh water, in cooling, has arrived at this temperature, it ceases to be farther condensed with cold, and its internal motions (which, as hath already more than once been observed, are caused solely by the changes produced in the specific gravity of its particles) cease of course, and ice immediately begins to be formed on its surface: but as the condensation of salt water goes on as its heat goes on to be diminished, its internal motions will continue; and it is evidently impossible for ice to be formed at its surface, till the whole mass of the water has become ice cold, or till its temperature is brought down to the supposed point 32° . It would therefore give off a quantity of heat equal to 8 degrees at least of Fahrenheit's thermometer more than the fresh water would part with before ice could be formed on its surface.

To be able to form an idea of this enormous quantity of heat, we have only to recollect what has already been said, and we shall find reason to conclude that it would be sufficient to melt a covering of ice equal in thickness to $\frac{2}{3}$ ths of the depth of the sea. It would therefore be sufficient in that part of the North sea (lat. 67°) where Lord Mulgrave sounded at the depth of 4680 feet, to melt a cake of ice 265 feet thick.

But

But the heat evolved in the formation of each superficial foot of ice, would be sufficient to raise the temperature of a stratum of incumbent air 2220 times as thick as the ice (consequently in the case in question 265×2220 feet or 869 miles thick) 28 degrees, or from the temperature of freezing water to that of 50° of Fahrenheit's thermometer, or to the mean annual temperature of the northern parts of Germany.

The heat given off to the air by each superficial foot of water in cooling *one degree*, is sufficient to heat an incumbent stratum of air 4.4 times as thick as the depth of the water 10 degrees. Hence we see how very powerfully the water of the ocean, which is never frozen over except in very high latitudes, must contribute to warm the cold air which flows in from the polar regions.

But the ocean is not more useful in moderating the extreme cold of the polar regions than it is in tempering the excessive heats of the torrid zone; and what is very remarkable, the fitness of the sea water to serve this last important purpose, is owing to the very same cause which renders it so peculiarly well adapted for communicating heat to the cold atmosphere in high latitudes, namely, *to the salt which it holds in solution.*

As the condensation of salt water with cold continues to go on, even long after it has been cooled to the temperature at which fresh water freezes, those particles at the surface which are cooled by an immediate contact with the cold winds must descend, and take their places at the bottom of the sea, where they must remain, till by acquiring an additional quantity of heat their specific gravity is again diminished. But this heat *they never can regain in the polar regions*; for innumerable experiments have proved beyond all possibility of doubt, that there is no *principle of heat* in the interior parts of the globe, which by exhaling through the bottom of the ocean could communicate heat to the water which rests upon it.

It has been found that the temperature of the earth, at great depths under the surface, is different in different latitudes, and there is no doubt but this is also the case with respect to the temperature at the bottom of the sea, in as far as it is not influenced by the currents which flow over it; and this proves to a demonstration, that the heat which we find to exist without any sensible change during summer and winter at great depths, is owing to the action of the sun, and not to central fires as some have too hastily concluded.

But if the water of the ocean, which, on being deprived of a great part of its heat by cold winds, descends to the bottom of the sea, cannot be warmed *where it descends*, as its specific gravity is greater than that of water at the same depth in warmer latitudes, it will immediately begin to spread on the bottom of the sea, and to flow towards the equator; and this must necessarily produce a current at the surface in an opposite direction. There are the most indubitable proofs of the existence of both these currents.

The proof of the existence of one of them would, indeed, have been quite sufficient to have proved the existence of both; for one of them could not possibly exist without the other; but they are several direct proofs of the existence of each of them.

What has been called the gulf stream in the Atlantic Ocean, is no more than one of these currents, namely that at the surface, which moves from the equator towards the north pole, modified by the trade winds, and by the form of the continent of North America; and the progress of the lower current may be considered as proved directly by the cold which has been found to exist in the sea at great depths in warm latitudes; a degree of temperature

much below the mean annual temperature of the earth in the latitudes where it has been found, and which of course must have been brought from colder latitudes.

The mean annual temperature in the latitude of 67° has been determined by Mr. Kirwan, in his excellent treatise on the temperature of different latitudes, to be 39° ; but Lord Mulgrave found on the 20th of June, when the temperature of the air was $48\frac{1}{2}^{\circ}$, that the temperature of the sea at the depth of 4680 feet was 6 degrees below freezing, or 26° of Fahrenheit's thermometer.

On the 31st of August in the latitude of 69° , where the annual temperature is about 38° , the temperature of the sea at the depth of 4038 feet was 32° ; the temperature of the atmosphere (and probably that of the water at the surface of the sea) being at the same time at $59\frac{1}{2}^{\circ}$.

But a still more striking and incontrovertible proof of the existence of currents of cold water at the bottom of the sea, setting from the poles towards the equator, is the very remarkable difference that has been found to subsist between the temperature of the sea at the surface, and at great depth at the tropic, though the temperature of the atmosphere there is so constant, that the greatest changes produced in it by the seasons seldom amount to more than five or six degrees; yet the difference between the heat of the water at the surface of the sea, and that at the depth of 3600 feet, has been found to amount to no less than 31 degrees; the temperature above, or at the surface, being 84° , and at the given depth below, no more than 53° *.

It appears to the Count to be extremely difficult, if not quite impossible, to account for this degree of cold at the bottom of the sea in the torrid zone, on any other supposition than that of cold currents from the poles; and the utility of these currents in tempering the excessive heats of those climates is too evident to require any illustration.

These currents are produced, as we have already seen, in consequence of the difference in the specific gravity of the sea water at different temperatures: their velocities must therefore be in proportion to the change produced in the specific gravity of water by any given change of temperature; and hence we see how much greater they must be in salt water than they could possibly have been had the ocean been composed of fresh water.

It is not a little remarkable, that the water of all great lakes is fresh, and nearly so in all inland seas (like the Baltic) in cold climates, which communicate with the ocean by narrow channels. We shall find reason to conclude, that this did not happen without design, when we consider what consequences would probably ensue, should the waters of a large lake in an inland situation in a cold country (such as the lake Superior for instance in North America) become as salt as the sea.

Though the cold winds which blow over the lake in the beginning of winter would be more warmed, and the temperature of the air on the side of the lake opposite to the quarter from whence these winds arrive, would be rendered somewhat milder than it now is; yet, as the water of the lake would give off an immense quantity of heat before a covering of ice could be formed on its surface for its protection, it would on the return of spring be found to be extremely cold; and as it would require a long time to regain, from the influence of the returning sun, the enormous quantity of heat lost during the winter, it would

* Philosophical Transactions M.DCC.LII.

remain very cold during the spring, and probably during the greatest part of the summer; and this could not fail to chill the atmosphere, and check vegetation in the surrounding country to a very considerable distance. And though a large lake of salt water in a cold country would tend to render the winter somewhat milder on one side of it, namely on the side opposite to the quarter from whence the cold winds came; yet this advantage would not only be confined to a small tract of country, but would not any where be very important, and would by no means counterbalance the extensive and fatal consequences which would be produced in summer by so large a collection of very cold water.

When the winter is once fairly set in—when the earth is well covered with snow, and the rivers and lakes with ice, and more especially when the ice as well as the land is covered with that warm winter garment, a few degrees more of cold in the air cannot produce any lasting bad consequences. It may oblige the inhabitants to use additional precautions to guard themselves, their domestic animals, and their provisions, from the uncommon severity of the weather; but it can have very little influence in the temperature of the ensuing summer; and it is even probable, if it influences it at all, that it tends rather to make it warmer than colder. Lakes of salt water could therefore be of no real use in winter in cold countries, and in summer they could not fail to be very hurtful; while fresh lakes, as they are frozen over almost as soon as the winter sets in, and long before the whole mass of their water is cooled down to the temperature of freezing, must preserve the greater part of their heat through the winter; and if they are of no use during the cold season, they probably do little or no harm in summer*.

X.

Useful Notices respecting various Objects.—Welding of Cast-Steel—Flexure of Compound Metallic Bars by Change of Temperature.

1. Welding of Cast-Steel. By Sir THOMAS FRANKLAND, Bart.†

THE uniting of steel to iron by welding is a well-known practice; in some cases for the purpose of saving steel; in others, to render work less liable to break, by giving the steel a back or support of a tougher material.

Ever since the invention of cast-steel (or bar-steel refined by fusion) it has generally been supposed impossible to weld it either to common steel or iron; and naturally—for the description in Watson's Chemical Essays (vol. iv. p. 148) is just, that in a welding heat it “runs away under the hammer like sand.” How far the Sheffield artists, who stamp much low-priced work with the title of cast-steel, practise the welding it, I am ignorant; but though I have enquired of many smiths and cutlers in different parts of the kingdom, I have not yet found the workman who professed himself able to accomplish it. If therefore I should describe a simple process for the purpose, I may be of use to the very many who are incredulous on the subject. If any one has made the discovery on principle, he has reasoned thus: Cast-steel in a welding heat is too soft to bear being hammered; but is there no

* Throughout this Essay, but more particularly in the last chapter, the author has very strongly insisted on the final causes of the laws he has endeavoured to develop. I have no wish at present to discuss the question, how far the propagation of truth may be advanced by speculations on those causes; but have added this note, lest I should be thought to have mutilated his Essay by leaving out so striking a part. My aim has been to communicate his physical discoveries only. N.

† Phil. Trans. M.DCC.XCV. 296.

lower degree of heat in which it may be soft enough to unite with iron, yet without hazard of running under the hammer? A few experiments decided the question; for the fact is, that cast-steel in a white heat, and iron in a welding heat, unite completely.

It must not be denied that considerable nicety is required in giving a proper heat to the steel; for, on applying it to the iron, it receives an increase of heat, and will sometimes run on that increase, though it would have borne the hammer in that state in which it was taken from the fire.

I need scarcely observe, that when this process is intended, the steel and iron must be heated separately, and the union of the parts proposed to be joined, effected at a single heat. In case of a considerable length of work being required, a suitable thickness must be united, and afterwards drawn out, as is practised in forging resp-hooks, &c.

The steels on which my experiments have been made, are Walker's of Rotherham, and Huntsman's, between which I discover no difference; and though there may be some trifling variation in the flux used for melting, they are probably the same in essentials.

2. Flexure of Compound Metallic Bars by Change of Temperature.

A BAR of steel upwards of six inches long, 0.56 inch broad, and 0.05 inch thick, was hard-folded* to a bar of brass of the same dimensions, but twice the thickness. This compound bar was fixed at one end to a simple metallic bar, as in fig. 2. Pl.V. (facing p. 96) and at the other end there was a sliding piece to measure the deviations by flexure. It must be observed, that a thin piece of metal was placed between the simple and compound bars, at the pinned part A; so that the bars were not permitted to come into contact in any temperature, but each position was determined by the sliding piece, by means of a micrometer-screw and magnifier applied to it whenever its place was required to be known. The clear part of the compound bar from the pin A, to the end D, was exactly six inches.

By heating the apparatus from 66° to 212° , namely 146° , the catch was moved through 129 parts of the micrometer, each of which was $\frac{1}{1400}$ th of an inch. The quantity 129 was therefore equal to 0.0921 inch.

Both sides of the compound bar were then carefully filed away till the metals were each (as near as could be ascertained) only half their former thickness. The flexure, by a like difference of temperature, as in the former trial, now proved 278 parts, or 0.199 inch.

The bar was then filed to about $\frac{2}{3}$ ths of its original thickness, still preserving the relative thicknesses of the metals. Its flexure by the same heat as before was 313 parts, or 0.224 inch; in which, however, from the spring of the thin bar, it was doubted whether the catch had been moved to the same distance as the bar itself might have moved alone.

On considering these deviations, the numbers prove to be inversely as the thicknesses of the bars, with no greater error than might arise from the measures by which the real thicknesses were ascertained. These experiments may therefore be said to agree with the theory†.

* For the information of those who may not be acquainted with the processes of folding, I must observe, that this operation consists in uniting the pieces by the fusion of a kind of brass, which contains an extraordinary proportion of zinc, and flows into the joint before the heat is sufficient to melt the pieces. This kind of work will bear hammering and bending, because the folder is not brittle. Soft-folding is effected by plumbers' solder or tin, which forming a kind of bell metal with the copper is brittle in the joint, and therefore unfit for work intended to sustain either blows or flexure.

† Philosophical Journal, I. 64.



Fig. 1.



Fig. 2.

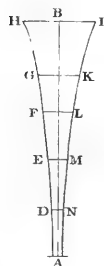


Fig. 3.

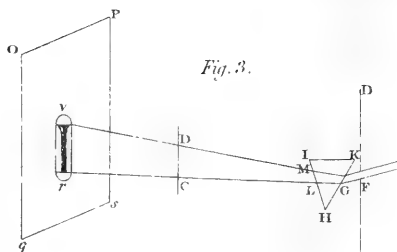


Fig. 4.

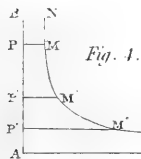


Fig. 5.

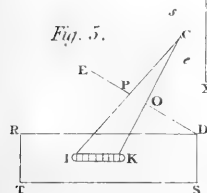


Fig. 6.

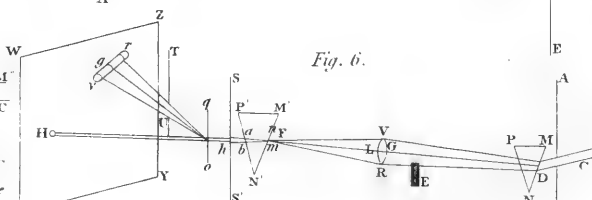


Fig. 7.

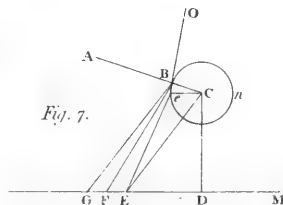


Fig. 8.

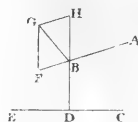


Fig. 9.

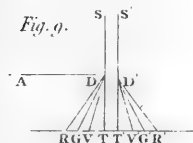


Fig. 10.

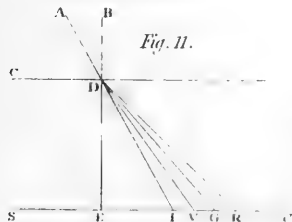


Fig. 11.



Fig. 12.





A
JOURNAL
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NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

MARCH 1798.—SUPPLEMENT.

ARTICLE I.

Observations on Water-Spouts seen from Nice. By M. MICHAUD, Correspondent with the Royal Academy of Sciences at Turin.*

WATER-spouts, as Mr. Senebier remarks, are phenomena which arise too seldom, and are too difficult of observation, to admit of our forming an accurate notion of their circumstances, or pointing out the means by which their causes may be investigated. Yet, as nature is never more disposed to explain her secret operations than in such as are performed on a great scale, there is no doubt but that these phenomena, if well explained, would lead to useful results †.

I do not flatter myself with the hope of giving a complete explanation of the cause of water-spouts, such as M. Senebier appears to desire; but as the true explanation can only be had by careful and frequent observation, I hope my work will not be totally useless if it should afford an additional step towards the desired end.

My observations did not in strictness commence till the 6th of January 1789, but it appears necessary, for the satisfaction and information of my readers, to mention some facts of an earlier date. I must therefore remark, that after a mild season for the greatest part of the month of December 1788, at Nice, where the winter is not in general severe, with clear weather in the day-time, our atmosphere underwent a total change on the day of the new moon, which was the 27th of that month. On that day a very violent storm of wind arose, attended with a degree of cold as acute as ever was known in the memory of man. The sky became covered with clouds, and snow fell to the depth of more than eight inches.

* Turin Mem. VI.

†. Senebier sur les Moyens de perfectionner la Météorologie. Journal de Rozier, Mai 1787.

As the same effects are produced by the same causes, I have little doubt but that water-spouts were formed near us on the night between the 27th and 28th. But, as I did not see them, it becomes unnecessary for me to enlarge upon any which I did not myself clearly observe.

The severe cold had frozen the snow, and rendered it so compact that, notwithstanding the influence of several days of clear weather soon afterwards, in which the heat of the sun was very perceptible, there was not the least drop of water fell from the eaves of the house in which I dwell, which is exposed to the sun in winter for eight hours, and, being situated near the sea, is perfectly sheltered on the north side by the eminence of the rock of the castle. This fact appeared very surprising to me, after a residence of about forty years in this town. Several old persons remarked, that this snow would wait for another fall before it melted, and I found by the result that the observation was true.

On Sunday the fourth of January 1789, at the phasis of the first quarter of the moon, the cold was again renewed, and continued severe on the Monday and Tuesday. At eight in the morning I first observed an immense mass of clouds towering upwards, and extending from north-east to south, which rose towards the zenith, by advancing to the westward. Accustomed as I am to consider these clouds according to the system of my old Professor of Natural Philosophy, Father Beccaria*, I concluded that they would proceed to desolate our fields, the fruits of which, particularly the oranges and lemons, had already perished by the antecedent cold. And as a strong wind then prevailed over the face of the sea, I foretold to my two eldest sons, that it was very probable we might discover some water-spout in the course of the day. In fact, about five minutes after ten in the morning, I observed on the sea, at the distance of not more than a musket-shot from the shore, a round space of ten or twelve toises in diameter, in which the water did not really boil, but seemed ready to boil. Plate XXI. V. For there appeared all round, and sometimes within the circle, vapours in the form of mists, eight toises and more in height, having the appearance, though on a scale incomparably larger, of those vapours which rise from the surface of water beginning to simmer. I saw clearly that this was, if I may so express myself, the embryo of the foot of a water-spout driven along by the wind, while the clouds were not sufficiently advanced to afford the stem a body. It continued therefore to move before the wind from east to west, keeping, to my very great surprise, its surrounding vapours, elevated like sails, notwithstanding the extreme force of impulsion which drove it towards the shore. As soon as it came near the land, the circle was contracted, the mass of vapour became of less dimensions, and at the moment it touched the land it was at once overset by the wind, under the appearance of a long train of mist, fig. 1, b, which was speedily dissipated. I then perceived that the hope I had formed of seeing water-spouts during the day, was on the point of being realized: but as my occupations demanded my attendance elsewhere, I charged my two eldest sons to watch alternately at the window, in order that the phenomenon might not pass unobserved.

At last, about eight minutes before noon, my second son came to me, exclaiming, "Father, here is a very superb water-spout." His earnestness was equal to that of a sailor,

* *Electricisme Atmospherique.* We have a translation in English.

who, after a long and tedious voyage, first discovers the land. I followed him to the window, and beheld an immense water-spout passing majestically before Nice, as represented in fig. 2. The clouds had already occupied not only the upper and southern part of the atmosphere, but they had proceeded towards the west, so as to cover the whole extent within my view; with this circumstance nevertheless, that they had left uncovered beneath and towards the south a part in the form of a segment of a circle, through which, at an extreme distance, some clouds were discerned, upon which the sun threw the colours of the morning.

The foot of this water-spout, which was in every respect infinitely superior to that which Messrs. Papacin, Renaud, and myself, observed in 1780, was so ample, that a man of war of 100 guns, with all its sails, might have been enveloped and even concealed in it. For we are, from daily practice, sufficiently accustomed to judge of the size of objects at different distances by that of the image formed upon the retina, assisted by our habitual reasoning. And hence, from the circular form of the foot of this water-spout, some judgment may be made of the volume of vapour it afforded.

Instead of the tranquillity it exhibited at its first appearance, this lower part assumed the resemblance of the crater of a volcano, with this exception, that it threw out nothing but large streams of cloud and spouts of sea-water. But it threw these in parabolic streams from the centre to the circumference, and all around, with such impetuosity and violence as to render it very evident to us, that an inexpressible effervescence must have prevailed in the interior basin, though the great distance and the opacity of the surrounding vapour prevented us from seeing the phenomena as distinctly as we saw the ebullition of the water-spout of 1780*.

The diameter of the water-spout, and that of its expanded upper part, were large in proportion. Its colour was a very deep indigo, the same as that of the clouds, which extended from east to west. It was impossible for us to observe the ascent of the vapours of fresh water; but the observation of 1780, in which this was clearly seen, supplies this defect. It will also be shewn that the ascension was again seen in a most complete manner.

While we were looking at this extraordinary appearance, which my sons beheld for the first time, and which seemed to have concentrated all their senses in one, on a sudden an impetuous shower of hail discharged itself against the windows in grains of the size of pistol and musket balls. We immediately suspended our observations, in order to close the shades of both stories of the house, in which the whole family assisted, for fear of having the windows absolutely broken to pieces, as happened a few years before. But I soon perceived that this precaution was absolutely useless, or at least unnecessary. For the hail, though in a few minutes it covered the ground to the height of four inches, did not in the least damage the trees in the garden behind our house. It consisted merely of large flakes of snow rounded by the wind in their fall, and possessing neither the weight nor the hardness of hail. Upon opening some of the pieces I found them to consist of a thin compact shell, nearly empty within, excepting a few rays from the centre to the circumference.

* See the *Journal de Physique*, XXX. Part I. 254. for *Observation sur une Trombe de Mer, faite à Nice de Provence en 1780, et adressée à M. Faujas de St. Fond, par M. Michaud*. The facts are less minutely stated, but nearly agree with the present memoir. N.

The degree of congelation in these balls was so slight that they began to melt the moment they touched the ground, and accelerated the fusion of the snow which had fallen before.

This frozen snow, which during its fall had obscured the air sufficiently to prevent our seeing the water-spout through the blinds, having ceased, we resumed our observations with all possible diligence, and beheld another water-spout somewhat inferior in magnitude to the former which had disappeared. It followed nearly the same course as the other. By the account of time employed by each in its successive passage, I estimated that the one before us must have been the third; nevertheless, by confining my narration to what I really saw, it must be considered as the second only. This water-spout having continued its course towards Antibes, we observed that it began to contract in all its dimensions, some time before it arrived at the shore, and that the foot was reduced to nothing when it touched the ground. It contracted insensibly upwards, the expanded conical part became broader and more rare, and the whole joined the mass of clouds in the same manner as one mist incorporates with another. There must consequently be some error in the account of the observation of 1780, where I say that the spout withdrew upwards as quick as lightning. This expression gives too precipitate an idea of the dissolution of the water-spout. Having thus kept sight of it until its total extinction, I returned towards the place where I had discovered the first water-spout, and was greatly surprised at discovering a new foot ready formed, without any descending spout a, fig. 3, Plate XXV. My astonishment was founded on these three circumstances:

1. The existence of the foot of the water-spout without its stem or body. For before this observation, and from the facts in 1780, I considered it as indubitable that the enveloping matter of the foot or recipient was a production of the body of the water-spout itself, or an expansion of its proper substance. Now I saw clearly enough in this phenomenon the identity of the substance which composes clouds and mists, and that it was not supplied by the water-spout. The embryo of the spout which I had seen at ten o'clock, appeared to show that it was probably produced by the sea.

2. I was surprised to see that this foot was stationary at the place of its formation, whereas those which I had before seen were carried swiftly along by the wind. For though it was not impossible but that this foot might be carried by a motion along the line of sight, and consequently not perceptible by me; it was at least certain that it gained nothing from east to west, that is to say, from my left to my right, the direction in which the sea, the clouds, and the other water-spouts which had travelled so far in so short a space of time, were carried.

3. I was astonished, that the body of the water-spout being wanting, which, according to my notions, might increase the intensity of the power by which this appearance is produced, it was nevertheless possible that this envelope should be capable of remaining upright and stationary. In this uncertainty I suspended my reflexions to observe the result. I remarked a kind of teat or protuberance, b, fig. 3, projecting obliquely from the lower part of the clouds which arrived from the east. The foot continued motionless, and the protuberance preserved its oblique direction, till the moment when by the action of the wind it arrived at the foot; at which instant we all three observed the protuberance directed itself perpendicularly towards the foot, and like an immense sack of gauze unroll itself

from the extremity *c*, fig. 3; when the folds of this sack disappeared, and the body of the water-spout, which was grey and transparent, fixed itself in the bottom of the foot, assumed the vertical position, and became larger in diameter. My second son, who, as well as his elder brother, possesses a very clear sight, immediately exclaimed, "See, father, how rapidly the vapours fly up through the bag." I saw, in fact, that they seemed to expand it with a kind of tension, at the same time giving it a deep indigo colour, which was communicated to the cloud. At the same instant the colour of the whole water-spout became so deep that we could distinguish no motion in its expanded part. We observed only that the whole phenomenon moved from east to west, and was destroyed on the coast of Provence. Lastly, a fourth was formed, which was destroyed in the same manner, without any such reproduction, beyond the hills of Antibes, as I observed in 1780, because, their course being more oblique towards the north, they could not meet the gulph Jean, and the prolongation of their track was altogether over land. A fall of snow succeeded immediately afterwards, which was of the usual density and configuration. It lasted all the rest of the afternoon and the following night, so that on the following day there was as much snow on the ground as before. It afterwards rained for a long time, which cleared the country of the snow that had accumulated. As the impetuous wind of the preceding day continued with undiminished force through the whole night, and the other accessory circumstances were likewise present, I think there is reason to conclude that new water-spouts must have been formed in the afternoon, and perhaps in the night of the 6th; but the obscurity of the atmosphere, from the fall of snow, did not permit me to observe them. I shall therefore proceed to make some remarks on the wind which caused this phenomenon.

Though the velocity of this wind was nearly equal to that of the greatest storms in our seas, the waves were not proportionally deep. Two circumstances appeared to concur in producing this effect; the first, that by the form of our coast an east wind cannot have passed over so great an extent of sea as a wind from the south-west, from which quarter our greatest storms come. This cause is constant with regard to our local situation. The other circumstance was, that the wind did not blow obliquely downwards, but moved parallel to the surface of the sea. This supposition, which is the only one that requires proof, was confirmed at the time of observation by the appearance of a small Catalan vessel, which the wind of the 6th of January blew ashore near Nice. I saw her pass before my windows, driven by a force she was incapable of resisting. She did not labour much, but came to an anchor at a little distance, from which, however, the violence of the wind drove her on shore, though without considerable damage, since she was got off a few days after, and pursued her voyage. The force of this wind was seen not only in the instance of this vessel, but in a considerable number of others which were lost on the neighbouring shores.

It is an observation very agreeable to the opinion of Professor Toaldo, and at the same time well established by every observation I have made since I directed my attention to the phenomena of water-spouts, from the beginning of 1789 to the 19th of March in the same year, on which day we observed those which I shall presently describe: it is a confirmed observation, I say, that the phases of the moon are accompanied with a change of weather.

It is certain that the cold weather which suddenly came on with the new moon of the 27th of December, recurred again exactly at each new phasis. I cannot be deceived in this respect, because the chimney of my apartment smoked each time, and did not smoke but during this accidental cold. Two days afterwards, when the weather became milder, this chimney acted as usual, and did not smoke again until the following change. I could urge many other circumstances in favour of the opinion of Mr. Toaldo; but I forbear, because they are foreign to my present subject.

Lastly, on the 19th of March the wind, which had begun the preceding evening, blew with a degree of impetuosity less than that of the 6th of January. The clouds were accumulated from the east towards the west, but they were much less condensed than at that time. At forty minutes after eleven in the morning, we observed two water-spouts, a, b, Plate XXV. fig. 4, which moved at the same time, the one after the other. The most remarkable circumstances in these water-spouts were: 1. The prodigious enlargement of the protuberance d, fig. 4, from the extremity of which hung the effective spout b, which was incomparably thinner; but the wonder disappears, when we reflect that the following spout, which maintained itself in the same state as those we had before observed, robbed the preceding one in some measure of its support; so that this enlargement was, as it were, a commencement of dissolution, and the thinness of b was a proof of the little intensity of electric power then acting; a conclusion which is also confirmed by the following circumstances. 2. The incapacity in the feet of these two water-spouts to elevate their surrounding plumes. It is seen, a b, fig. 4, that they were reversed by a force which prevented their rising in a perpendicular direction, like those of the preceding water-spouts. At the extremities of the plumes here described, as well as at the centre of the circle near the surface of the sea, there was formed a small atmosphere: but as it was not extensive, the vapours were so few that we had very little snow, which continued for about half an hour, when the weather cleared up. During the transition of these water-spouts very distant thunder was heard five or six times.

In the interval between the observations of the 6th of January and the 19th of March, other water-spouts must have been formed on the coasts of Provence. It is certain, at least, that I saw the appendices projecting from the clouds, and that the product of frozen snow reached our first hills on this side of the Var; but as my prospect, being limited by the mountains of Provence, did not allow me to see these water-spouts in such a manner as to make any drawing or description, I shall here conclude my observations, and attend to the results.

I don't know whether I am misled by partiality for my own observations, when I express my opinion that the facts noted by me on the 12th of April 1780 are of great value in natural philosophy, as well because of the vicinity of the water-spout which appeared that day, as of the transparency of the surrounding vapours of the foot, which exhibited the interior ebullition with scarcely any obscurity. Whence it follows,

1. That there is a real ebullition in the sea, at the place circumscribed by the foot of the water-spout.
2. That the vapours of the water which must arise, are the product of an evaporation which

which must separate fresh water from the salt; it being ascertained by experiment, that distillation is the only method by which sea-water can be rendered completely fresh.

I shall take the liberty in this place to make use of a comparison, by way of explaining more perfectly the phenomenon we at that time beheld. It will not probably be unacceptable to such as have never seen a water-spout, at least in so favourable a position. Suppose the chimney of a baker's oven, such as we have at Nice, in which the fuel commonly burned consists of branches of pine recently broken from the tree, and sufficient to bake large quantities of bread; let this chimney be imagined to be in the state of throwing out immense clouds of vapour and smoke. Suppose a funnel of glass to be adapted to the aperture at the lower part, and to have its diameter enlarged upwards, until it terminates in a very extended vessel. It may easily be conceived that the ascending vapours will be so pressed together in the narrow neck of the glass, as to render it opaque, without permitting the successive undulations of the vapour to be discerned. But in proportion as they proceed further from the cause of motion, which is the fire of the oven, and arrive at a part of the glass where they have room to expand, the vapours, being less condensed, will exhibit their peculiar motions, and the successive eruptions of the smoke will indubitably be seen. In this imaginary experiment, let the foot be supposed to be abstracted; let the ascending vapour be simply that of boiling water, and the conical tube of glass here contemplated, will afford a natural representation of the phenomenon observed in 1780, and confirmed on the 6th of January 1789.

It may perhaps be objected, that these facts do not at all agree with Professor Muschenbroek's theory of water-spouts, given in his *Essai de Physique*. This objection was started by all my philosophical acquaintance against the report of our observation in 1780, and engaged me to suspend the publication of my memoir for several years, because it was not possible to reconcile the means employed by nature in the production of water-spouts, nor their use, with the ideas of the celebrated Dutch Professor. Besides which, having only one observation to offer, though well supported by the testimony of two respectable persons, I flattered myself that I might remove all the difficulties by representing a water-spout in the same manner as lightning and thunder are imitated by the electrical machine. For it seemed to me at that time, and still appears to be, a thing very practicable. But at present, by a new series of observations in confirmation of that of 1780, I see clearly that the processes of nature are very different from those pointed out by Professor Muschenbroek. That enlightened and most accurate observer had no opportunity of examining this phenomenon in a favourable position himself, and has been equally unfortunate in the explanation of the supposed descent of water, which really ascends in water-spouts, as well as in the formation of the foot, which according to his theory is a mass of sea-water in its natural state. I can affirm without fear of contradiction by experience, that this foot or atmosphere as it may be called is nothing but the matter of clouds and mists.

It must moreover be remarked, that in the time of Muschenbroek the theory of electricity had made so little progress, that he did not avail himself of it in the account of fiery meteors. It is therefore less wonderful that he should not have resorted to it in his theory of water-spouts.

What then is the agent, it may be asked, which causes this ebullition in the sea, and raises

raises the vapours through the water-spout to the cloud? Simply to affirm that this agent is electricity, without further proof, is in fact to say nothing. To this question I must answer, that I have exhibited the products of observation: I have related what I have most clearly seen. I think I can discern the cause without being able to exhibit proof: but I shall be happy to be anticipated by philosophers of greater skill in this theory, and think it better to suspend my judgment than yield to the seductive pleasure of explaining every thing by adding to the mass of error in natural philosophy.

The second fact which presents itself in our observations is, that two causes unite in the formation of water-spouts, or rather two different modifications of the same cause. When the foot appears without the water-spout, it is not the productive cause, but rather an effervescence which prevails in the sea at that place. But how many interesting questions might be asked respecting this part of the phenomenon! What cause is it so powerful as to retain the foot a, fig. 3, and keep it motionless, notwithstanding an impetuous easterly wind, until the projection in the cloud which is to form the water-spout shall arrive directly over it? Was the apparent bag which developed itself from the cloud, pre-existent in the projecting part? As I can make no satisfactory reply to these and other questions which might be proposed, I shall proceed to the third remarkable fact.

3. When the foot of a water-spout begins to approach the earth, its diameter contracts, its height is diminished, and its volume becomes less and less; so that the foot is reduced to nothing at the instant it touches the shore. From the attentive examination I have made, it has appeared that the foot even of the greatest water-spouts began to diminish when the depth of the sea beneath became less than the elevation of the foot itself above the surface. If this be true, as I think it is, it may be concluded that the effervescence which supplies the spout with water, and forms the surrounding vapours of the foot, extends itself in depth nearly as much as the foot itself rises above the sea, and that materials for the supply of vapour become defective in quantity in proportion to the shallowness of the water.

Explanation of the Drawing.

FIG. 1. Plate XXIV. represents the imperfect foot a of the water-spout seen on the 6th of January 1789, at five minutes after ten in the morning. On the left hand are seen the clouds which rise towards the zenith, but still considerably distant. This foot had plumes elevated nearly like sails, and was driven towards the shore by the wind. In proportion as it came near the land it contracted, and was reduced into a column of mist, which the wind overfet on the land the moment the supply of water was wanting.

Fig. 2 represents, letter a, the enormous water-spout observed on the same day at eight minutes before noon. Nothing could more nearly resemble a ship of war on fire than this phenomenon, excepting that no flames appeared. I have endeavoured to shew the continual jets of its surrounding vapour, and of the water which issued from the centre. At b are seen the remains of a water-spout after it has been destroyed by the foot having touched the ground.

Fig. 3. Plate XXV. a represents the foot of the second water-spout ready formed, which was probably the third. It has yet no spout. At b is seen a protuberance tending obliquely towards the east, and advancing to the west with the cloud to which it is suspended. At
the

the letter c it may be observed how the protuberance b having arrived over the foot a became vertical, and unrolled itself instantly in a kind of large bag, of the figure of an inverted cone, nearly transparent, like gauze. As soon as this bag composed of the matter of the cloud had developed itself, which occupied the time of three or four seconds, and had fixed its small extremity in the bottom of the foot d, it became straight, without folds. A vapour like that seen in 1780 immediately rose up the tube, extended it in the form of a water-spout, deprived it of its transparency, gave it a deep indigo-colour, like that of the clouds, and at the same instant the foot and the spout were moved from east to west, and followed the course which the impulse of the wind gave to the clouds to which the spout was attached. This spout being destroyed, the following spout exhibited the same phenomena as the others. It must be remarked, that the distance of the protuberance b from the foot already formed at a could not be augmented in the figure; but this protuberance, when first observed, was at more than a league distant from a, which remained motionless till its arrival. It is probable that it may have arrived from a still greater distance. It is also to be noted, that the spouts 2 and 3 were somewhat less in all their dimensions than the first.

Fig. 4. exhibits two water-spouts, which were seen following each other on the 19th of March. The wind was less strong than on the 6th of January, the sea less agitated, the clouds less accumulated, and less deep in colour. The intensity of the phenomenon was also proportionally less. It may be seen at a and b that the surrounding plumes of the foot had not the power to raise themselves up, as in the preceding figures, but were kept down by the wind. The enlargement of the upper part of the spout d appeared to be a commencement of dissolution, such as was observed at b fig. 2. These two water-spouts proceeded to the point of N. Dame de la Garde, beyond Antibes. Those of the 6th of January reached the shore between the town of Antibes and the mouth of the Var. The line they described from the time we first saw them to that of reaching the shore may be estimated at five or six common leagues.

Advertisement respecting the Figures.

THE magnitude of fig. 1. in these drawings must not be considered as proportional to those of the figures 2, 3, 4. The first water-spout passed at no greater distance than a musket-shot from our windows; the others were at the distance of two or three leagues. It is necessary to make allowance for what the first gained by its nearness, and the others lost by their remote situation, in order to form a judgment of the respective size of each supposed to be at the same distance.

II.

Experiments and Observations on the Inflexion, Reflexion, and Colours of Light.

By HENRY BROUGHAM, Jun. Esq.

[Concluded from page 563.]

HAVING endeavoured to unfold the property of flexibility, as varied in inflexion, deflexion and reflexion; and also the physical cause of this property; and having indulged in a speculation depending on this cause, I flatter myself neither altogether useless nor unimportant;

portant; I hasten now to the natural phenomena, the explanation of which depends on the property whose existence and nature we have just now been investigating; and, that we may treat this part of the subject with conciseness and order, we shall rank the phenomena under a division similar to that under which we laid down the principles, beginning with those appearances which are explicable on the principles of flexion.

1. It is observable, that when a body is exposed in the sun's light so as to cast a shadow, and another body is approached to it, either between the sun and it, or the shadow and it, or in the same line with it, the shadow of the one body comes out a considerable way, and meets that of the other. Now it is evident, that when the bodies are held at a sufficient distance from one another, a penumbra is formed round the shadow of each, making it less than it should be were there no inflexion; but when the bodies are brought so close to one another that the edge of the one is within the sphere of the other's inflexion, the light being already bent by this last, the former can have none to bend, and consequently no penumbra in the part of the shadow corresponding to that part of the body which is within the other's sphere of inflexion; and the rest of the shadow having a penumbra, this part that has none will be larger than it, and increase as the bodies approach, till at last it meets the other shadow: the like appearance happening when the shadows are thrown on the eye. Mr. Melvill has endeavoured to shew that it belongs simply to a case of vision*. However, we have now seen that it has no reference to the structure or position of the eye; but only to the common nature of all shadows†.

2. If we shut out all the light coming into a room from external objects, except what may pass through a small hole of $\frac{1}{4}$ or $\frac{1}{8}$ th of an inch in diameter, the images of the external objects, as clouds, houses, trees, will be painted on the opposite wall by the rays of light crossing at the hole; but if a piece of rough glass or of very fine paper be held so as to cover it all over, the light does not pass through; then if the paper be wetted with oil, or the glass with water, so as to give either a small degree of transparency, the first rays that come through are those from red and orange objects, and last from blue and violet. Now it is evident that transparency in general, and this particular fact, are explicable by what was before laid down. It was found by Newton, that a body transmits the light incident on it more or less according to the continuity of its particles; and that a strong reflexion takes place on the confines of a vacuum‡: How does this happen? The initial velocity of light is sufficient to carry it through the first surface or set of particles; but it is so much diminished that it is reflected by the repulsive power of the back side of these particles, unless there be others behind at a certain distance, namely, that at which inflexion or attraction acts, that is, apparent contact: this attraction renews the impetus of light, and transmits it to another set, and so on. Now this action being strongest on the largest and red particles, and weakest on the blue and violet, if the continuity be diminished, the former will be transmitted, and not the latter; which is conformable to the experiment just now mentioned.

3. The doctrine of flexibility furnishes an easy and satisfactory explanation of the different colours which are assumed by flame. Whether we suppose the light to come from the burning body or the oxygenous gas, the largest or red particles have the strongest attrac-

* Edinburgh Literary Essays, Vol. II.

† See, however, Philos. Journ. I. 431.

‡ Optics, Book II, Part III. Prop. 3.

tion for bodies, the violet the weakest ; when therefore the gas and the body combine, the precipitation of light must be in the reverse order of the affinity between the particles of light and those of the bodies. If then the combination take place slowly, the violet and blue particles will be first emitted, and last of all the red : and this is consistent with fact ; for any inflammable body whatever, on being lighted, burns at first with a blue or violet flame, and afterwards has its flame of two or three distinct colours, blue, white, red, &c. as is seen remarkably in the case of a candle. Nay, I have observed in the flame of a blow-pipe, all the seven primary colours at once. When indeed a body is burnt in pure oxygenous gas, the combination is so rapid, that white light alone is precipitated undecomposed ; but in common air, where the azotic gas impedes the combustion, the above phenomena are obvious.

4. A curious phenomenon has often surprised philosophers, namely, blue shadows. These I have observed at all times when the paper on which I received them was illuminated by the sky and any other light; and the reason of them I take to be this: that the shadow made by one light is illuminated by the blue rays from the sky ; for I have often observed purple, and even reddish ones, when the sky or clouds happened to be of those colours; and this account of the matter is confirmed by an experiment. Having received the coloured spectrum made by a prism with a large refracting angle, on a sheet of rough white paper, and held above it another sheet ; I stopped all the rays that illuminated the first, except the blue, and violet, and red ; and if I held a body between the blue and the second paper, its shadow was red ; and if I held a body between the red and the paper, its shadow was blue ; and so of other colours. This I take to amount to a demonstration of the thing *.

5. Passing over other phenomena of less note, I come now to one that has divided opticians more than any other ; I mean the coloured fringes that surround the shadows of bodies. I made several observations on these, which enable me to conclude that each fringe is an image of the luminous body ; for, holding between my eye and a candle two knife-blades, as I approached the one to the other, the edge of the candle seemed multiplied, and soon became coloured, coming wholly away from the candle ; and, as the knives approached still nearer, became distinct dilated images, highly tinged with the prismatic colours ; and just before the knives met, the candle, whose edges had been all along coloured with red and yellow, became much distended, till at last it was divided in the middle, one half seeming to be drawn away by each knife, and then it wholly disappeared. I have observed three kinds of these images, two without, and one within the shadow : the first had its colours in the order from the shadow, red outermost, and violet innermost ; the second and third had the colours in the contrary order ; but the second was so very faint that I could never perceive it, unless when let fall on my eye. All this is easily explained by the different flexibility of the rays. In fig. 9, Plate XXIII. let AD be a body by which the rays SDT and S'D'T' pass, and let SD be within AD's sphere of inflexion, and S'D' within its sphere of deflexion ; then SD will be bent into DG : but because of the different inflexibility of its parts, the red will be bent into DR, and the violet into DV, and the intermediate rays will fall between R and V ; the whole forming an image RGV, separated into the seven primary

* Since writing the above, I find the same explanation of the matter given by Mr. Melvill and some of the French academicians, particularly Messrs. Buisson and Biquelien ; also Count Rumford (*Philos. Journal* l. 100) ; but I have thought fit to keep it in, on account of the experiment that occurred to me in illustration of it.

colours; and in like manner by the different deflexibility of the parts whereof S'D' consists, an image without the shadow, as V'G'R', will be formed, similar to VGR, R' being red, and V' violet, all which is both theory and experience; and the same explanation may be extended to the other cases. Now in all these, the bending power stretching to a very small definite distance, and being of different degrees of strength at different distances from the body, several pencils or small beams, passing through different parts of the spheres, will be acted upon by the power in its different states of strength, and each beam will be disposed into an image in the way before described. Of these images I have sometimes observed four, and even, by using great care, the faint lineaments of a fifth. In forming them, the power acts strongest at the smallest distances, and of consequence bends the mean flexible rays that pass near, farther inwards or outwards than those that pass farther off; so that the extreme rays will in the former case be more separated from the mean than in the latter; and the nearer image will always be the largest and most highly coloured; which is consistent with fact. This explains fully the celebrated experiment of Sir Isaac Newton with the knives, and the explanation is confirmed by the experiments which I related above on flexibility, where the bending force acted most strongly on those images formed out of red light, and least strongly on those formed out of violet and blue light. A number of other phenomena are explicable on the same principles, being only particular cases, as it were, of the coloured fringes or images: I shall here mention a few of the most remarkable.

6. When making some of the experiments which I have related in the course of this paper, I observed, that when the sun was surrounded, but not covered, by clear white clouds, the white image on the chart (the hole being $1\frac{1}{2}$ inch in diameter) was surrounded by two rainbows pretty broad and bright; in the colours were red on the outside, and violet next the white of the image. These bows must not be confounded with one which sometimes appears wholly of a dull red and yellow, when the sun or moon shines through a cloud, and which is owing to the direct transmission of the red rays, and reflexion of the others; for not only are the colours different in species, in brightness, and in number, in the phenomena under discussion, but likewise they are formed by the hole in the window, as I knew by altering its shape into an oblong; and the colours now were not disposed in circles, but in broad lines of the same breadth as the bows had been, running along the shadow of the hole's sides, and in the same position of colours as before. It is evident that their cause is the inflection of the light which comes from the clouds by the sides of the hole (for if the sky have no clouds the colours do not appear) which separate the white light into the parts of which it is composed.

7. It is observable, that when we look at any luminous body at a distance greater than one or two feet, its flame appears surrounded by two bows of faint colours, the innermost of them terminating in a white which continues to the flame; and the colours are red outermost, and green and blue innermost: the appearance is most remarkable if we look at a small hole in the window-shut, the room being otherwise dark; and if the eye be pressed upon, and then opened, the colours are more lively than before, as Descartes observed*; from which both he and Newton concluded that the appearance was owing entirely to wrinkles formed on the surface of the eye by the pressure†. But this could neither form.

* De Meteoribus.

† Lect. Opticæ. Sect. III. ad finem.

the bows with the regularity in which they always appear, nor could the colours be in the order above mentioned from the different refrangibility of the rays: it will also be obvious to any one who tries the thing, that the pressure only increases the brightness and breadth of the bows, but does not form them. The true solution of the difficulty seems to be this: The rays which enter the pupil are inflected in their passage through the fibres which extend over the cornea, and which are very minute, but opaque; by these they are decomposed into fringes, having the red outermost and the violet innermost; and the fringes formed by each fibre, being joined together, form the bow. How then does the pressure enlarge and vivify them? The fibres are naturally extended over the surface of a spherical segment: when this surface is compressed into a plane circle, they are condensed into a much less space, and consequently brought nearer to one another; the rays are therefore more inflected and separated than before. If this explanation be true, it will follow that the like bows may be produced by small hairs like fibres placed near one another; and this I found perfectly consistent with fact: the bows are in this case brighter than in the other; and the small hairs on a hat or the hand made them brighter than any other I have tried. A circumstance which I observed in both cases seems to shew clearly the identity of the causes; the white space which reached from the interior bow to the flame, was speckled or mottled in a manner which cannot be easily described; but which any one will perceive upon trying the experiment.

8. The last of these phenomena which I shall mention is the celebrated one observed by Sir Isaac Newton, namely the rings of colours with which the focus of a concave glass mirror is surrounded. Sir Isaac made several most ingenious and accurate experiments to investigate their nature*; and finding their breadth to be in the inverse subduplicate ratio of the mirror's thickness, he concluded that they were of the same nature and original with those of thin plates described by him†. The Duc de Chaulnes pursued these experiments with considerable success: he found that the rings were brighter, the nearer to the perpendicular the rays were incident; and that if, instead of a concave glass mirror, a metal one was used, with a small piece of fine cambric, or reticulated silver wire, stretched before it, the colours were no longer disposed in rings, but in streaks of the same shape with the intervals between the threads: hence he concludes that they are owing to inflexion; that in passing through the first surface they are inflected, and condensed by the second‡. I am not, I own, quite satisfied with this account of the matter: that they are produced by inflexion the Duke's experiments put beyond doubt; but that they should be formed in passing through the first surface, and reflected by the second, is quite inconsistent with the ratio observed by their breadth, this being greater in the thinnest glass, and also with the order of the colours. Besides, all the coloured images, which fall on the back side of the mirror, will be (by what we before found, when speaking of flexibility§) reflected into a white focus; so that, upon the whole, there appears every reason to believe that the rings are formed by the first surface, out of the light which, after reflexion from the second surface, is scattered, and passes on to the chart. It will follow, 1st, that a plane mirror makes them not; for the regularly reflected light, not being thrown to a focus, mixes with the

* Optics, Book ii. Part IV.

† Mémoires de l'Académie pour l'Année 1755.

‡ Book ii. Parts I. and II.

§ Part II. Obs. 6. of this paper.

decompounded scattered light, and dilutes it. 2d, That the nearer to the perpendicular the rays are incident, the more light will be reflected to the focus, and consequently the less will dilute and weaken the rings. 3d, That the thinner the mirror is, or the nearer the two surfaces are, the broader will the rings be. 4th, That the rings farther from the focus will be broader. And lastly, that when homogeneous light is reflected the fringes or images will be larger, and farther from one another in red than in any other primary colour. All which is perfectly consistent with the experiments of Newton and Chaulnes. There is only one difficulty that may be started to this explanation: How happens it that the colours (made by the mirror) are always circular? We answer, It is owing to the manner of polishing the concave mirror, which is laid between a convex and concave plate, and then turned round (with putty, or melted pitch) in the very direction in which the rings are. If it should be asked, Why does the thickness of the mirror influence the breadth of the rings exactly in the inverse subduplicate ratio? we answer, That to a certain distance from the point of incidence (and the rays are never scattered far from it) this is demonstrable to hold as a property of mathematical lines in general.

Having found that the fringes by flexion are images, of the luminous body*, I thought that from this consideration a method of determining the different degrees of flexibility of the different rays might be deduced, similar to that which I had formerly used for determining their degrees of reflexivity†. I therefore made the following experiment:

Obj. 12.‡ Having let into my darkened chamber a strong beam of the sun's light, through a hole $\frac{1}{10}$ th of an inch in diameter, I held a hair at four feet from the hole; and receiving the shadow at two feet from the hair, I drew a line across the middle of the coloured images, and pointed off in each the divisions of the colours, as nearly as I could observe; and repeating the observation several times, and at different distances, I found, by the same way I had formerly done in my experiment on reflexivity, that the axis, or line drawn through the middle of each, was divided inversely according to the intervals of the chords which sound the notes in an octave, *ut, re, mi, sol, la, fa, si, ut*. But as the measures in these experiments were very minute, and the operations of consequence liable to inaccuracy, I thought proper to try the thing by another test.

Obj. 13. The sun shining into the room, as before, I placed at the hole an hollow prism made of fine plate glass and filled with pure water, its refracting angle being 55 degrees. The spectrum was thrown on an horizontal chart eight feet from the window; and at four feet from the prism there was placed in the rays a rough black pin $\frac{1}{10}$ th of an inch in diameter. The shadow in the spectrum was bounded by hyperbolic sides, as before described; and drawing a line, which might be the axis of the shadow, and pass precisely through its middle, I marked on one side six or eight points of the shadow's outline in each set of rays; and this being often repeated at different distances and in different shadows, the position of the axis remaining the same, the curves formed by joining the points were all parallel; which shows that each line of inflexion taken apart has a given ratio to the line of incidence. I afterwards divided the axis according to the musical intervals, and thus found where each colour of the spectrum had terminated, in what colour each part of the shadows had been,

* Page 589, line 11. and † Page 460.

‡ So numbered in the original: N.

... of ... and ...

and

and by what rays formed. Then I joined the parts that I had marked, and obtained a curve, which I took to be, either nearly or accurately, an hyperbola of the fourth order. I next measured the ordinates (the axis of the spectrum and shadow being the axis of the curve) at the confines of each colour; first, the ordinate at the extremity of the rectilinear red, then that at the confine of the red and orange, and so on to that at the extreme rectilinear violet. To each of these ordinates I added the greatest one, or that in the violet; which (in fig. 10) is VV' ; that is, I produced vV to V' , so that vV' is equal to vV ; and through V' I drew $V'R'$ parallel to the axis VR , and produced gG to G' , and rR to R' ; then from V' I fet off $V'g'$ equal to $G'g$, and $V'r'$ equal to $R'r$, and the other ordinates in like manner; and I found, according to the method before described*, that VV' was divided inversely, after the manner of the musical intervals. It is therefore evident that the inflexibilities of the rays are directly as their deflexibilities and reflexibilities, but inversely as their refrangibilities. The same may be proved by measuring and dividing the images made in the inside of the shadows. These I have found to be at equal incidences and distances, equal to the images on the outside, both in breadth, in distance from the edge of the shadow, and in the relation which their divisions bear to one another; wherefore whatever be the ratio of the angle of inflexion to that of incidence, the same is the ratio of the angle of deflexion to that of incidence; so that the angle of deflexion is equal to the angle of inflexion. If farther proof of this proposition be desired, the following experiment and observations, which from the importance of the thing I do not scruple to add, may be sufficient.

Obs. 14. When two knife blades were placed by one another in a beam of light which entered the dark room, so that the one might form and the other distend the images, I made in one of the blades (with a file) a small dent, which, on the chart, cast an elliptic or semicircular outline; then I observed that the images of both blades were disturbed by it, and wound round the edges of the semicircle; and they were all affected in precisely the same manner and degree. So then the first knife deflected the images formed by the second in precisely the same degree that it inflected those images which itself formed, and so of the other knife; otherwise the effect of the dent would have been different upon the two sets of images. We may therefore conclude that the angles, or sines of inflexion and deflexion, bear the same ratio to the angle or sine of incidence, and that they are equal to one another. My next object was to determine this ratio in one of these cases, and consequently in both; and it was very agreeable to find data for the solution of this problem in [Newton's measurement of the images and shadow; since this philosopher's well-known accuracy in such matters, besides the singular ingenuity of the methods he employed, made me more satisfied with these than any experiment I could make on the subject. In fig. 11. CS is the line perpendicular to the chart SU, and passing through the centre of the body, whose half is CD or SE. EB is parallel to CS, and AI a ray incident at D. ADI or EDI is the angle of incidence, EDR that of the red's deflexion, EDV that of the violet's, and EDG that of the intermediates. According to Newton †, CD was $\frac{1}{320}$ th of an inch, DE six inches, SI $\frac{1}{128}$ th of an inch, RV $\frac{1}{128}$ th, and consequently RC $\frac{1}{128}$ th; GS was $\frac{1}{2}$ th; whence the angles IDE, EDV, EDG, and EDR, will be found to be $4', 30''$; $5' 7''$; and

* Page 560.

† Optics, Book III. Obs. 3. and Art. 9, respectively.

9', respectively. Now the natural lines of 4', 30''; 5'; 7' and 9', are as the numbers 1309, 1454, 2035 $\frac{1}{2}$, and 2617, which are as the lines of incidence, deflexion, and inflexion, of the violet, green, and red. Thus the angles of flexion of the extreme and mean rays being given, those of the other rays are found by dividing the difference between 1454 and 2617 in the harmonical ratio; for then the red will be equal to 145 $\frac{1}{2}$, the orange 87 $\frac{3}{4}$, the yellow 155 $\frac{1}{2}$, the green 193 $\frac{1}{2}$, the blue 193 $\frac{1}{2}$, the indigo 129 $\frac{1}{2}$, and the violet 258 $\frac{1}{2}$; and by adding to the number 1454 the violet, and to their sum the indigo, and so on, we get the flexibility of the red from 2617 to 2471 $\frac{5}{8}$, of the orange from 2471 $\frac{5}{8}$ to 2384 $\frac{3}{4}$, of the yellow from 2384 $\frac{3}{4}$ to 2229 $\frac{1}{4}$, of the green from 2229 $\frac{1}{4}$ to 2035 $\frac{1}{2}$, of the blue from 2035 $\frac{1}{2}$ to 1841 $\frac{1}{2}$, of the indigo from 1841 $\frac{1}{2}$ to 1712 $\frac{1}{2}$, and of the violet from 1712 $\frac{1}{2}$ to 1454, the common line of incidence being 1309. It is therefore evident that the flexibility of the red is not to that of the violet as the refrangibility of the violet to that of the red; and a little attention will convince us that we had no reason to expect the analogy should be kept up in this respect; for the refrangibility of the rays depends on the species of the refracting medium, and follows no general rule; whereas our calculation has been made concerning the action of the bending power at a certain distance, greater than that whereat the particles of media act on the rays in refracting them. It was observed in the mathematical propositions prefixed to this paper, that the angle of flexion is less than that of incidence, when in the case of inflexion the angle made by the ray and the body is acute, and when in the case of deflexion that angle is obtuse; and when the ray is perpendicular, or parallel, the angle of incidence vanishes in both cases. It is evident therefore, that in both these situations of things the ratio of 1309 to 2036 being that of a less to a greater, will not enable us to find the angle of flexion, although it serves very well when the ray before inflexion makes an obtuse, and before the deflexion an acute angle. I have therefore mentioned the angle made by the bent ray with the incident, which gives a general formula; for let the angle of incidence be I, and that which the bent ray makes with the incident B, then F being the angle of flexion, we have $F = B \pm I$; so that if $I = 0$, $F = B$, or if the incident makes an obtuse angle with the body in the case of deflexion, and an acute in that of inflexion, then $F = I - B$, and in the remaining case $F = I + B$.

These observations enable us to give a very short summary of optical science. When particles of light pass at a certain distance from any body, a repulsive power drives them off; at a distance a little less this power becomes attractive: at a still less distance it again becomes repulsive; and at the least distance it becomes attractive, as before, always acting in the same direction. These things hold, whatever be the direction of the particles; but if, when produced, it passes through the body, then the nearest repulsive force drives the particles back, and the nearest attractive force either transmits them or turns them out of their course during transmission. Farther, the particles differ in their dispositions to be acted upon by this power in all these varieties of exertion; and those which are most strongly affected by its exertion in one case, are also most strongly affected by that exertion when varied, except in the cases of refraction, of which we before spoke; and these dispositions of the parts are in all the cases in the same harmonical ratio. Lastly, the cause of these different dispositions is the magnitude of the particles being various.

All that remains now to be done on this part of the subject, is to explain one or two phenomena relating to reflexivity.

1. It has been remarked, that if we look at a candle, or other luminous body, with our eyes almost shut, bright streaks seem to dart upwards and downwards from it. Newton* explains this by refraction through the humours adhering to the eyelids; Rohault † and Mr. Young ‡ ascribe them to reflexions; Descartes makes them arise from wrinkles on the eye's surface; De la Hire from refraction through the moisture on the eyelids, as through a concave lens; and Priestley || from inflexion through the lashes. The truth of Sir Isaac's observation is obvious, because the streaks which dart from the top of the luminous body are formed by the under eyelid, or at least by the moisture adhering to the under ciliary process, and those which appear from the bottom of the body by the upper eyelid; which could not be, either if they were formed by reflexion from the processes, or by inflexion through the lashes.

I have however observed another kind of streaks, mottled with broken colours of all kinds, and formed by reflexion from the moisture on the processes. In these the under streak corresponds to the under process, and *vice versa*. They may be formed by any polished body held in the proper position between the pupil and luminous body. The colours are very beautiful when made by the sun, and resemble, in form and irregularity of arrangement, some of the streaks made by large half-polished bodies, as described in Part II. of this paper.

2. The next object of attention is one of the greatest importance to our theory, namely, the formation of images by reflexion. Three things here require explanation: the number of the images, their colours, and their variations in point of size.

Obs. 15. I have uniformly found that no reflecting surface forms them except it be curve, and (its surface) of a structure somewhat fibrous. A plain mirror, nor a concave, nor a convex one, do not make them; unless they are of that structure; and for the same reason quicksilver, when held so as to reflect the light incident upon it, forms them not; but by triturating it, so as to divide it into small particles, and by placing these in the beam of the sun's light, each particle formed an image with the colours in the regular order, and very bright. On holding a cylinder in the rays, and observing the lengths of the images, I found that if the curvature was increased, the images were also increased in size, being more distended and highly coloured. These things immediately suggest the explanation. Each of the small fibres forms an image, which, from the different reflexivity of the rays, is divided into the seven primary colours. But why does not a plain mirror form one of these upon the same principles? In fig. 12. let AE be the curve surface of a very convex mirror, that is of a small fibre, GC a ray reflected by the small surface DC. It will be separated into CI red, and CK violet, by the unequal action of FC on its parts. But if DC is continued to L in a straight line, then LC's sphere of reflexion extending a little way beyond it to KC, the part nearest to C, and not to IC, will drive KC, and also the indigo and part of the blue, nearer to the perpendicular; then IC being within LC's sphere of inflexion, will, together with the orange, yellow, and part of the green, be brought nearer to KC; so that IC and KC will both be brought to an angle equal to that of incidence, and will be reflected in a parallel white beam. If LC is removed a little, or the surface becomes more convex,

* *Leét. Opt. Sect. III. ad finem.*

‡ *Phil. Transf. 1793.*

† *Physica, p. 249. Clark's edit.*

|| *On Vision, vol. ii.*

IC is attracted, and KC repelled, but not so much as to reduce them to parallelism and whiteness; an image being formed narrower, and less coloured, than when LC is moved so far round that KC is attracted, and IC deflected or repelled. If LC is moved round, so that the mirror is concave, then KC is repelled and IC attracted, as before, unless the curvature be considerable; and then KC and IC are both repelled, and an image formed in the *caustic* by reflexion. In *Obs.* 3. we found that certain irregularities in the surface of the reflector caused the images to be in the inverted order of colours. How does this happen? In fig. 13. let f, f_e, e_r, r_i , and i_b , represent the sections of the convex fibres on the surface of the reflector, and let the ray AB be reflected from f , separated into Br red and Bv violet; then if AB was so inclined to f , that Br and Bv fell upon e_r , the side of the fibre next to f , and a little larger than f , it is evident that Bv will be reflected into vV, and Br into rR, and an image VR will be formed, having the violet outermost, and the red innermost, the intermediate colours being in their order from V to R. Lastly, it is evident that the greater the angle of incidence is, the longer will be the image, and the farther separated its colours; for which reason the farther the images are from the shadow, the less dilated and coloured will they be. Nor will they have the same appearance at all distances from the point of incidence. Very near it they will be all in the form of fringes across the streak, the breadth being greater than the length (if I may use the expression); but as we recede from it they will become distended, as before described, the length increasing faster than the breadth, and at one point or distance they will be just as long as broad; all which agrees with experiment. And it is needless to show by particular demonstration, the manner in which one image is divided from another, the reason obviously being the manner in which the fibres on the reflecting surface are arranged and inclined to one another.

3. A number of phenomena involved in that of the images are explicable by what has been said on them. If a piece of metal be scratched, and then exposed in the sunshine, a number of broken colours will be formed by the scratches, as may be seen either by letting them fall on the eye, or by receiving them on a white object. This is evidently owing to the different reflexivity of the rays incident on the scratches, which are so many irregular specula, of great curvature; the images are therefore distorted and broken, just as a candle, &c. appears broken and coloured when viewed through a piece of irregular crystal, such as the bottom of a wine glass. If we look attentively at any object exposed in the light of the sun, provided it be not polished, we shall see its surface mottled with various points of colours from the specular nature of its minute particles. If we look towards the sun with a hat on our head, held down, so that the sun's direct light may not fall on our eyes, but on the hairs of the hat, and be reflected, we shall see a variety of lively colours darting in all directions from those hairs; and we may easily satisfy ourselves that they are not the consequence of flexion, by trying the same thing with unpolished threads; in which case they do not appear, provided the threads be not very small. In the same manner we may account for the colours of spider's webs, of different cloths which change their colours when their position is altered, and of some fossils which appear of different streaks of colours when held in the light; such as the fire marble of Saxony, &c. All these bodies having surfaces of a fibrous structure, each fibre reflects and decomposes the rays.

4. The consideration of the foregoing phenomena inclined me to think that, upon the

principles which have been laid down, the colours of natural bodies may be explained. The celebrated discovery of Newton, that these depend on the thickness of their parts, is degraded by a comparison with his hypothesis of the fits of rays and waves of ether. Delighted and astonished by the former, we gladly turn from the latter; and unwilling to involve in the smoke of unintelligible theory so fair a fabric, founded on strict induction, we wish to find some continuation of experiments and observations, which may relieve us from the necessity of the supposition. My speculations on this subject have by no means been completed, as I have not yet finished the demonstrations and experiments into which it has engaged me to enter; but in order to complete my plan I shall offer a few hints on the subject:—The parts of light are affirmed in Prop. III. Book I. Part I. of the Optics to be different in reflexibility; that is, according to the author's definition, in disposition to be turned back, and not transmitted at the confines of two transparent media. That the demonstration involves a logical error, appears pretty evident. When the rays, by refraction through the base of the prism used in the experiment, are separated into their parts, these become divergent, the violet and red emerging at very different angles, and these were also incident on the base, at different angles from the refraction of the side at which they entered: when therefore the prism is moved round on its axis, as described in the proposition, the base is nearest the violet, from the position of the rays by refraction, and meets it first; so that the violet being reflected as soon as it meets the base, it is reflected before any of the other rays, not from a different disposition to be so, but merely from its different refrangibility. Although then this experiment is a complete proof of the different refrangibility of the rays, it proves nothing else; and indeed an experiment will convince us that the rays all have the same disposition to be reflected, provided the angle of incidence be the same. For I held a prism vertically, and let the spectrum of another prism be reflected by the base of the former, so that the rays had all the same angle of incidence; then turning round the vertical prism on its axis when one sort of rays was transmitted or reflected, all were transmitted or reflected. We cannot therefore apply the different reflexibility of light to the explanation of the colours of bodies, since this property has no existence. But we have shewn that the rays differ in reflexibility, taking the word in the new sense, as explained above. Let us see whether this principle will not solve the important problem. It is evident that the particles of bodies are specular. Now, I take the colours of bodies to depend not on the size, but on the position, of these particles, or at least on only the size in as far as it influences their position; an idea perfectly familiar to mathematicians.

Obs. 16. In making some of the experiments which I related above, on the reflexibility of light, I observed, among the regular images made by most of the pins which I used, one or two all of the same colour, as red, blue, &c.; and when the pin was moved these moved also, becoming of other colours in regular order like the rest; which shows plainly that their being of one colour at first was owing to some fibre in the surface jutting out, or rather to several of these, which stopped the red and all the rest but the blue of several images, or the blue and all the rest but the red. Farther, I produced several regular images by two or three very small pins, and with considerable trouble. I at last contrived to place them in such a position, as that one blue colour of considerable size might be produced, then a red, and so on, by altering the posture of the pins. Now whether the posture or the size be altered it matters not, for the one affects the other. Is it not evident that this experiment,

and the conclusion to which it evidently leads, may be transferred to the colours of natural bodies, as seen by reflexion? For, the parts being specular and spherical, each will form an image of the luminous body; and by the position of the sides of the neighbouring ones, any six of the colours may be stopped while the seventh emerges; and if this happens in one part it will happen in all, since, that the texture and size of the parts is the same throughout has never been called in question. But it will be asked, How are the particles to reflect a mixture of different colours? We answer, That a particle having its side concave and front convex will produce the effect; for the colours will be thus mixed in a proportion determined by the position of the others. How can whiteness and blackness be produced? If the particles be large, then the whole light incident on each will be reflected and separated; and all the images being compounded and mixed together, a confused sensation, or a sensation of white, will be the result. For, the parts being transparent, and the images formed by the convex surface of the second row of particles, these will be larger in proportion to the thickness of the particles or plates through which they have to pass before they meet with obstruction, and consequently will not be stopped by other particles; and in like manner the colour will be red if the particles are a little less, and so on. If the particles be very small, the light will be separated into images also small; with which and with one another the particles interfering, the light by many reflexions and obstructions will be totally lost. How do bodies appear of their proper colours, though no luminous body be shining whose image may be formed by a reflexion? They reflect images of the clouds which reflect the sun's white light; for if we hold between our eye and a hole in the window illuminated by the light of the clouds a reflecting body, as a pin, &c. coloured images are formed of the hole distended, like those of the sun, as I have often found; and the same holds of inflexion. Why does cutting a body to pieces not alter its colours? This only changes the position of masses of particles, not of the particles themselves; but if by bruising them we change their magnitude and position, we change also their colour: thus the leaves of vegetables bruised in a mortar, many paints powdered, &c. Why do many bodies change colours when viewed in different positions? Because they reflect two colours or more of each image to different quarters; and it matters not whether their position with respect to us, or our position with respect to them, be changed. How do bodies become coloured by transmitted light? Because the foregoing reasonings apply also to the flexion of the rays in their passage through the parts of bodies. These observations appear to me to furnish a very simple solution of the problem. I shall endeavour in a future communication to confirm what has been said by other remarks and experiments; for it would be tedious, and perhaps superfluous, to illustrate what has been said by figures and demonstrations*.

Pursuant to these remarks, it will not be difficult to account for the rings of colours of thin plates by reflexion, as we before did those of thick plates by flexion†. Indeed, those formed in the experiment of the two lenses, supposed by Newton to be owing to the plates

* It is obvious that the different refrangibility of the rays will not account for the bright and distinct colour of bodies. If the refracting angle of a prism be continually diminished, till, for example, it is equal to one of a minute, the refraction will produce no sensible colour. Indeed, almost every piece of plane glass has its sides in a small degree inclined to one another, and yet no colours are formed. Much less then will refraction through the infinitely smaller parts of bodies produce separation of the rays.

† Page 589.

of air between them, appear to have a different cause, as may be without much reasoning gathered from the curious experiments of the Abbé Mazeas*, and even from one or two of Sir Isaac's own, in which he supposes some medium more subtle than air to be between the glasses†. But at present I forbear entering into the subject any farther. This paper has already been extended to a greater length than was at first intended. And I hasten to conclude by a short summary of propositions, containing the principal things which have been demonstrated in the course of it.

Prop. I. The angles of inflexion and deflexion are equal at equal incidences.

Prop. II. The sine of inflexion is to that of incidence in a given ratio (which is determined in the paper).

Prop. III. The sun's light consists of parts which differ in degree of inflexibility and deflexibility, those which are most refrangible being least flexible.

Prop. IV. The flexibilities of the rays are inversely as their refrangibilities; and the spectrum by flexion is divided by the harmonical ratio, like the spectrum by refraction.

Prop. V. The angle of reflexion is not equal to that of incidence, except in particular (though common) combinations of circumstances, and in the mean rays of the spectrum.

Prop. VI. The rays which are most refrangible are least reflexible, or make the least angle of reflexion.

Prop. VII. The reflexibilities of the different rays are inversely as their refrangibilities, and the spectrum by reflexion is divided in the harmonical ratio, like that by refraction.

Prop. VIII. The sines of reflexion of the different rays are in given ratios to those of incidence (which are determined in the paper).

Prop. IX. The ratio of the sizes of the different parts of light are found.

Prop. X. The colours of natural bodies are found to depend on the different reflexibilities of the rays, and sometimes on their flexibilities.

Prop. XI. The rays of light are reflected, refracted, inflected, and deflected, by one and the same power variously exerted in different circumstances.

III.

On certain Points of Nomenclature. By a Correspondent.

To Mr. NICHOLSON.

SIR,

IT appears to me, that the new name *tartarin*, proposed to designate the substance commonly called vegetable alkali, is more objectionable than some of the number of names which are in use.

1. Because the word *tartar* is familiarly known to always denote a salt of acid properties, which consists of nearly three parts of tartareous acid, and one of vegetable alkali.

2. The word *tartareous acid*, which is already employed to denote the acid contained in tartar (and for which substance there is no other name), so nearly resembles the word *tartarin* as to make this name an improper one for the alkali of tartar and of vegetables in general.

* Mem. de l'Académie pour l'Année 1738.

† Optics, Book ii. Part I. Obs. x. and xi.

3. But a very small proportion of alkali is obtained from tartar comparatively with that got from other sources; and it seems at least as improper to call it *tartarin* because it is gotten from tartar, as it would be to call it fern alkali, wormwood alkali, beech alkali, &c.

I agree that the name *pot-ash* is very objectionable, and I wish that another could be thought of that is appropriate, especially because this word is commonly understood in commerce and in the arts to denote the vegetable alkali in its ordinary mild but impure state, as used in manufactories; in which state the alkali is only partially saturated with carbonic acid, and which therefore should be called, according to the new nomenclature, *sub-carbonate of pot-ash*; whereas, in the new system, the name *pot-ash* is intended to signify the alkali in its pure or caustic state.

Considering, 1st. That the alkali called *pot-ash* is very generally diffused through the whole vegetable kingdom, which neither of the other species of alkali are—2. That the other fixed alkali, commonly called *fossil alkali*, or *soda*, exists principally in the mineral kingdom—3. That the volatile alkali or ammoniac is obtained almost entirely from the animal kingdom—4. That the denomination *vegetable alkali* is still very generally employed, and has been so for a very long time—5. That the three alkaline salts resemble one another in so many properties as to form a natural genus which may continue to be denominated *alkali*:—I say, from these considerations it seems best to retain the former specific terms *vegetable*, *fossil*, and *volatile alkali*; but abbreviated, as proposed some years ago by Mr. Christie, and accordingly to employ the names *vegalkali*, *feskali*, and *volkali*, till more apposite names be suggested.

Give me leave to remark, that English writers are not uniform in their orthography of several terms of the new system, and I apprehend some of them write improperly; for instance:

Oxygen and oxygene for *oxygen*.
 Hidrogen and hidrogene for *hydrogen*.
 Oxid and oxyde for *oxide*.
 Sulphat and sulfate for *sulphate*.
 Sulphit and fulfite for *sulphite*.
 Muriat, &c. &c. &c. for *muriate*.
 Calorique for *caloric*.

We hear also sometimes erroneous prosody, for instance *cālōric* for *cālōric*.

The French writers uniformly write *gaz*, and the English generally *gas*. In the English, either mode may be employed; but the French probably prefer their orthography *gaz* on account of the word *gas* sounding too much like another word in their language which excites an indelicate idea.

I am, Sir, your sincere friend,

A VERY HUMBLE PHILOGER.

Feb. 17, 1798.

NEW PUBLICATIONS.

Essai sur les Ouvrages Physico-mathématiques de Léonard de Vinci, avec de Fragmens tirés de ses Manuscrits apportés de l'Italie. Lu à la première Classe de l'Institut National des Sciences et Arts. Par J. B. Venturi, Professeur de Physique à Modène, de l'Institut de Bologne, &c. A Paris, chez Duprat, 1797.—Or, *An Essay on the Physico-mathematical Works of Leonardo da Vinci, with Extracts from his Manuscripts brought from Italy.* Read before the First Class of the National Institute of Sciences and Arts. By J. B. Venturi, Professor of Natural Philosophy at Modena, Member of the Institute at Bologna, &c. Quarto, 56 pages, with one plate.

AMONG the treasures of science and art which the French have lately brought from Italy, are thirteen manuscript volumes written by Leonardo da Vinci, whose extraordinary powers as a painter, sculptor, musician, geometer, philosopher, and engineer, are well known. Citizen Venturi, who resided in France during the war in his own country, has obtained the communication of these manuscripts, and has selected such parts as appear deserving of publication, which he purposes to print in three separate and complete treatises, on mechanics, hydraulics, and optics. The fragments in the present work are for the most part distinct and separate from those principal matters, and are enriched with notes by the editor.

Leonardo was born in 1452. He was the natural son of a notary, whose family possessed at this day at Vinci in Tuscany the situation of respectable mediocrity. Nature, which, as C. Venturi observes, is not actuated by considerations of birth, was prodigal of her gifts to this man. His person was beautiful; his disposition animated and lively, and the powers of his mind wonderful. He applied to geometry, music, and painting, and in each of these pursuits he soon excelled his teachers. He was called to Milan to cast an equestrian statue in bronze, which Louis Sforza consecrated to the memory of his father. He offered his services to the Duke in every undertaking relating to military machines, water-works, sculpture, mechanics, and painting, with a defiance to any one who should pretend to excel him; a boast which indeed he had the means of supporting. When France took possession of the Milanese at the end of the 15th century, he passed several years at Florence; not because he was in any respect prejudiced against the French, as is reported; on the contrary, Louis the XIIth gave him a pension, together with certain duties on the Milanese canals, where Leonardo was employed under the French government. While he was at Florence he selected two of the most beautiful ladies of the country, with the intention of drawing their portraits, and presented them to Louis the XIIth. He left Milan, and repaired to Rome in 1513, after Sforza had again entered the Milanese; after which he went to France on the invitation of Francis I. where he died, as it is said, in the arms of that prince.

I shall forbear at present to enter into any detail respecting the contents of this interesting work, in which this great genius, who preceded Chancellor Bacon in the true method of philosophizing near a century, has the peculiar felicity of an editor and commentator, whose valuable annotations bespeak him a complete master of the subjects of science and research he has undertaken to publish and elucidate. On a future occasion I shall present my readers with some extracts of the most curious parts.

Physiology;

Physiology; or, An Attempt to explain the Functions and Laws of the Nervous System; the Contraction of Muscular Fibres, and the constant and involuntary Actions of the Heart, the Stomach and Organs of Respiration, by Means of simple, universal, and unvarying Principles. To which are added, Observations on the intellectual Operations of the Brain; and on the Diversity of Sensations; with Remarks on the Effects of Poisons, and an Explanation of the Experiments of Galvani and others on Animal Electricity. By E. Peart, M. D. &c. 8vo. 327 pages. W. Miller, 1798.

I have just received this work; for which reason I can at present only announce its publication, and say that the author's explanation of the phenomena mentioned in the title appears to be founded upon the system which he offered to the world in his Elementary Principles of Nature published in 1789.

Travels in the Two Sicilies, and some Parts of the Apennines. Translated from the original Italian of the Abbé Lazzaro Spallanzani, Professor Royal of Natural History in the University of Pavia, F. R. S. &c. &c. In Four Volumes 8vo. 1500 pages, with 11 Plates. Robinsons, 1798.

This celebrated philosopher had the good fortune to obtain a clear and distinct view of three out of the four craters which are still burning in the volcanic countries through which he travelled, namely, Etna, Stromboli, and Vulcano, of which he has given designs; but at Vesuvius his wish could not be gratified. The Lipari Islands have been the subject of his assiduous examination, as well with regard to their igneous productions, as the state, character, and occupations of the inhabitants; and the environs of the unfortunate city of Messina afforded much information, from the variety of natural objects they presented. Scylla and Charybdis; the fisheries in the Straits of Messina for the sword-fish, the ravenous shark, and for coral, with various other interesting objects, also engaged his attention during his excursion in the year 1788-89.

To say that these volumes contain a treasure of geological and chemical information, is scarcely necessary to those who are acquainted with the former labours of the Professor. I must therefore more particularly observe, that he has traversed these interesting districts, contemplated and compared the volcanic products with a particular regard to local circumstances, and the inferences to which those circumstances lead; examined the several substances with every precaution which the difficulties of fixing the external characters and obvious properties could suggest; submitted them to the action of fire before the blow-pipe, and in the furnace, with the pyrometer of Wedgwood and pneumatic apparatus; and lastly, that in such specimens as could not from their external characters be referred to known earths already analysed by chemists of reputation, he has had recourse to the methods of humid analysis. Where his experimental enquiries have proved concise, he has frequently incorporated them with the account of his journeys; but where otherwise, he has placed them with such subjects of general discussion as could not form a part of his narrative in the order of time.

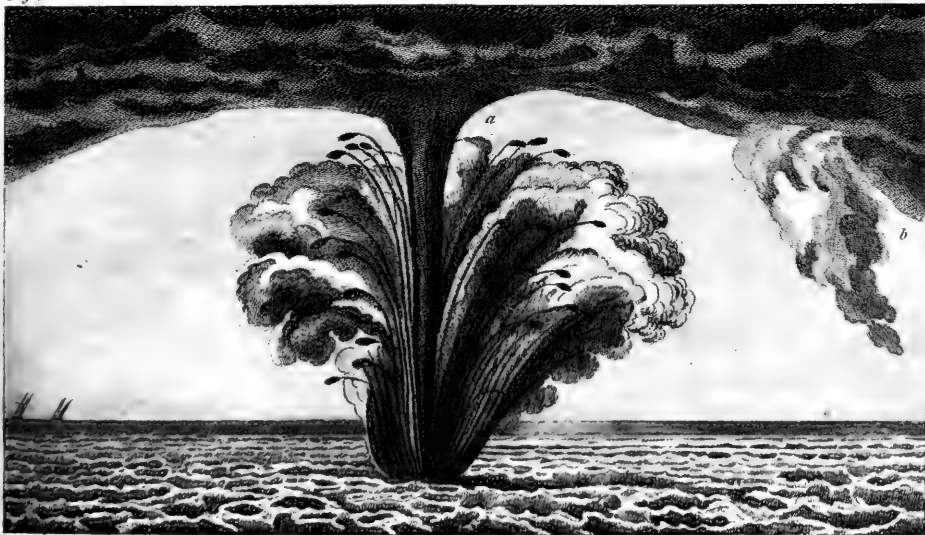
From this rapid sketch of his method of proceeding, the reader, who may be acquainted with the advantages which the modern improvements of Chemistry must afford in the hands of an enlightened observer, will be able to form some estimate of the value of the work.

END OF THE FIRST VOLUME.

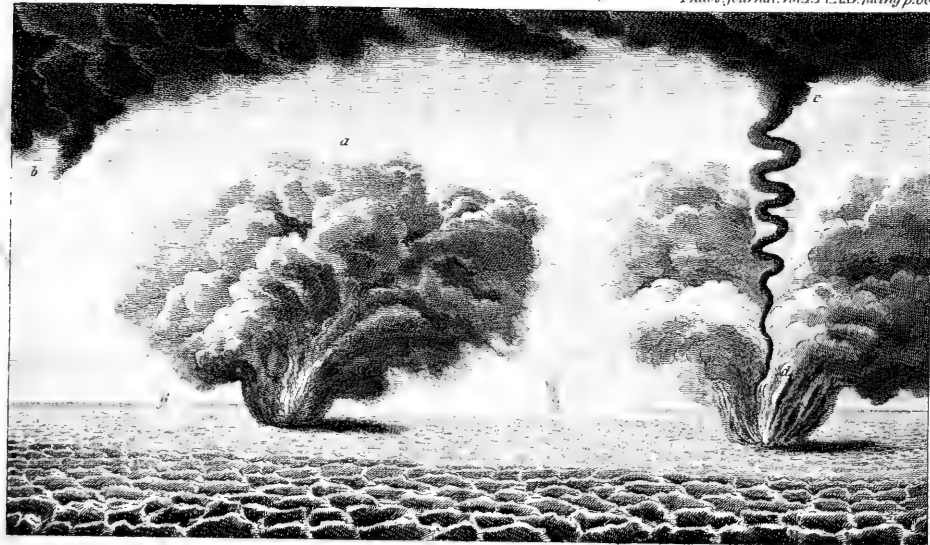
Doneau &c
July. 1791



The Phenomena of Water Spouts.

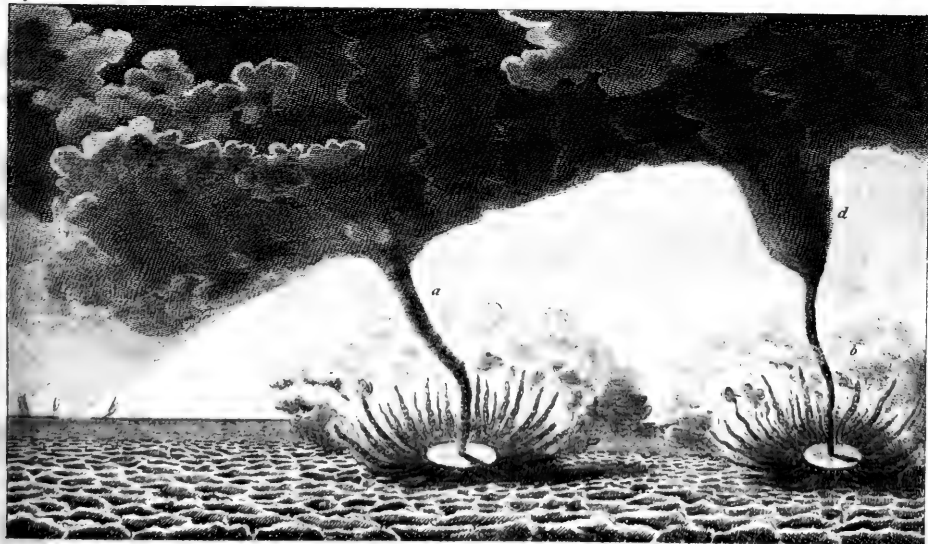






The Phenomena of Water Spouts.

Fig. 4.







$\frac{1}{5}$ fine

